Romans and Batavians. A Regional Study in the Dutch Eastern River Area, II

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ABSTRACT

The further analysis of material from the eastern part of the Dutch river area, presented in a previous article (BROB 31, 7-217), is concerned with several different topics. The introductory chapter 7 provides a link with the earlier publication (chapters 1-6) and continues the discussion of the landscape during the Roman Period, the associated flora and fauna, and the potential for human exploitation.

Chapter 8 is devoted to the typology of the sites. On the basis of excavations and finds in and around the river area, and data from elsewhere, the different kinds of settlements, cemeteries, and other sites are examined. Another topic is, of course, the interpretation of the observed differentiation, especially for the settlements, and in part also the development from the Late-Iron Age until the Early-Middle Ages, a subject that is elaborated in the concluding chapters 10-12.

The intervening chapter 9 is the publication of a small-scale excavation at Arnhem-Meinerswijk, carried out in 1979 for two main reasons: to investigate the nature of this site, which proved to be a Roman frontier fort, and, at the same time, to test the procedure for predicting the type of habitation and its dating on the basis of surface finds. In this case that proved to be a success and, as discussed in paragraph 8.4.3, there are reasonable prospects of doing the same for other types of settlement sites in the future.

Although they are discussed only sporadically before chapters 10-12, Romans and Batavians or, in more general terms, allochthones and autochthones and their interaction in a spatially and chronologically restricted area are the real subject of the entire study.

In chapter 10 the background situation is examined: the development during the Late-Iron Age until 12 BC. The population emerges as a rather egalitarian tribal society. A combination of archaeological, historical, anthropological, and ecological data shows that the socio-economic structure of that society was focused on extensive cattle-breeding.

Caesar's activities only marginally involved the river area but under Octavian the region was gradually brought under control by sending a group of Chatti there who were called Batavians or received that name after their new homeland. In addition to historical evidence there are now also at least some archaeological clues for this event. These point to a strategically important area in the centre of the region.

After a theoretical discussion of imperialism and colonialism and the specific Roman form of these general concepts, the various effects of the incorporation of the river area in the Roman state are further examined in chapter 11. The initially mutually beneficial symbiosis of the frontier tribe and the state is shown to have already changed rather drastically before the mid-1st century AD. Disruption of the socio-economic system combined with strong acculturative pressure can be shown to have been major underlying causes of a native revolt.

Its suppression, in AD 70, was followed by further enforced developments and a major investment program in Flavian times. Only in the 2nd century did this result in archaeologically traceable indications of acculturation; nevertheless, the entire administrative and economic structure is shown to have been maintained by external input, with a local population gradually developing into a soldiers' caste.

It appears (in chapter 12) that these people were capable, however, of keeping at least the nucleus of their lands under control during the events of the late-3rd century. With new immigrants such as the Heruli they may have functioned more or less as in early-Roman times, acting as a buffer in the emerging defence-in-depth system and together providing troops for the Roman army.

A wholesale population replacement can be demonstrated for the first and only time after AD 350, as a result of major Frankish invasions. The river area was then settled by Franks, notably Salii, again fulfilling a similar function with Roman military control definitely still extending up to the Rhine. Perhaps stimulated by the region’s role as a trade corridor between Britain and the Rhineland, a new and viable social formation then developed, with a mixture of Germanic and Roman elements.

After the collapse of the western Roman empire this polity extended its influence further south. This same process, however, combined with the region’s now irrelevant geographical position, caused the river area to become an unimportant periphery again, with a new cycle of developments starting only from the late-6th century onwards.
7 INTRODUCTION

The present article is the second part of a study of the habitation in the eastern part of the Dutch river area (fig. 54) from the Late-Iron Age until the earliest Middle Ages, the Merovingian Period. It thus covers approximately the millennium between 250 BC and AD 750, which follows from its purpose. Very briefly stated, that purpose is to collect and interpret the available evidence on society in the river area during the Roman Period. Obviously, this necessitates inclusion of the immediately preceding and subsequent chronological phases.

An introduction to the general context of the present study in the Eastern River Area (ERA) Project and a discussion of aims and methods has been provided in chapter I. The subsequent five chapters of Part I are mainly restricted to a presentation and interpretation of basic data of various kinds. In chapter 2 historical information is used to define five major chronological phases in the millennium studied. These are the Late-Iron Age (c. 250-12 BC), the Early- (12 BC-AD 47), Middle- (AD 47-c. 270), and Late-Roman (c. AD 270-425) Periods, and the Merovingian Period (c. AD 425-750).

Chapter 3 is devoted to a discussion of the landscape and its reconstruction during the Roman Period. Chapter 4 is concerned with the chronology of the different sites and the basic typology of different kinds of sites as used on Appendices 1-5 (see below, under technical remarks). The information about all individual sites has been assembled in chapter 5, which is a catalogue listing finds, literature, and other relevant information. Finally, in chapter 6, a number of find categories is presented in more detail from varying points of view. These range from a concern with social or economic significance to a limited discussion of chronology or provenance.

In this second part, the information from chapters 1-6 is further analysed. Chapter 7 mainly provides some data which are additions to or corrections of those in Part I on the basis of recent research and some new finds. It also intends to facilitate as much as possible the independent use of Part II.

Chapter 8 is devoted to the typology and interpretation of the sites in the five differentiated chronological phases. One of these sites, the Roman fort at Arnhem-Meinerswijk which was discovered as a result of the present study, is treated separately in chapter 9. The remaining three chapters are an attempt to draw together the information presented and analysed in the foregoing chapters and to integrate it with other relevant data into a coherent picture of developments in the river area between c. 250 BC and c. AD 750. However, equally important as the synthesis of all information into a multi-dimensional but still largely descriptive ‘occupation history’ of the region is the attempt, in addition, to analyse the processes underlying the observed developments or patterns, or at least some of them.

Within limits, archaeological, historical, and geological data and other information can be used to formulate and sometimes test a number of hypotheses about the workings of society in the river area. These can be illuminated by – and sometimes perhaps shed light on – socio-cultural processes at higher levels of analysis. After all, people in the river area were not an autarkic group but subjected to influences of a far more encompassing nature. But this is as far as we can go. The unavoidable and additional self-imposed limitations outlined in chapter I and later have defined the present study as research at the (micro-)regional level: to collect the available information and to explore its potential and shortcomings. It thereby serves as a pilot-study for the ERA project and as
one of the elements for a comprehensive supra-regional approach which could be spatially defined as the basins of Scheldt, Meuse, Rhine, and Ems (see Part I, fig. 4).

Technical remarks
The first part of this study was published in the Berichten ROB 31, 1981, p. 7-217. In order to avoid cumbersome cross-references, that article is not cited but referred to directly by chapter, page, and figure numbers. For this reason, chapters and figures have been numbered consecutively throughout the entire study, which is also published as a separate book. For references to chapters 1-6 and figures 1-53 readers of BROB 34 therefore need to consult BROB 31; references to page numbers

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of chapters 7–12 in BROB 34 always indicate the number between brackets.
To facilitate the use of the present article, the most important information about the modern topography of the river area has been indicated on fig. 55. More details can also be found on Appendices 1–5 of Part I. But these are maps primarily designed to present the distribution of the different types of sites during the five phases against a reconstructed geological background. They are at the same time sheets of the Archaeological Map of the Netherlands 1:100,000 published by the ROB. Figs. 60-64 below are abstracts of these Appendices.

7.1 ADDITIONS TO THE CATALOGUE

After the publication of Part I several new findspots were discovered in the eastern river area and even more new finds were reported from known sites. That was to be expected, and in most cases the new information does not contribute important additions. Therefore, in this paragraph only those new finds or sites will be mentioned which are relevant to some of the issues discussed in the following chapters. They are an addition to chapter 5 and presented in the same way (for abbreviations see chapter 5, p. 95).

199 Lathum-Lathumse Waard
Finds (l) late-Roman bronze hairpin (see fig. 56)
Collection Museum Zevenaar
Other data isolated find

544 Ooij-Stenen Kamer
Situation a.s.s. (not recorded by soil survey)
Finds (a–e) x, (l) glass bottle, (m) x
Collection GAS, private
Literature JROB 1983, 112-3; JAWNN 1984, 9-13
Other data settlement

545 Oosterhout-Verburgtskolk
Situation a.s.s. (not recorded by soil survey)
Finds (b) Br 5, (c) xx, (d) St 109A, St 109B, xx, (h) illegible round stamp probably VEXEXGERINF, (j) xx, (o) Bö B, Bö D 9-12
Collection Historische Kring Oosterhout
Other data settlement

546 Huissen-Zwanewater
Situation Finds in secondary location
Finds (b)xx, (c) xx, (d) Br 36, (e)Nnb89, xx, (h) lX[6], lX[6] and LBG[ on tegula, (j) xxx, (k) x, (m)Bö D 9-12
Collection private
Literature JAWNN 1984, 16-7
Other data isolated find
7.2 THE LANDSCAPE AND ITS POTENTIAL

In chapter 3, the genesis and structure of the landscape in the eastern river area and various related issues were discussed in detail. It is not the purpose of this paragraph to summarize or otherwise repeat what has been said there but to discuss a number of necessary additions or corrections. Notably some additional evidence on the nature and extent of the fluvial system during the periods covered by the ERA project, which has only recently become available, allows improvements to the reconstruction of the landscape presented earlier (fig. 13).

A second major subject is a short overview of the potential of the landscape for the subsistence of its inhabitants. This is intimately related to the subject of chapter 3 but was deleted in the first part of this study because it has a direct bearing on such matters as carrying capacity, subsistence strategy, and economic change which will be discussed here. Data on the actual – as opposed to the potential – use of the landscape provided by archaeological and palynological analysis and the presently available archaeozoological and palaeobotanical information, are included in the following chapters.

7.2.1 Geology and Reconstruction

As was to be expected, new information that became available after the publication of the Eastern River Area sheet of the Archaeological Map of the Netherlands (Appendices 1-5), in 1982, has resulted in a number of corrections in the form of changes or additions, but also in confirmation of arguments. Furthermore, two important studies which were then still in preparation are now available. These are the publication of sheets 39W and 39O of the Geological Map of the Netherlands,\(^1\) and of the geological development of part of the central Dutch river area.\(^2\)

Both studies provide a wealth of information relevant to most or all of the subjects treated in chapter 3 and to the geology on the Appendices; this is based on manuscripts provided by both authors and thus needs no vital corrections. The other additional information or necessary changes concern two subjects. First, there are several issues related to the map which are an addition to paragraph 3.2. Second, there is the new information which necessitates further comments on the reconstruction of the fluvial system during the period investigated.

Additions and corrections

For the area north of the Rhine and IJssel, the discovery of site 542 has demonstrated that the area between Nude and the Rhinedike (c. square 171/440), left blank on the Appendices, should be included in the pre-Roman and Roman channel deposits.\(^3\)

For the area between Rhine and Waal (the Overbetuwe), the presence of site 545 proves that south of Oosterhout (c. square 185/431) these same channel deposits are still uneroded beyond the dike under the present watermeadows. This phenomenon may, however, be of local significance and cannot be bordered or extended to other areas along the Waal. In addition, the discovery provides a context for the finds from the nearby site 155, interpreted as a probable cemetery.

For the area between Waal and Meuse (Land van Maas en Waal), the presence of sites 367 and 534 near Heumen (c. square 186/420) and a reconsideration of the geological evidence\(^4\) have led to a different conclusion than that which is incorporated in the map\(^5\). The Pleistocene fluvial deposits between Nijmegen and Wijchen can indeed largely be considered a flood-basin, but an exception should have been made for a small area from Molenhoek to Heumen indicated by Pons as 'high reddish brown river terrace soils' (Sh 1-3 soil series). These should be counted among the (Pleistocene) fluvial deposits with a

1 Verbraeck 1984.
2 Havinga/Op 't Hof 1983.
3 Cf. chapter 3, 40, note 54.
4 See Pons 1957, 1966.
5 See chapter 3, 32 and 42.
relatively high-lying surface' on Appendices 1-5. The area concerned has been indicated in the new reconstruction of the landscape as presented in figs. 58 and 59. In the area south of the Rhine and Waal (Ooijpolder and Duffelt), two important new finds have confirmed the somewhat uncertain interpretation of the geological situation. The location of the clearly eroded settlement near Kekerdom (site 533, square 197/431) shows that the deposits there are indeed post-Roman. Even more important is the discovery of site 544 near Ooij (square 192/458). This undisturbed site was found as a result of clay extraction from deep pits. The site consists of two stratigraphically differentiated settlements, one from the middle-Neolithic Michelsberg culture and one Roman. The Neolithic site shows that, in contrast to the earlier interpretation, the stream-ridge is originally very old and was probably partly rejuvenated during the Bronze or Iron Ages when bank deposits covered the Neolithic site. On those same deposits, the Roman settlement was established and, in fairly recent times, covered by bank deposits from the present Waal. Further to the south, there are also flood-basin deposits. The actual situation is thus more complicated than expected, but the new discovery demonstrates that the geological situation during the Roman Period must have been as indicated. These findings also show that a survey, such as proposed for this area, may indeed produce significant results. Another addition to our information about the same region is provided by the results of new geobotanical research in the Kermisdahl near Kleve. The stream-ridge from Kleve to Millingen is very deep and stratigraphically differentiated observations have shown that Roman levels are only 1 m below the surface. The deep stream-ridge must have been largely formed by redeposited material eroded by the Kermisdahl meander-curve from the Pleistocene soils south of Kleve in pre-Roman times. The geological situation as indicated on Appendices 1-5 is thus largely correct, with one important exception, namely, the peat-filled channel there. It was indicated as such because it was assumed, on the basis of the possible presence of a Roman road on the inside of the meander-curve, that the channel must have been fossil in Roman times. This, however, is not true. A C14 dating of clay-gyttja (1810 ± 80 BP; GrN 12603) is a first indication for this, but even the date obtained is too old. Palynological analysis of the channel fill has conclusively demonstrated that the channel only started to fill up in the Late-Middle Ages, presumably in the 12th century AD. These findings are, by the way, in agreement with the fact that the 9th-century border between the dioceses of Utrecht and Köln is the same channel which was then still a river-course.

An important second conclusion from the now available information must be that the road, designated by Gorissen as Roman, cannot have been the limes road because it was located on the eastern bank of the Rhine during the Roman Period. This does not entirely exclude the possibility that the road, known as Steypad (Steenpada) in AD 1394, is Roman, but it is more probable that the undated (!) gravel road was in fact constructed much later as a connection along the successor to the Roman Rhine, the so-called Alter Rhein. This interpretation also removes the difficulty that the limes road would have passed by Qualburg (site 460-461) at a distance of 2 km. Moreover, the site can now be situated on the bank of the Rhine, which is also more in accordance with expectations because Quadriruburgium, with which it can probably be identified, is reported to have been refortified in AD 359 to protect transports of British grain up the Rhine.

Another Roman road should also be mentioned again here, namely, that from Cuijk to Nijmegen. As discussed, the road presumably crossed the Meuse at Cuijk, but no reference was made to the positive evi-

6 Chapter 3, 51.
7 The site has also yielded early- and late-medieval finds.
8 Chapter 3, 75.
9 Cf. chapter 3, 44, and note 85. New information provided by Dr D. Teunissen, Nijmegen.
10 Gorissen 1952, 49 and fig. 37.
11 It is another example of the unreliable results of gyttja-datings mentioned earlier (chapter 3, 48-9, and notes 107 and 109). See also below, 207-8.
12 Research included in a doctoral paper by Mrs J. van der Sman, University of Nijmegen, Dept. for Biogeology.
13 Gorissen 1935, 4. The medieval boat with querns from the Rindern-Spyck harbour (site 452), connected to the same channel, is another indication.
14 Mentioned in chapter 3, 44 and 64.
15 E.g., a secondary road from the Rindern fort across the river to some specific important site beyond the limes. That such sites did exist is shown by the example of the Tegularia Transrhena
16 On Qualburg, a late-Roman fort and quite possibly a sta-
17 Ammianus Marcellinus XVIII 2, 3-6.
18 Chapter 3, 64.
idence for this in the form of wooden posts in the present-day river which may have belonged to a (late-)Roman bridge. A radiocarbon date of 1715 ± 35 BP (GrN-6006) confirms this interpretation.

A last addition to chapter 3 which should be mentioned is the presence of Roman (and later) settlement sites near Ravenstein and Neerloon, in the Maaskant area south of the Meuse (squares 172/422 and 174/422). These illustrate what was already indicated in the discussion of the maps, namely, that the inadequate depth of the borings for the soil maps which had to be used as a basis for the map here prevents a satisfactory reconstruction of the still uneroded (pre-)Roman deposits. Most of the area south of Ravenstein, left blank on the maps, should evidently be included among the 'pre-Roman and Roman channel deposits', but a reconstruction cannot be attempted until a geological survey is carried out here.

The Roman Period fluvial system.

The major Roman river-courses as reconstructed in chapter 3 do not need to be reconsidered here. The system of functioning rivers and their courses is sufficiently known in outline, although ongoing research, such as described above, has added new detail to the general picture and will do so in the future. Apart from the major rivers there is, however, the question of the fluvial system in general, encompassing various other streams, and especially the degenerating channels of pre-Roman rivers or the new courses reaching maturity only in later times.

In chapter 3, attention was paid to the fact that more channels must have been to some degree still functioning than could be indicated and that some of the available C14 datings were not reliable. A reconsideration of these data, and notably the implications from palynological research, had to be awaited in order to reach further conclusions. A highly illuminating study by Teunissen now allows some more concrete interpretations.

On the basis of pollen diagrams Teunissen, following De Jong, concludes that C14 dates of especially gyttja-like sediments tend to be too old. Both authors agree that the assimilation of C14-free CO2 from geologically old calcareous rock by the organisms in the gyttja is the most likely explanation. As a result, some channels can now be shown to have remained open much longer than could hitherto be assumed on account of the radiocarbon dates. This is true for nos. 3 (Oude Zeeg), 7 (Oosterhout), 8 (Ressen), 9 (Baal), 10 (Homoet) in fig. 15, for the above-mentioned date from the Kermisdahl deposit, and probably also for a few others.

All this does not mean that during the period studied one should reckon with many more functioning rivers – in the sense of major channels – than assumed previously. On the other hand, several fossil channels which were, except for the channel between Driel and Elst, considered to have been little more than small brooks or drainage gullies and thus deleted from the reconstruction (fig. 13) must now be assumed to have been still open and navigable streams. As was to be expected, the careful analysis by Teunissen has also shown that there are chronological differences between the various channel-fill deposits. These are relevant not only as indications for differentiation between various branches but, in combination with geological data and the behaviour of river systems, also lead to differentiation between different trajectories of the same system.

The degeneration of a river-branch normally starts with the silting-up of its upper reaches, slowly choking the channel, while this same channel remains open much longer downstream. This process is amply documented for the Lower Rhine and also the IJssel, which were only preserved as functioning rivers due to human interference. With reference to Teunissen's more elaborate evaluation it can be concluded that the principal pre-Roman Lower Rhine system developed into such a state of degeneration. The major Roman Period branch was undoubtedly that along which the forts of Herwen-De Bijland (182), Loowaard (194), and Meinerswijk (126) were situated but its geological age is quite young. It can now be demonstrated that its predecessor(s), with branches from Haalderen and from Ressen to Elst and from there to Driel as well as over Homoet towards...
Randwijk, were still open and navigable channels until the Late-Roman Period. Also, as presupposed in chapter 3, channels in the yet older stream-ridge Herveld-Zetten-Randwijk were as a rule presumably still carrying water.

As demonstrated by their characteristic plant communities, most of these channels remained open until the Late-Middle Ages but their upper reaches had silted-up much earlier, between AD 300 and 700. This implies that the process started in approximately the beginning of the Late-Roman Period, and was contemporary with the Duinkerke II marine transgression phase (AD 250-600). It may well have been a sequence of events with entirely natural causes, although this is not necessarily true. As Berendsen has shown, the formation and degeneration of river branches is a highly complex process. At least in the marine and perimarine areas there is a relation with sea-level changes and presumably climatic factors, but this relation is such that the formation of new branches is closely connected to transgression phases while their degeneration and silting-up accompanies regression phases.

This evidence cannot be extended to the eastern river area without additional research, but it does indicate that it is not necessary to accept natural causes unconditionally and look no further. There are at least two good reasons why the upper reaches of the older channels would have been kept open artificially, if that were necessary, thus preventing those branches from degenerating much earlier than from AD 300 onwards. These are the fact that they surely provided useful waterways but also the fact that any additional water entering the Lower Rhine this way must have been most welcome, not only in Drusus' day but also later on. That the organization behind such work could no longer function in the Late-Roman Period or that the work was no longer relevant or necessary (because of climatic change), or all of these circumstances together may then have allowed the start of the process of silting-up.

Of course, there is no decisive evidence to prefer either the 'natural causes' or the 'cessation of human interference' hypothesis, although the latter will need further evidence. In any case it is quite certain that up to the 3rd century the older channels were still a functioning part of the Rhine system and thus provided a direct and navigable route from Nijmegen on the Waal to the Lower Rhine, over Elst to Driel and Randwijk. This fact calls attention to another aspect discussed in chapter 3, namely, the presence of the Drusian mole at the fork of the Waal and the Roman Period (easternmost) branch of the Lower Rhine. It is now necessary to evaluate this choice of location, because in view of the new information the construction could also have been built more to the west, close to Nijmegen. After all, in Drusus' time its purpose was merely to divert more water to the Lower Rhine. The answer must be that its actual position was technically the best location to achieve optimal results. However, in chapter 3 evidence was presented in support of the upper course of the IJssel as the Drusian fosse. If this evidence is accepted, the location of the mole is in fact the only possible one to achieve the desired effect. We know too little about the precise condition and water transport capacity of the different channels to use this as additional evidence for the IJssel as the Drusian fosse, but it is nevertheless worth mentioning. The more so because, in the absence of an IJssel route, the probable early camp in Driel (site 117) becomes an even more acceptable interpretation, but the location of the certain early camp in Meinerswijk (see chapter 9) is then no longer readily understandable.

As regards the IJssel problem, there are two other new publications which should be mentioned here, namely, those by Verkerk and Heidinga concerning its function in post-Roman times. Heidinga quite correctly takes the position that in principle archaeological and historical information should be employed to make deductions about the way in which the river functioned, independent of the geological arguments. In this way, and nota-

29 Berendsen, op. cit., 83-4.
30 See chapter 3, 52-5.
31 Which is, by the way, another argument in support of the proposed early-Roman camp (site 117) in Driel and also of the as yet purely hypothetical fort of Randwijk.
32 There is a strategic advantage of the easternmost branch over the others in the limes concept, and thus from Claudian times onwards, but not in the context of an offensive campaign which only needed a reliable waterway to the north.
33 There is, at the moment, new unpublished material which again provides support for the proposed interpretation. A new boring in the Giesbeekse Broek again provided strong evidence against the 'dry period hiatus' devised by Poelman (cf. chapter 3, 60) and the pollen diagram, with four C.14 dates in the critical trajectory, clearly shows an early-Roman date for the beginning of the IJssel with rapid sedimentation until the 4th century, much less from then until c. AD 1000, and an increase again afterwards. Pers. comm. Dr D. Teunissen.
bly using the — in his view — late-9th century portus of Deventer on the IJssel as evidence, he concludes that the IJssel cannot have provided a suitable waterway during the 7th and the early-8th centuries. The conclusion incorporates a dangerous element of reasoning ex nihilo\(^3^5\), but it is nevertheless possible. As noted by Heidinga, it implies nothing about the condition of the river in earlier periods.

Support for Heidinga’s view is perhaps supplied by Verkerk, on the basis of an older itinerary in the mid-13th century recorded by Albert von Stade, and describing the route from Rome to Denmark. This route explicitly takes a course from Xanten over Arnhem, Utrecht, and Staveren while Deventer or Kampen are not mentioned. If the itinerary was indeed very old when it was recorded, and datable to the 8th century at the latest, it could be further confirmation for a period in which the IJssel was not an important waterway. On the other hand, the indicated route may well represent a positive decision and not a negative one, because there is every reason to expect pilgrims to travel by way of Utrecht if they had a choice!

Neither approach to the problem thus provides reliable evidence, and a source stating explicitly that the IJssel was or was not navigable is lacking. All we have are inferences which suggest that in the earliest Middle Ages the IJssel was not an important route. Although uncertain, this viewpoint is plausible, but it can have no bearing on the discussion of an earlier IJssel route.

That it does indeed not imply anything about the situation more than seven centuries previously is also shown by the evidence for the 17th- and 18th-century situation at the Lower Rhine/IJssel fork. At that time the upper course of the IJssel was virtually silted-up (fig. 57), much to the distress of the Province of Overijssel and the towns of Zutphen, Deventer, and Kampen.\(^3^6\) The situation was remedied by additional waterworks, but it is symptomatic for the troublesome situation there caused by a rather unfavourable angle of bifurcation of the IJssel. Of course this evidence cannot be projected unconditionally into earlier periods, but at least the general information about the fluvial system and the relation between Lower Rhine and Waal is remarkably similar in Roman sources and those of the 15th century and later mentioned by Van de Ven.

Thus the digging of a fosse or the amelioration of an existing insignificant overflow or basin drainage gulley by Drusus’ troops, for which there are various kinds of evidence, should have resulted in a new Rhine branch. Perhaps this branch functioned only briefly, but it may have been of some importance for several centuries, depending on the extra water provided by the waterworks at the bifurcation of Lower Rhine and Waal. Because there is

35 This is illustrated by the fact that Heidinga’s very recent study is already out of date where the early-medieval occupation of Deventer is concerned: see Van Es/Verwers 1985, esp. 28–9, on Merovingian occupation there.

36 See Van de Ven 1976, esp. chapter 11.
evidence that the river was of no real significance in Merovingian times it must be assumed that the branch had silted-up before or was in that process during this period, functioning only intermittently at times of high water. The sedimentation data mentioned above (note 33) support such a view. There may well be contemporaneity, if not common causes, with the degeneration of the other branches discussed previously. In addition, the branch of the Meuse in the Land van Maas en Waal (the so-called Wijchense Maasje) also silted-up in approximately the same period.\(^37\)

With the new information it is now possible to refine the reconstruction of the landscape as presented in fig. 13 and along the lines already set out at the end of paragraph 3.3, and to include some minor corrections. As a general overview fig. 13 remains valid, but the chronological evidence is such that an overall picture of the entire period studied is even more difficult than it already was. Therefore, two new reconstructions are presented in figs. 58 and 59.\(^38\) The first shows the situation from the Late-Iron Age to the end of the Middle-Roman Period (250 BC-AD 270); the second that at the end of the entire period covered by this study, in the last phase of the Merovingian Period (c. AD 700). For the Late-Roman Period the situation is somewhat uncertain, but the currently available data suggest that fig. 58 should be more relevant than fig. 59.\(^39\)

7.2.2 Ecology and Land-Use

In his important treatise on archaeological cartography, Hallewas also devoted a section to the relation between characteristics of the geological landscape and its exploitation potential for specific forms of land-use.\(^40\) His discussion drew attention to a relatively new and for archaeology potentially useful product of the geo-sciences, namely, the so-called land(-use) capability classification (bodemgeschikheidsclassificatie, Bodenschätzung). There are some obvious difficulties in employing present-day soil valuations for archaeological purposes but such data can nevertheless become a useful tool and the method by which they are obtained is also important. It gives insight into the various valuation factors which, sometimes to very different degrees, are relevant to forms of (pre)-historic land-use as well.\(^41\)

In principle, such information should be an important factor in a discussion of the human ecosystem of a region.\(^42\) In practice, however, difficulties arising from lack of information about groundwater-levels, temperatures, and the like only allow a still rather general and undifferentiated valuation of soils. When the suitability of the geological deposits in the river area is estimated with regard to arable farming and stock-breeding, the most important valuation factors are ‘drainage’ and ‘moisture supply and nutritional condition of the soil’, for arable the factors ‘relief’ and ‘crumbling capacity of the soil’ should be added. Based on the data compiled in the soil survey studies,\(^43\) the following generalizations can be made about the deposits indicated in the legend of Appendices 1-5 (on a scale very-moderate-limited-not):

<table>
<thead>
<tr>
<th>Deposit</th>
<th>Suitability for arable</th>
<th>Suitability for pasture</th>
</tr>
</thead>
<tbody>
<tr>
<td>Channel deposits</td>
<td>moderate-not</td>
<td>very-moderate</td>
</tr>
<tr>
<td>Bank deposits</td>
<td>very</td>
<td>very</td>
</tr>
<tr>
<td>Flood-basin deposits</td>
<td>not</td>
<td>moderate</td>
</tr>
<tr>
<td>Brook deposits</td>
<td>limited</td>
<td>moderate</td>
</tr>
<tr>
<td>Low-lying Pleistocene fluvial deposits</td>
<td>limited-not</td>
<td>moderate</td>
</tr>
<tr>
<td>High-lying Pleistocene fluvial deposits</td>
<td>limited</td>
<td>very-moderate</td>
</tr>
<tr>
<td>Low-lying coversands</td>
<td>limited</td>
<td>very</td>
</tr>
<tr>
<td>High-lying coversands and fluvio-periglacial deposits</td>
<td>moderate-limited</td>
<td>very-moderate</td>
</tr>
<tr>
<td>Loess</td>
<td>limited</td>
<td>limited</td>
</tr>
<tr>
<td>Fluvio-glacial deposits and ice-pushed ridge</td>
<td>limited-not</td>
<td>limited</td>
</tr>
</tbody>
</table>

37 Note that chapter 3, 62.
38 Note that the legend symbols 3 and 4 now have a slightly different meaning. This was necessary because most river branches were originally indicated as channel zone deposits while others can now only be drawn as channels or channel deposits. In order to arrive at a balanced picture the only solution proved to be the use of channel deposits, the extent of which is often known or can otherwise frequently be estimated on good grounds. The two alternatives are unacceptable because the precise location of major channels can only rarely be indicated and to indicate the entire channel zones of the largely pre-Roman stream-ridges would severely distort the reconstruction by overemphasizing already less important branches.
39 It is not impossible that future research will show that the change from fig. 58 to 59 can be related to the end of the Duin-kerke II transgression phase or, perhaps better formulated, to the beginning of a new marine regression phase (and its general causes) around AD 600. But this is no more than speculation.
It appears from this table that, as an overall estimate, channel zone deposits are in all respects the best soils while the high ice-pushed ridges and related deposits are virtually good for nothing, with all others in between. It should be realised that the indications given here may not be transferred directly to other areas because they are partly based on the recurrent and frequent combinations of deposits and particular groundwater-levels in this area only. Elsewhere, the same deposits may normally be associated with other levels, which could significantly alter the valuation. At the same time, this warning indicates the limited absolute value for the situation during the period studied: we do not know what the levels were then and, moreover, we can be quite certain that they changed to a smaller or larger degree during that time. Therefore, the valuations are only applicable in a relative sense, to indicate suitability of one type of deposit versus another, and even then they are not without problems.

Also directly related to the question of soils and what humans could do with them is the matter of the natural vegetation associated with different deposits. The situation in the past and developments therein, for the period under discussion mainly those under human influence, can be evaluated by palynological research. The results of such research, as far as available, will be discussed wherever appropriate in the following chapters. But, apart from that, it is also useful to have some idea about the potential natural vegetation in addition to what can be said about the agricultural potential. It is, of course, almost impossible to do this by way of reconstruction, but it can be done by way of the ‘potential natural vegetation’ concept in ecosystem research. This is based on the knowledge of different standard-types of plant communities and their requirements, and proceeds by assuming complete cessation of all human activity and immediate succession, that is, the development of the vegetation to its natural final stage (climax), on the basis of present-day soil, climatic, and other conditions. Obviously there are many problems with such an operationalization of the concept, but it still does produce a general idea of what the potential vegetation should have been like on the various deposits. From the Vegetation Map of the Netherlands scale 1:200,000, which gives an overview of the potential natural vegetation, the following conclusions can be derived for the eastern river area.

The Holocene or fluvial deposits are dominated by the main vegetational series of the Alno-Padion, the alderbird’s cherry association (Elzen-Vogelgersverbond, ooibossen, Auenwälder). A finer distinction is possible for the Holocene deposits, which are dominated by three different associations of a sub-series, that of the oak-ash forest abounding in elm (Ulmion carpinifoliae, iepenrijke eiken-essenbossen). The channel and bank deposits are characterized by the Fraxino-Ulmetum (ash-elm forest, essen-ipebos) or the Anthracco-Fraxinetum (ash forest, fluikruurdijk essenbos), or a mixture of both, primarily depending on the calcium-content of the soil. Forests like this have a very species-rich undergrowth of shrubs and herbs. On the driest parts of these deposits, notably the highest-lying stream-ridges, oaks may have grown in relatively large numbers.

The flood-basin deposits, peat, and brook deposits are typically dominated by a second major sub-series, the Circaeo-Almin (moist alder-ash forests, vochtige elzen-essenbossen), with alder, ash, and willow as the main trees and a dense undergrowth of herbs and grasses.

41 Hallewas 1981, table 1.
42 Cf., for example, the studies by Welinder (for a summary, see Welinder 1984).
43 STIBOKA 1973, 1975, and 1976 which each contain a chapter on the agricultural possibilities and an appendix with valuations of different soil types combined with groundwater-levels. For the terminology on deposits used in the text below, see chapter 3, p. 29-39.
44 Other factors may, of course, also be important. Probably the best example is the loess (or Sandlös) area which is not very suitable for agricultural activities in the region studied for various reasons, including relief. There are, however, a few places with this deposit where the valuation is ‘very’, and in other regions, such as the loess-belt from Belgium over the southern Netherlands into the German Rhineland, the overall valuation of loess is bound to be much higher.
45 See Kalkhoven a.o. 1976. The same issues are also treated (in German) by Tüxen 1956 and Trautmann 1972.
46 These are extensively treated in the literature (cf. note 45) and include the problems of sometimes very drastic human interference in the landscape or the unerasable presence of exotic vegetation.
47 Kalkhoven a.o. 1976, Appendix 1 (two sheets).
48 Vegetational series and syntaxonomical terms after Westhoff/Den Held 1969; for a short overview of characteristics of the different sub-series, see Kalkhoven a.o. 1976, paragraph 2.2.3 and Appendix 3.
Fig. 58 A reconstruction of river-courses during the Late-Iron Age and Roman Period: 1 Pleistocene deposits, 2 flood-basin deposits and peat, 3 bank deposits and pre-Roman channel zone deposits, 4 estimated channel deposits (meander-belts) related to active river branches, major brooks, and some of the small and in part still active fossil channels, 5 present river-channels, 6 boundaries of deposits, 7 reconstructed boundaries of deposits. The reconstruction in this way is much more tentative than the general picture presented in fig. 13. Scale 1:250,000.
Fig. 59 A reconstruction of river-courses during the Merovingian Period: 1–7 see fig. 58. Scale 1:250,000.
On the Pleistocene deposits the Quercion robori-petraeae (oak association) is dominant. These are little stratified and species-poor climax-vegetations. The most general sub-series is the Fago-Quercetum (beech-oak forest, beuken-eikenbos), which occurs on the loess and mineralogically richer coversands and fluvioglacial deposits. In some areas, notably on the ice-pushed ridges, a variety of the same kind of forest, but with less species, may be found, while the mineralogically poorest areas (especially the fluvioglacial deposits) have an oak-birch forest (Querco roboris-Betuletum, eiken-berkenbos). This may, however, not be a true climax-vegetation but the result of human exploitation and poorness of the soil due to the removal of biomass in subrecent times.

On the basis of the foregoing, it is possible to make some general statements about the theoretical potential of the eastern river area for various ways of land-use. This potential has no direct relation to a situation in the past, because that will be determined by additional factors such as previous human interference, the nature of society and its economy, or technological constraints and capabilities. But there is at least the general framework of what is possible and what not, and where there are restrictions or limited options. This can be contrasted with the available ecological and archaeological data in the following chapters.

As an overall conclusion, it can be said that the actual present-day river area is entirely and eminently suitable as a stock-breeding area. All deposits are at least moderately but often even very suitable for pasture, but the more natural vegetation associated in the Alno-Padion, in principle, also quite suitable, especially for cattle. For arable farming, there are fewer options because this cannot be practised in the flood-basins nor even on some more shallow bank and channel deposits. The overall suitability for arable of these, and the brook deposits, is highly variable. Only the high bank deposits are, as a rule, quite well suited for arable.

Other exploitation possibilities should also be mentioned here. One of these is wood which can be used as fuel but some species, notably oak (from the highest parts of the stream-ridges), ash, and elm also for constructional purposes. The same is true for resources such as reed and rush. Another and conceivably quite important contribution are the fish resources which, in a delta area with three major rivers and numerous small streams, are very considerable indeed. In addition, birds, small game such as beaver and especially hare, and large game such as roe deer, elk, and perhaps wild boar and red deer should also be counted among the resources of the river area.

The Pleistocene deposits offer a different picture. Coversands with a low-lying surface and fluvial deposits are often quite suitable for stock-breeding and this can also be done on the higher coversands and loess areas, but a considerable degree of human interference with the natural vegetation of the Quercion robori-petraeae is needed, which should result in a development towards Nardo-Callunetalia (matgrass and heather communities). Stock-breeding on the ice-pushed ridges and fluvioglacial deposits is not very likely but not entirely impossible, especially when it is considered that on the Pleistocene deposits in general sheep may be kept more profitably than in the riverine area. The Quercion robori-petraeae is also eminently suitable for feeding pigs.

Most Pleistocene deposits can to some extent be used for arable, but the better areas are restricted to fluvial deposits and coversands with a relatively high-lying surface, the rest is rather or even very marginal in this respect. Manuring or long periods of fallow are necessary. Apart from being an obvious source for fuel and timber and some small game, the deciduous woods will have housed large game such as red deer en roe deer, elk, wild boar, and perhaps aurochs, but only boars may really have thrived there unless the woods were fairly open as a result of human exploitation.

For the river area as a whole, a few mineral resources should be added to this picture of the resource potential.
Fig. 60 The distribution of late-Iron Age settlements (c. 250–12 BC) in the eastern river area. After Appendix 1, with additions: 1–7 see fig. 58, 8 settlement. Scale 1:250,000.
Fig. 61 The distribution of early-Roman settlements (12 BC–AD 50) in the eastern river area. After Appendix 2, with additions: 1–7 see fig. 58, 8 legionary camp and other (possible) camps, 9 large settlement, 10 settlement. Scale 1:250,000.
The Holocene and Pleistocene clays are the most obvious of these, but not the only ones worth mentioning. There is also the possibility for exploiting bog-iron ore, beds of which occur in the area studied and in very large numbers to its immediate south and northeast. A third resource, exploited increasingly in recent years but also in the past, is the gravel which is available in enormous quantities in the Holocene as well as Pleistocene areas. A final topic to be discussed in the present context are the surface areas of the different deposits with their varying resource potential. Unfortunately, these data cannot be derived directly from the geology on Appendices 1-5, because it is relevant to know the extent of these areas during the period studied. Therefore, the eroded areas have to be considered as well, on the basis of a reconstruction such as presented in figs. 13 or 58-59. The total surface of the study area is 1,650 km², of which 764 km² are Pleistocene and 886 km² Holocene deposits. In Roman times the Pleistocene area must have been larger but this concerns mostly low-lying fluvial deposits later covered by Holocene basin clay and is thus less relevant. Only a small part of the Pleistocene area was eroded in later times, but the surface (mostly coversands) concerned cannot have been large. The total eroded area ("Post-Roman channel deposits and present river courses" on the legend of Appendices 1-5) is 306 km². On the basis of the reconstruction of the Roman Period landscape, it can be concluded that 70% of that area consisted of channel zone deposits, 25% was a flood-basin, and not more than 5% a coversand area. For the entire region, this results in the following estimated total surface areas:

1 Channel zone deposits 281 + 214 = 495 km²
2 Flood-basins 257 + 77 = 334 km²
3 Peat and brook deposits 42 km²
4 Low-lying Pleistocene fluvial deposits 25 km²
5 High-lying Pleistocene fluvial deposits 50 km²
6 Low-lying coversands 45 km²
7 Coversands, loess, and fluvi-o-periglacial deposits 457 + 15 = 472 km²
8 Fluvioglacial and glacial deposits 187 km²

Total 1650 km²

With the exception of most of the actual levees and part of the bank deposits, a very substantial part of the Roman channel zones can be expected to have had a rather low-lying surface and clayey structure. Several parts of the river-system were still relatively young or even (the IJssel) at the very start of their development, and frequent flooding must have restricted the land-use potential. An exposed site such as the fort in Meinerswijk (no. 126) illustrates this because it could only be maintained by artificial raising of the soil, and evidence for repeated flooding is quite clear (cf. chapter 9).
Fig. 62 The distribution of middle-Roman settlements (AD 50-270) in the eastern river area. After Appendix 3, with additions: 1-7 see fig. 58, 8 legionary fortress and (possible) frontier fort, 9 regional centre (civitas capital) and secondary centre (vicus, pagus capital), 10 military vicus and settlement. Scale 1:250,000.
Fig. 63 The distribution of late-Roman settlements (AD 270–
c. 425) in the eastern river area. After Appendix 4, with addi-
tions: 1–7 see fig. 58, 8 fortified (military) settlement and possi-
ble stronghold of unknown status, 9 small fort (burcga), 10 set-
tlement. Scale 1:250,000.
Fig. 64 The distribution of Merovingian settlements (c. AD 425–750) in the eastern river area. After Appendix 5, with additions: 1–7 see fig. 58/59, 8 (possible) large settlement, 9 settlement. Scale 1:250,000.
Although, as mentioned in the previous paragraph, the pre-Roman channel zone deposits had more still functioning channels than was hitherto assumed, these cannot have imposed comparable restrictions. Only the lowest parts of these stream-ridges may have been quite wet during the annual flooding of the flood-basins, but then such flooding, which leaves a layer of fertile mud, has its advantages.  

From the above considerations it may be concluded that of the total channel zone area of nearly 500 km\(^2\) some 60 km\(^2\) consisted of channels sometimes partly and sometimes entirely filled with water. Of the various channel and bank deposits by far the largest part, at least 300 km\(^2\) (namely, an estimated 90\% of the stream-ridges and at least 25\% of the deposits along functioning rivers), may be considered to have been composed of relatively sandy clay with a relatively high-lying surface. The remainder, approximately 140 km\(^2\), were sometimes much more clayey and had a lower-lying surface.

These data are, of course, only relevant for some forms of land-use and hardly or not at all for others. They influence the natural vegetation potential but they do not have a great effect on an activity such as cattle-breeding. They are, on the other hand, very important for settlement location and could be anything from very to hardly relevant for arable farming, depending on the kind of crop, technical capabilities, as well as the size of the population and the nature of its economy.

Indicative for this are even the land-use data from the last century or so. On the first topographical maps (mid-19th century), so much land suitable for arable was indeed used as such that the map gives a quite reliable picture of the geology: stream-ridges emerge when arable is mapped. A similar procedure is entirely impossible with recent topographical maps and this agrees with current land-use figures of 83\% pasture v. 17\% arable,\(^61\) which gives 735 v. 151 km\(^2\), and in fact even less,\(^62\) for the study area. This change may be attributed to economic developments, because better drainage conditions and increased technological means could easily have increased the arable surface. Without going into any details it can thus be illustrated that the land-use potential of the river area is only a framework within which actual land-use may vary considerably. Especially the degree to which the region is tied into larger socio-economic networks and the demands or impulses coming from there determine what actually takes place.

### 7.3 THE DISTRIBUTION OF SETTLEMENTS

An overview of the distribution of settlements during the five differentiated chronological phases is presented in figures 60-64. These figures are in fact abstracts of Appendices 1-5 and designed to give a general overview of the habitation in the eastern river area. Other types of sites such as cemeteries and isolated finds have been deleted as well as most of the differentiations indicating the quality of the evidence for individual settlement sites. The different types of settlements, as far as indicated, are based on the discussion in chapter 8 and are therefore in some cases slightly different from the legend on the Appendices. However, not all settlement types discussed in chapter 8 are indicated by separate symbols.

On the other hand, the number of settlements indicated is larger than on the Appendices. It includes the settlement sites from site no. 530 onwards listed at the end of chapter 5 and in paragraph 7.1, and also sites not included in the catalogue of which descriptions are stored in the CAA.\(^63\) Therefore, the settlements indicated on figures 60-64 represent the total numbers known in December 1984, when the inventory of new sites was terminated.

The geological background is also an abstract from the Appendices, but in this case the simplified and to some degree speculative reconstructions from figures 58 and 59 have been used.

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60 Cf. Pons 1957, 135.  
61 Haans 1979, 104-5.  
62 The built-up areas and water surfaces should be deducted first.  
63 The Central Archaeological Archives. See chapter 1, note 27.
8 TYPOLOGY OF THE SITES

8.1 INTRODUCTION

It is the purpose of this chapter to examine more closely the different sorts of sites referred to in general terms in the preceding chapters. This is, of course, a necessary step in the analysis of archaeological data from the region. It is also a critical step, because the conclusions reached thereby determine to a large extent the outcome of the entire study. The classifications made here are important building blocks for any reconstruction or model of (aspects of) the occupation of the region during each of the periods treated and for any hypothesis on diachronic developments. Also, the degree of differentiation which can be reached is not only informative in itself. It points at the same time to shortcomings in the present study and the available data, and may therefore direct further research. It definitely determines the interpretational limits to which the present evidence may be pushed.

For these reasons, and also because a description of several particular (and some types of) sites should be provided anyway because they are new interpretations, a detailed discussion is presented in this chapter.

For the period between c. 250 BC and c. AD 750, 543 sites have been recorded in the eastern river area. In relation to the legends in Appendices 1–5, the basic assumptions and interpretations leading to a site typology were discussed in paragraph 4.2. These include two fundamental levels of interpretation, namely:

a establishing the contemporaneity of sites, resulting in a division over five periods, and

b establishing the general type of activity, resulting in a division in settlement sites, burial sites, and isolated finds.

1 To this number should be added another 19 sites, located south of the Meuse in the Province of Brabant. These were known during the first compilation of the catalogue (chapter 5) in 1980, but could not be incorporated (see chapter 1, 14). Some are, however, indicated on figs. 60–64. Furthermore, nearly 100 finds from unknown findspots, with uncertain find-circumstances, and the like, have been left out. A number of these must have been found on sites recorded in the catalogue, the association no longer being reliable or even lost. Additions and corrections to chapter 5 are listed in chapter 7, which brings the total at 549 sites.

2 An example is discussed in paragraph 6.5.1, namely, the statement that the occurrence of brick with military stamps (in a primary context) on a settlement site inevitably implies that the site is military. This example indicates at the same time that qualitative measures are not completely deterministic (which is almost necessarily true for all social sciences; see, e.g., Galtung 1967, 321–3) because the fact that the measure is not (completely) effective in the river area does not imply that it should therefore be rejected altogether. This normally has to be done with a completely deterministic statement, where one case that does not confirm is enough to falsify the entire statement (hypothesis).

3 See Claessen 1980.
not be employed casually to designate types of sites. They are not always straightforward and their use may introduce more difficulties than are solved.

All these matters need, at least in principle, a different approach for the successive periods, because it is known beforehand that there will be more to differentiate during the heyday of the Roman occupation than in other periods. What is also known is that, certainly during part of the Roman Period, many of the sites in the river area can only be properly evaluated against the background of a much wider superstructure. This is not only true for military settlements, but also for a settlement type like the villa. As Rivet has shown in his admirable analysis of what exactly constitutes a Roman villa, it is a type of settlement which is only understandable in a very wide context. These are special problems, which need to be incorporated in the discussion of what sorts of settlement can be discerned in the river area.

The regional network itself, however, will be discussed in chapters 10-12, devoted to the necessary integration of the settlement types into regional and supra-regional patterns and systems and their development in the course of time.

The fact that sites are treated here more or less in isolation does not cause any problems because, after all, they are differentiated by mutual comparisons. It does, however, impose restrictions on the sorts of sites that can be used. At the very least, the sites that do not definitely belong to the period under discussion cannot be used at all, which rules out a lot of the 'possible' and 'probable' sites indicated on Appendices 1-5.

8.2 THE LATE-IRON AGE

8.2.1 Settlements

There are 155 settlements which have been dated with certainty to the Late-Iron Age. These are the settlements to be discussed here, but they do not represent the total number of inhabited sites. There are another 87 settlement sites which were probably or possibly occupied during this period, so the maximum number is 242. The true number will probably be closer to the latter figure, because it often turns out that when more material is available from a site, it contains finds datable to the Late-Iron Age. Also, the distribution is very uneven because virtually all of the sites are located in the river area proper. The distribution on the Holocene deposits may give a relatively complete picture, but this is definitely not true for the distribution on most of the Pleistocene deposits. Especially south of the Meuse and also north and east of the Rhine, the evidence is known to be deficient. In view of the fact that not all of the 87 uncertain settlements will actually date to the Late-Iron Age and that, on the other hand, quite a few sites still remain to be discovered and some have been eroded, an estimate of 250-300 for the total number of late-Iron Age settlements would seem to be in the correct order of magnitude for the area studied.

The period in question is rather long, between 200 and 250 years, and except for the 44 or 45 sites which were already inhabited before the Late-Iron Age and continued to be occupied afterwards (one site, 228, yielded early- but no middle-Iron Age material), there is no reason to believe that they all existed during the entire period. This is indeed rather improbable, but unfortunately there are no means by which a finer chronological subdivision can be made. One could assume that the sites with only Iron Age material ended well before the beginning of the Roman Period, but this would have to remain unconfirmed. The same is true for the hypothesis that settlements with the latest late-Iron Age (La Tène D) material, combined with native and imported Roman Period finds, may have started only towards the end of the Late-Iron Age. Although in this case the presence of finds such as the glass bracelets Haevernick type 3a and 3b, which date to the last century BC, can be interpreted as an indication, it does not constitute proof.

With all this in mind, the use of chronology for further differentiation seems to be very restricted. The only hypothesis which can actually be tested is that there may be a difference between the settlements with a long and continuous occupation and those without. There is, of course, other information which may be useful in this respect, as is discussed below. But it is important to realize that, apart from reasons why there could be differences produced from the figures for the 1st century AD discussed below, 232–3 and 394–7.

4 Rivet 1969.
5 For a poignant and very instructive indication of the difference, see Flannery 1976, 161–2. A system is defined as the set of rules that generates a pattern.
6 The true number is probably even higher as might be deduced from the figures for the 1st century AD discussed below, 232–3 and 394–7.
7 See Haevernick 1960; Peddemors 1975, 106–8; below, fig. 120 (distribution map).
between settlements, there are also reasons why these could be or could seem to be absent.

There may well be a high degree of uniformity, caused by actual uniformity in the past as well as by discriminatory factors which are not archaeologically traceable. Also, the landscape itself may prevent both the genesis and the discovery of significant differences. As for the first aspect, it is conceivable that the rivers, by altering their courses or by inundating certain areas, cause fairly regular changes in the localities which are most optimally situated in relation to traffic routes, agricultural potential, and the like. Regarding the second aspect, it is also conceivable that the rivers themselves were the prime location, and the situation is such that if the most important settlements were normally located on the banks of, or very near, the major rivers, it is very likely that they have been eroded in later periods. All these factors will make it difficult to interpret the results of an analysis of the settlement types of the Late-Iron Age.

The structure of settlements

Although restricted in usefulness, the chronological position, location, and sorts of finds are the only attributes of settlements which can be used in an analysis of individual sites. Data on relative size of settlements, on the type of buildings, their disposition, and the like, are very restricted. As was explained in chapter 3, the size of an ancient settlement soil cannot be used as a measure of settlement size. The only data on settlement sizes are those provided by records of the extent of surface scatters of pottery in the literature or in the CAA, and these are seldom present. The same is true for data on the kind and number of the buildings in settlements, and on their internal structure. Intensive ploughing makes it virtually impossible to estimate, for example, the number of houses in a settlement from surface scatters, which means that excavation or remote sensing methods are the only potential sources of information.

There are a number of (partially) excavated settlements with recognizable late-Iron Age features. These are:

- Ede–Maanderbuurtweg (4), Zetten–De Hoge Hof (75), Raayen–De Woerd (109), Bemmel–De Heuvel (143), Ressen–De Kerkenhof (150), Ressen–De Woerdt (151), Lent–Het Laauwik I (158), Ewijk–Ewijkse Velden (232), Wijchen–De Pas (302), Wijchen–De Berendonk (333), Alverna–Heumenseweg (351), Nijmegen–Kops Plateau (417), Berghem–De Lallenberg (473), Haren–De Spaanse Steeg (475), Deursen–Onze Lieve Vrouwenberg (479), Oss–IJsselstraat (487) and Cuijk–Padbroek (515). Notwithstanding this rather long list of 17 excavated sites, these can contribute information only in a general way because they had limited results or have not, or inadequately, been published. The only investigated settlement north of the Rhine is Ede–Maanderbuurtweg. During the small rescue excavation at least one house-plan was discovered which could be dated in the Middle- to Late-Iron Age. The house measured 25 x 5.5 m and was partly three-, partly two-aisled.

Few such definite data are available from the river area, although the published excavation-plans and scattered remarks in the literature are very clear about one phenomenon, namely, the absence of recognizable house-plans in a number of cases. The excavations of sites 109, 333, 473, 479, and 515 are essentially unpublished, site 158 yielded Iron Age material but hardly any features, and the data on site 475 are mainly of a stratigraphical nature. These sites cannot be considered here. The excavations at sites 143, 150, and 302 were very limited, but they seem to indicate exactly the same as the larger excavations on sites 75, 151 (see fig. 65), and 232, namely, one or several virtually inextricable ‘swarms’ of post-holes, combined with a number of ditches, refuse pits, wells, or other features. As the excavator of site 232 put it in his annual report: ‘The numerous features do form several groups, as is evident from the distribution of granaries belonging to these groups, but still the other elements within these groups remain beyond the reach of our perceptive powers’.

Because as yet no ‘post-hole swarm’ settlement has been excavated entirely, it is not completely impossible that clear house-plans are actually present at the same sites. But observations such as those made in Ewijk (232) and similar phenomena in Zetten (75) or Heteren (93) indicate that this is rather unlikely. The excavation plans show a number of nuclei in the distribution of post-holes and the related features and finds indicate that these
must represent dwellings of some sort, which are extremely resistant to archaeological analysis. A convincing interpretation of this striking phenomenon is not readily available. Although found elsewhere, such clusters of post-holes occur so frequently in the river area that it can be considered a regional characteristic. Chronologically, it seems to be especially common in the Middle- and Late-Iron Age. Earlier settlements normally have recognizable house-plans, but the post-hole swarms do continue into the Roman Period, as demonstrated by sites such as Heteren-Het Lage Land (93) and Heteren-Uilenburg (95). On the other hand, they were never the only kind of settlement. Middle- to late-Iron Age houses have been excavated at site 351 near Wijchen and a continuous native tradition of house-building is also demonstrated by the plans excavated at sites 487 (Oss-IJsselstraat), 214 (Druten-Klepperheil), and other settlements outside our study-area.

In order to evaluate the significance of the post-hole swarm type of settlement, two factors of primary relevance have to be considered. First, that they are so characteristic for the river area. Second, the available evidence clearly indicates that these sites are not different from others in the sense that they represent the permanent residence of one or more families. Both statements require further discussion.

The fact that the post-hole swarms are characteristic for the river area should not be interpreted as a geographical restriction. They do occur elsewhere and in addition, as has inevitably happened in the past, one runs the risk of ending up with an unproductive culture-type 'explanation'. In this way, 'characteristic for the river area' has been interpreted as 'typically Batavian', the sites in question remaining an otherwise inexplicable 'cultural trait'. Instead, it is more useful to consider that 'river area' in its geological sense means the area of Holocene river deposits and that it is on those soils that post-hole

12 For a short report on site 95, see JROB 1983, 31–2.
13 Compare Verwers 1972, 87 and Abb. 57. The two-aisled houses in the settlement Wijchen-Holenbergseweg (Haalebos/Willems/Giebels 1976) near site 305 can be dated to the Middle-Iron Age only.
14 See Hulst 1978. Other examples are mentioned there (p. 145, note 13).
15 A good example is Braat's (1949, 34) disheartened conclusion that 'in the final analysis, Batavian settlements are never more than a tangle of post-holes'.
swarms occur so frequently. The habitable parts of these deposits, the stream-ridges, are also very fertile. This last circumstance has led to suggestions that the post-hole swarms could be not regular but special kinds of settlements: satellite settlements used for the exploitation of different natural resources during part of the year or central deposits for storage of agricultural produce. This interpretation is supported by the apparently numerous four- or more-post granaries visible on the excavation plans and the absence of regular house-plans. The latter is certain, because it is difficult to see how 'normal' house-plans can become completely unrecognizable even after a number of houses have been built on the same spot. As long as detailed ecological studies providing evidence to the contrary are lacking, the 'storage-site' interpretation remains a serious possibility. It contains, however, two elements. The first of these is the idea that the unusual structure of the sites should not be explained as a typical cultural phenomenon but as the result of social and economic factors leading to a particular subsistence strategy. This is a very attractive hypothesis which, as will be discussed below, is supported by the available information. The other element, however, is the suggestion that the structure of the sites implies that they were not settlements, that is, that no people lived there or at least not permanently. This suggestion is contradicted by the second primary consideration mentioned above, that they do represent permanent habitation. There are a number of reasons for this assumption. First, the increase in the number of sites and also the pollen diagrams clearly indicate population growth during the Iron Age and large parts of the Holocene channel and bank deposits can be shown to have been permanently habitable from the Bronze Age or at least the Early-Iron Age onwards. Permanent habitability does not imply permanent habitation, but the large scale deforestation and increase of pastures and arable land cannot be understood when the post-hole swarms are not considered to be permanent settlements. As far as we know today, their relative number, although of course difficult to quantify, is too high and we would be left with too few 'real' settlements to account for the observed changes.

A second reason is the fact that it is difficult to understand why special-purpose sites should have been created unless most of the population was in fact concentrated in only a few large and centrally located settlements. There is no evidence for these on the Holocene deposits, they were not necessary for environmental reasons and, as will again be seen below, there is no evidence of a hierarchically organised social system which could be expected in such a situation. In addition, there is the possibility suggested by the excavations in Ewijk (site 232) that at least sometimes a late-Iron Age post-hole swarm was succeeded by a settlement with normal houses during the Roman Period. An important third reason to consider them permanent settlements is provided by the finds and features of the post-hole swarms themselves. As already observed by Van Es, the mere reconstruction of granaries leaves too many post-holes unaccounted for. Moreover, as is evident from the citation on p. 224 above, the swarms as such are structured: they are composed of spatially differentiated clusters of post-holes, each with a number of identifiable granaries but also with other features such as refuse-pits, wells, ditches, and the like, identifying them as household-units. The quantity and the kind of finds from these sites is also not different from others. They have the same, large amount of pottery, sometimes (site 151) even evidence for pottery production, bones, querns, and loomweights. In short, they have everything one would expect on a permanently inhabited settlement-site, and we have to face the fact that they are a regular form of permanent habitation and, thus, there must have been houses. The houses are difficult to analyze, but that may be more a result of preconceptions regarding prehistoric houses which has led to a strong emphasis on recognizing variable but still highly formalized types of house-plans in Dutch archaeology. To be sure, there are good reasons for this approach and it has proved to be very effective; nevertheless, because of this it creates difficulties when very simple, unusual, or irregular house-plans are concerned. We are in trouble when there are too few traces, such as in some middle-Neolithic settlements, when there are too many, such as the post-hole swarms, or when

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16 There are possible exceptions, such as sites 302 and 417 (for the latter site see Holwerda 1943 and especially Bloemers 1974 and 1978b; see also chapter 6, p. 181, note 212), but the excavated parts of these settlements are too small for definite conclusions.

17 This hypothesis was proposed by Van Es 1981, 169.

18 See Teunissen 1982. An overview of several diagrams is presented in his fig. 11. See also fig. 137, below.

ties, but additional factors, such as the need for fuel in the undoubtedly caused primarily by increased agricultural activities, which is archaeologically difficult to detect. After all, the designation as post-hole swarm or cluster is somewhat misleading, because there are still regularities present, such as straight or curved rows of post-holes, different rows at right angles, etc. Viewed in this way, the post-hole cluster settlements from the river area are probably identical to very similar sites excavated in the adjacent German Rhineland. Recent excavations such as those in Weeze-Baal or Eschweiler-Laurenzberg have provided quite similar plans from which not only four-post granaries and six-post granaries or barns, but also nine- (or more) post houses have been deduced. In his overview of late-Iron Age settlements in the Rhineland, Joachim indicates that regular and formalized house-types such as those known from the Netherlands and northern Germany or the Haps-type houses are absent. Houses tend to be small, often 9-post, structures with a surface of only c. 15 m², but other and irregular configurations as well as larger one- to two-aisled houses (e.g., Grevenbroich-Gustorf) also occur. They represent a tradition of house-building which, according to Joachim, is undoubtedly derived from southern Germany and which goes back to at least the late-Bronze – Early-Iron Age. It thus appears that this tradition coexisted with others in the Dutch river area because there is not much doubt that structures such as described by Joachim can be found in our post-hole swarms. In addition, it is not improbable that during the Late-Iron Age first-quality timber was no longer readily at hand due to the large-scale deforestation. The use of lesser quality beams may have caused additional irregularities in plans which are difficult to recognize as it is, and which overlap as well.

Finally, as Joachim has also observed, settlements with Eschweiler-type houses may testify to a different subsistence strategy, based on arable rather than mixed farming. At least for Eschweiler, this idea is also backed by ecological analysis. It is further supported by the large number of granaries on these sites and it confirms the economic interpretation discussed earlier, at the same time reconciling it with the permanent habitation argument. It is justified to propose, therefore, that the structure of settlements during the Late-Iron Age shows two different kinds of sites, due to a difference in house-building traditions which existed at least from the Middle-Iron Age onwards. The coexisting traditions are related to different subsistence strategies. The normal strategy is that of mixed farming, which is archaeologically visible in structures with a house and a byre under one roof. These occur in the river area, but they are almost universal on the generally less fertile Pleistocene soils. They are highly formalized and only slowly evolving house-types which, for the period under discussion, have typical two- or partly two-, partly three-aisled plans in the river area.

The very fertile Holocene river-clays, just as the equally fertile loess areas, allow a more specialized subsistence strategy of arable farming. Although animals are surely present and even necessary in that context, the relatively sophisticated house-and-byre structure is apparently replaced by a more simple house with spatially segregated structures around it. In this respect, it should also be remembered that the river area is not only suitable for specialization in arable farming. The large flood-basins are, in principle, eminently suitable for the opposite strategy, cattle-breeding. That situation can be con-

20 Beex/Hulst 1968.
21 Müller-Wille 1966; Joachim 1980. Location on fig. 80, p. 283.
22 Joachim 1982.
23 Reichmann 1979a. For location, see p. 283, fig. 80.
24 The analysis of Eschweiler-Laurenzberg was greatly facilitated – and is thus more reliable – by the fact that it had only one occupation phase of relatively short duration. There were very few overlapping features.
25 This is shown by the pollen diagrams (cf. note 18) and undoubtedly caused primarily by increased agricultural activities, but additional factors, such as the need for fuel in the smelting of iron ore and other industrial activities may have been significant. On the other hand, it is certain that first-quality timber could still be obtained in the region, as is evident from the presence of little disturbed oak and birch-woods near site 417 in the second decade BC (Teunissen/Teunissen-van Oorschot 1980).
27 Knörzer 1980, esp. 446–7 and 456.
29 Van Es 1982, 144–5.
connected to the post-hole swarms as well, because large herds of cattle cannot be stalled in a normal byre. They must normally have been left in the open in all seasons, and their owners could suffice with building much simpler houses for themselves only. In this situation, a large number of ‘granaries’ can also be expected, as storage facilities for fodder in the winter season.

Some further implications and evidence for the interpretation offered here are discussed in chapter 10. It offers at least a potentially fruitful insight into the causes of diversity in settlement and house types in the eastern river area. This diversity cannot, however, be easily if at all related to artefacts and certainly not to those from surface collections. It can only be established after excavation and is thus impossible to employ in a detailed differentiation of all known settlements.

Chronology, location, and finds
As far as chronological differentiation is concerned, but also in view of what has been said above, it is important to check whether the settlements already occupied before the Late-Iron Age have recognizable house-plans. From the few excavated sites there appears to be no relation between a long pre-Roman occupation and the presence of house-plans. Sites 4 and 351 have them but sites 151 and 232 (and possibly 302 and 417) are of the post-hole cluster type.

Similarly, there is no relation between a long pre-Roman occupation and the location of settlements on Pleistocene v. Holocene deposits. Of the 44 (45) settlements which existed before, during, and after the Late-Iron Age (see chapter 4, fig. 23), 14 are located on Pleistocene and 30 (31) on Holocene deposits. The 14 sites should be augmented with another 10 examples which existed before and during the Late-Iron Age, which results in a ratio of 24:30 (31). It should be noted that all of the settlements which did not continue into the Roman Period (4, 6, 24, 184, 185, 288, 302, 350, 351, and 523) are located on Pleistocene deposits. But this fact does not necessarily imply anything about late-Iron Age settlements. It could just as well be relevant in the context of the Roman occupation or not at all.

As far as the finds are concerned, there is also very little evidence of differentiation available. There are a few sites which have yielded somewhat extraordinary material such as bronze or iron La Tène fibulae (43, 209, 270) or pins (209, 270), bronze bracelets (209, 244), a bronze buckle (296), and iron knives (360). Relatively late are bronze Nauheimer fibulae (296, 349) and copper rainbow-cups (239, 244).

The glass bracelets discussed in paragraph 6.6.3 cannot be considered extraordinary in a qualitative sense, because they have a very wide distribution (see fig. 120). In a quantitative sense, however, the sites in the region around Wijchen are remarkable for the large numbers of bracelets found on them, a fact which was already observed by Peddemors.

The material evidence, thus, does not point to a significant association with long, continuous occupation or with different geological deposits. In fact, most of the evidence points, if at anything, to the near absence of differentiation between settlements, except economic specialization. The few special objects, accidentally discovered on a few sites, may not be used to set these sites apart from the others. The numbers of glass bracelets are more useful in this respect, because, whatever their interpretation may be, it is a reasonable assumption to associate them with some sort of wealth. This does still not permit differentiation between individual settlements, because the data on different sites have been collected in a way that is anything but uniform. But it does provide a clue in the sense that the region around Wijchen and along the Meuse at this point (in general: the middle of the south-western quadrant of the area covered by the map) is wealthy, albeit in a very relative sense.

At this (micro-regional) level it is also evident that the few exceptional metal finds are concentrated in the same area and that most of the sites involved are situated on Pleistocene or relatively old Holocene deposits. These sites may also be larger than others, because Appendix I clearly shows clusters of them which may, in reality, have constituted only one settlement. After all, settlements are only interpretations of findspots. On the

30 See p. 374–6.
31 Peddemors 1975, 105. Several sites can be added to his list, such as for example 275, with at least 37 bracelets.
32 It would be possible to name a few ‘rich’, but only in very rare cases to indicate definitely ‘poor’ settlements. The distribution pattern of glass bracelets is also distorted somewhat by

33 Especially the branch of the Waal around Ewijk and probably also the Wijchenese Maasje. See chapter 3, 42.
Holocene deposits, indications for such large sites are lacking.

Conclusions
When the evidence discussed in this paragraph is evaluated, it is clear that it still leaves much to be desired. Nevertheless, what we do have is enough to reach at least some conclusions about what late-Iron Age settlements in the river area were like. The most important conclusion must be that in general they were all more or less the same and that differentiation is very hard to find.

The only very clear difference is that between the structure of two kinds of settlements on the Holocene deposits, one having technologically fairly sophisticated houses with a byre, the other with simpler and smaller houses almost indiscernible from subordinate structures around them. A plausible interpretation for this phenomenon is a difference in subsistence strategy and economic specialization on the latter sites. But except for the fact that these are especially characteristic for the Holocene river deposits, they are like the others in all aspects that can be considered; as far as we know they are of approximately the same size and yield the same finds, without material diversity.

Such material may be absent because it has not been found and the fuzziness of the evidence can partly be attributed to a lack of archaeological investigation. On the other hand, the large quantities of finds from a considerable number of sites over the entire region, in addition to the data from excavations, should have provided firmer arguments for differentiation than are available. It is, therefore, not at all improper to conclude that at least material (and thus archaeologically traceable) differences must have been rather marginal.

Although there is economic or functional diversity, there is thus no apparent inequality and higher-order settlements are not identifiable. Only of the area around Wijchen can it be said in general that sites there are somewhat ‘richer’ and perhaps larger. The available evidence thus does not offer any arguments for a settlement hierarchy but suggests that if there is differentiation it is in degree and not in kind. The relative importance of the settlements at the top of the (short) scale, those around Wijchen, increases—or becomes archaeologically better visible!—in the last century BC. But the overall picture remains that of equality, which would agree well with the historical evidence, which does not suggest a highly stratified society.

There is only one factor which could change this picture significantly, namely, the destruction of important settlements by selective natural processes, especially the erosion of sites by rivers. The observed but fuzzy concentrations of wealth and trade contacts are located near major functioning rivers in the Late-Iron Age, and most of them situated at relatively high points. It is possible that the hypothetical most important settlements were located on—at that time also very high—points on the river banks. A site like 275 could then be viewed as an accidental survival, because the branch of the Meuse it is located on was cut off and did not develop further in later periods, while other settlements may have been completely eroded. In fact, there is even evidence for this further to the west, near Rossum and Lith. As Roymans and Van der Sanden showed, the finds collected during dredging at this strategic spot, where Meuse and Waal are very close together, point towards the existence of important settlements there, notably to a late-La Tène centre which is quite probably a result of the Batavian immigration. It does, therefore, not prove the existence of important settlements on the river banks during the Late-Iron Age. But it does indicate that the process of erosion of settlements did happen and that at least one very important settlement was actually located there at the end of the Iron Age.

There is one other important and possibly late-Iron Age settlement which should be discussed here, namely, the famous settlement at the Kops Plateau (417) in Nijmegen. Between 1915 and 1921 J.H. Holwerda directed excavations there which were, for those days, rather extensive. He concluded that he had found the oppidum Batavorum mentioned by Tacitus. Later excavations, which yielded an excellent stratigraphy, have shown that this interpretation is very unlikely.

In a recent review of all the available evidence it has been concluded that the initial settlement, which starts around 10 BC, can be attributed to military occupation which lasted to c. AD 10. The oppidum Batavorum, which is tentatively identified as the equivalent of Batavodurum, is (also tentatively) located at the large early-Roman settlement on and around the Valkhof (site 403).

34 See chapter 10.
35 Roymans/Van der Sanden 1980; see also chapter 11.
36 Holwerda 1920; 1921.
37 Bogaers/Haalebos 1975; see also chapter 11.
38 Bogaers/Haalebos 1979a and Bogaers 1979a.
Whether these identifications are correct, remains to be proved. For the present discussion it is significant that both sites 403 and 417 are early-Roman. The available and very considerable amount of material from both sites includes virtually nothing that points to the latest phase of the Iron Age, in contrast to the eroded settlement near Rossum/Lith mentioned earlier. Both these sites can therefore safely be ruled out as possible major late-Iron Age settlements, although attention should be called to pre-Roman occupation near both of them. Late-Iron Age pottery which probably points to a settlement has been found at site 402. The southern part of site 417 is definitely a settlement; in the literature it is usually referred to as the ‘Batavian village’. The excavations in 1975 showed that this site was already occupied in the Iron Age. In neither case, however, are there indications that the settlements were in any way especially important, nor indeed of positive connections with the early-Roman settlements.

8.2.2 Cemeteries
Archaeological evidence of burials in the eastern river area during the Late-Iron Age is almost completely lacking. Because the area was definitely inhabited and people inevitably die, there is a problem of archaeologically untraceable burials during the entire period. There are several observations which are relevant to this problem. First of all, it is not unique. At the beginning of the Iron Age, both in the northern and southern parts of the Netherlands, most if not all people were buried in large cemeteries, the so-called urnfields. Recent discussions of the evidence in both regions are provided by Kooi and Verwers. There are several differences between these two regions, most notably the near absence in the north of wealthy burials which occur in the south. These are also present in and near the river area (Oss, Wijchen, and possibly Ede), where large urnfields are also found. The material evidence clearly indicates that the urnfields in the river area are directly related to the southern group, but this is of little importance for the present discussion.

It has been established that both in the south and in the north several urnfields continued to be used throughout the Middle-Iron Age, although not in exactly the same way. There were several changes in the burial practices, some of which may go a long way to explain why virtually no burials dating to the last two centuries BC have been traced so far.

In both areas, as well as elsewhere in adjacent Germany and northern Belgium, interments within square ditches were a new phenomenon in the Middle-Iron Age. Plans of excavated cemeteries show very peculiar combinations of round and square ditches, as well as burials without peripheral structures. It is significant that when native cemeteries become archaeologically visible again, which is almost from the very start of the Roman Period, the excavation plans show exactly the same phenomena (examples from our area are sites 396 and 491). It would thus seem logical to assume continuity of burial practices in the centuries in between, although it remains to be shown why the burials have not been found.

An indication for this may be other changes during the Middle-Iron Age, notably the clear tendency to omit the use of urns and, even more significantly, of the grave-pit. Although the latter development has been demonstrated most clearly for the northern part of the Netherlands, changes like this make it easy to understand why late-Iron Age burials are not normally archaeologically traceable. Burials without urns or grave-goods are not noticed easily, those without a grave-pit are even less likely to be discovered.

Extrapolation from the Middle- to the Late-Iron Age leads to at least four different sorts of internments which are to be expected. These are:
1. cremation in a pit surrounded by a peripheral structure
2. cremation in a pit
3. cremation and pyre remains within a peripheral structure
4. cremation and pyre remains

The presence of a barrow may be assumed for all possibilities but this need not be so (especially in cases 2 and 4), which introduces another source of variation. There will

39 Holwerda 1943; Bloemers 1974 and 1978b.
40 Kooi 1979; Verwers 1972.
42 See Modderman 1951, 30-45.
43 Verwers 1972, 34-7, 42-8 and 51-3; Kooi 1979, 133-4 and 174. A concise overview of the dating evidence for both areas is presented in Lanting/Mook 1977, 159-65.
44 This continuity has been demonstrated elsewhere, see e.g. Decker/Scollar 1962 and Wightman 1970.
undoubtedly be even more alternatives, but the important point is that none of them is likely to be discovered. Even if burials of this sort occur in older cemeteries, they cannot be identified as such after excavation, unless radiocarbon dates place them in the Late-Iron Age. In this respect, it is also noteworthy that, with the start of the Roman Period, cemeteries immediately or very soon regain visibility in the south, while their northern counterparts remain the same and can only gradually be identified more clearly.  

The few possibly or certainly late-Iron Age burials mentioned by Lanting and Mook are already sufficient explanation for the near absence (= invisibility) of such burials in the eastern river area. An additional case was recently reported from its immediate surroundings: a small group of cremation burials from Oss-Ussen, which was accidentally discovered during the large-scale excavations there, yielded a radiocarbon date definitely placing it in the Late-Iron Age.  

There are, however, also at least a few possible burials from the eastern river area which should be mentioned here.  

There are a number of sites (314, 324, 520, 521, and 522 in the catalogue) which refer to the large urnfields around Wijchen. These have never been archaeologically investigated, but large amounts of urns found on them were systematically collected by the local factory-owner F.J.G.H. Bloemen and partly published by Modderman. While most of the pottery can be dated to the Late-Bronze Age and Early-Iron Age, there is also material from the Middle-Iron Age and a few Roman Period urns. In the recently published catalogue of sites in the Land van Maas en Waal, the sites mentioned above are, however, also indicated as having late-Iron Age pottery.  

It is conceivable, therefore, that some of the cemeteries do not have to be supposed but can actually be proved to have been continuously used until, or occasionally even into, the Roman Period. The evidence presented in a subsequent study in preparation by Peddemors has to be awaited on this matter; the sites involved have been indicated as isolated finds on Appendix 1.  

There are also three cemeteries, two of which are indicated as such on the map, which could conceivably contain late-Iron Age burials. The first is site 411, which was accidentally discovered during excavations in Nijmegen in 1974. Apart from a middle-Iron Age grave with the remains of spearheads, a horse-harness, and a waggon, the cemetery consisted of simple pits with cremations. It is located immediately to the northwest of the early-Roman cemetery 409, which is probably a burial-ground related to the large settlement around the Valkhof (403). It is tempting to propose that the entire complex was used continuously from the Middle-Iron Age onwards, but for lack of datable objects or C14-dates this has to remain a (rather remote) possibility.  

The second cemetery is site 490 near Uden in Brabant, partially excavated by the RMO in 1923. The earliest pottery from this cemetery dates to the Early-Iron Age, the latest is early-Roman, Gallo-Belgic ware, found together with two spoon-bow fibulae and other items. In the excavation report 38 burials are mentioned, of which 22 contain pottery. None of the burials was placed in a pit but all were surrounded by a ringditch. Again, it is quite conceivable that the cemetery was continuously used from the Early-Iron Age until the middle of the first century AD, but definite proof is lacking. The third cemetery, if it can be called such, is a collection of 'insignificant cremation burials', discovered before and during the 1974 excavations at Ewijk (site 232). They were generally located at some distance from the settlement. The undatable burials could well be connected to the central settlement, which dates to the Middle- and Late-Iron Age.  

Conclusions  

It is only occasionally possible to indicate with certainty a late-Iron Age burial. Their near absence can, however, be explained by their archaeological invisibility, probably caused by changes in burial practices which had already started in the Middle-Iron Age. These were reversed again at or after the end of the Iron Age, and are in some way related to the Roman occupation. Late-Iron Age burials are unique in that they are all so difficult to detect. A few may have survived under the rather unfertile (drift?-) sands near Wijchen and Alverna.
and others, which are deep enough under the present-day surface, have only been found as the accidental by-product of large-scale settlement excavations such as those in Ewijk and Oss-Ussen. Even then it is not a simple affair to date such simple cremation burials. That they are all so plain and difficult to find implies that they must have been quite similar in their material aspects. Little expenditure of energy is required to construct burial monuments and normally no grave-goods are present. This situation is different from the earlier urnfields which show more variation and occasionally exceptional burials such as those of Wijchen and Nijmegen, and also from the Roman Period cemeteries. It would be a bit too far-fetched to assume that for a period of roughly two centuries there was, therefore, no social and or economic inequality among the inhabitants of the river area. After all, the recognition of the status of any individual at his death may also be expressed in other ways. But the absence of any material expression of status strongly suggests that social differences in the population must have been rather limited. They should probably be comparable to those of the earlier Iron Age or, instead of being a further development in socio-economic stratification, represent even a simpler form of social organization.

8.3 THE EARLY-ROMAN PERIOD

The Early-Roman Period is rather short; it was defined in chapter 2 (p. 24-5) as the period between 12 BC and c. AD 50. Nevertheless, there is evidence for the occupation of at least 142 settlement sites. As mentioned in chapter 4, this evidence may be composed of both positive and negative arguments. Positive is the presence of early-Roman imported material, in most cases pottery. Although it is possible that in the course of six decades at least some of this material landed in every existing settlement, it is highly unlikely that it can be discovered on every one of them. The evidence presented in chapter 6 clearly indicates that large amounts of Roman pottery only became available from the end of the first century onwards. Early-Roman pottery will therefore only be found on sites which actually had quite a lot of it and, in some additional cases, by sheer luck. A simple calculation even shows that it is highly unlikely that any material is found at all.

If a site is assumed to have been occupied from the Late-Iron Age to the end of the Middle-Roman Period (which is frequently true) and that the amount of pottery used at any one time remains constant (which may not be accurate, but in the present context an acceptable proposition54), then the percentage of pottery from the Early-Roman Period will be c. 1/9th (11%) of the total quantity. If, further, a proportional rate of recovery is assumed for material from all three periods (in general not unlikely because most sites are hardly, if at all, stratified), then the percentage of early-Roman pottery in the sample will also be 11%. If the imported (Gallo-) Roman wares constituted 10% of the total amount of pottery used during the Early-Roman Period (which is probably a very optimistic estimate), the fraction of these wares will be only 1.1% of the sample. Finally, it is also necessary that the sherds are recognizable as early-Roman. This means that only rims (and not even all of them) and some other sherds are suitable, which is certainly not more than 25% of the total. An optimistic estimate of the fraction of identifiable early-Roman imported wares in a sample would therefore amount to only .275%, which is 1 out of 364 sherds!

These figures imply two things. First, that those sites which actually have a reasonable percentage of early-Roman sherds are probably special in some way; and second, that the absence of such sherds means nothing as far as the dating of unexcavated sites is concerned (except, of course, when a very large and representative collection of material is available). For this reason, definite occupation of sites during the Late-Iron Age and also during the Middle-Roman Period has been taken as (negative) evidence for occupation during the short Early-Roman Period.

Apart from the 142 settlements for which occupation is acceptable, there are no less than 118 possible or probably early-Roman settlements. They are generally sites where only relatively small amounts of material (<100 sherds and often only a few) have been collected. Most

54 The amount of pottery used is, of course, determined by such factors as the number of people living at a site and their social and economic behaviour, as well as, for example, the durability of different sorts of pottery. Native ware may break more easily than Roman pottery and thus cause a larger amount of pots discarded during the Late-Iron Age and Early-Roman Period. On the other hand, population numbers and variability of pottery presumably increased during the Middle-Roman Period on most sites.
of these are late-Iron Age sites which could still, or late 1st- or 2nd-century sites which could already be inhabited during the Early-Roman Period. The maximum number of early-Roman settlements is therefore 260, but an estimate of the original number is very difficult to attain by this figure. It is close to the maximum of c. 300 calculated for the late-Iron Age, but an evaluation of early-Roman population-size shows that the true number is more likely to be in the order of 400 settlements.\(^{55}\)

It is altogether possible that the actual number of settlements is roughly the same as during the late-Iron Age. The general tendency towards continuity of occupation visible in fig. 23 (chapter 4) would be in accordance with such a thought, but it does not constitute proof. It is very unfortunate that the presence or absence of conceivable negative effects of the arrival of Roman armies on the number of settlements cannot be evaluated more precisely for lack of reliable archaeological indicators.

There are, however, a few arguments which make a considerable negative impact unlikely. These are the continuity in native pottery traditions, the large number of essentially native middle-Roman settlements\(^{56}\) which cannot have come out of thin air, the positive effects of the occupation which led to new settlements (at the very least the military camps and related civil occupation), and also not only the absence of large-scale hostilities but, on the contrary, the presence of unusually friendly relations between Romans and natives which have been deduced from the classical sources.\(^{57}\) All in all, the total number of early-Roman settlements may be estimated as at least equal to that of the Late-Iron Age, due to Batavian immigrants presumably larger.

As far as the typology of settlements is concerned, it is evident from Appendix 2 and fig. 61 that there is obvious and clearly identifiable differentiation. The cause of this differentiation is, of course, the arrival of Roman armies. The known camps have usually been to some extent excavated, but an insight into the properties of excavated camps may lead to the identification of possible other military establishments and also to conclusions about civil settlements. Any differentiation of the latter will necessarily have to be based on measurements reflecting primarily differential interaction with the new superstructure.

8.3.1 Military Settlements

As far as military camps are concerned, a consideration of the Nijmegen sites 403, 406, 412, and 417 (see fig. 66) is of fundamental importance. A concise discussion of all the available evidence on these sites has been published recently.\(^{58}\) Unfortunately, only sites 406 and 412 are known well enough to permit a definite interpretation as military camps. Site 417 has also been defined as an, at least initially, military settlement, while site 403 is considered to be predominantly civilian. It is important to see which criteria are, or could be, employed to reach those conclusions. These criteria relate to four aspects of the sites: their location, their structure, the finds, and the available historical information.

All four sites are located on Pleistocene deposits. Sites 412 and 417 on the highest points of the ice-pushed ridge, 403 and 406 somewhat lower on fluvioglacial deposits. All sites are located on the brink of the ridge, which is very steep in the east (417, 412) and gradually declines to the west. The altitude over the (current) mean water-level of the Waal is 50 m for site 417, 35 m for 412, 25 m for 406, and 20 m for site 403. As shown by Appendix 2 and in part also visible on fig. 66, all sites are bordered to the south by dry valleys.

They are evidently located at high, easily defendable positions. This observation, combined with the fact that, notwithstanding very extensive excavations in the entire area, virtually to traces of pre-Roman occupation have been found,\(^{59}\) indicates that the location of all four sites, and especially 417 and 412, is determined by military strategic demands. On a clear day, it is possible to look out from these two sites over the entire Overbetuwe up to the Rhine and the ice-pushed ridges at Arnhem.

Sites 406 and 412 are clearly defined as military camps by their peripheral structures. Site 406 has a V-shaped ditch and a wall, site 412 has two V-shaped ditches, large wooden gates, and towers. Inside these structures, no definitely contemporaneous traces of buildings have been discovered, so it is assumed that both fortifications were largely tent-camps.\(^{60}\) Sites 403 and 417 are not as

\(^{55}\) See below, p. 396–7.

\(^{56}\) See below, p. 246.

\(^{57}\) See Bogaers 1960–61, esp. 263–72, for a summary of the arguments. See also paragraphs 10.3–4 and 11.3.

\(^{58}\) Bloemers/Bogaers a.o. 1979.

\(^{59}\) This observation is confirmed by a pollen diagram (see

Teunissen/Teunissen-van Oorschot 1980) indicating a nearly undisturbed oak forest on the hills. For an additional interpretation, see also below, ch. 11.3.2.

\(^{60}\) For site 412 evidence for timber buildings is, however, not lacking: see Bogaers/Haalebos 1977, 97–9 and 1980b, 56–9.
extensively excavated, but both may have been surrounded by ditches: a V-shaped ditch around site 417 and an asymmetrical V-shaped ditch (a so-called fossa Punica) around site 403. The restricted excavations since 1980 have produced plans of wooden buildings on

61 417: Bogaers/Haalebos 1979a, 19, Abb. 6, 3; 403: Bogaers 1979a, 30, Abb. 23, 5. The ditch is probably pre-Flavian.

Fig. 66 Nijmegen: sites from AD 30–70. For site nos., see chapter 5 (catalogue): 1 contour lines, 2 railway, 3 excavated area, 4 topographical coordinates, 5 outlines of buildings (not on this map), 6 double ditch around the Augustan legionary camp, an area unoccupied in this period, 7 inhabited area; investigated and/or many finds, 8 inhabited area; not investigated and/or few finds, 9 ditch, certain and hypothetical trajectory, 10 cemetery; investigated and/or many finds, 11 cemetery; not investigated and/or few finds, 12 road. Scale 1:10,000.
site 403 and there is enough evidence to assume that these are present on site 417 (cf. note 60).

As far as the finds are concerned, no comparative quantitative analysis is possible because of lack of sufficient published data. In general, it seems that a fair amount of native pottery is present, but the quantities are rather insignificant in relation to (Gallo-)Roman wares. It is also worth noting that the early-Roman wheel-turned pottery from the four Nijmegen sites together probably accounts for 99% or more of all such pottery found in the eastern river area so far. The only well-published excavation to provide interpretable data is the investiga-

tion of a chronologically significant sequence of layers from site 417. The data included in the excavation report permit a calculation of the percentage of native pottery, which is c. 5%. The conclusion of the excavators that native pottery increases strongly from the oldest to the later layers is definitely not correct. Layers 1-2 do indeed contain little native pottery, namely 4% (99:4), but the same is true in later periods: layers 3-5 have 6% (230:15) and layers 6-7 have again 4% (442:20). In qualitative terms there are two properties which can be considered, apart from ‘preponderance of Roman pottery’. All settlements have yielded specifically military metal objects. Intelligible graffiti have also been found on all of them, and an early inscription was found in secondary position on site 403 (fig. 67).

From a review of the available material evidence, it is clear that the differences between the four sites are primarily related to differences in the level of investigation. There are no compelling reasons why sites 403 and 417 should not be interpreted in exactly the same way as the camps 406 and 412, namely as military settlements. There is, however, historical information about an oppidum Batavorum and about a place called Batavodurum. As Bogaers has convincingly demonstrated, Batavodurum should certainly be localized in Nijmegen. It is also likely that both names refer to the same place, a town (fortified capital, stronghold) of the Batavians. This should imply at least a large civil component for such a town. If ‘civil’ is in some way related to ‘native’, neither sites 403 nor 417 qualify as a civilian settlement on the available evidence. Site 417 has been shown to have started as a military camp on the basis of the finds and there is no convincing evidence that it did not remain a camp until the end of its habitation around AD 70. Its location is also hardly suitable for permanent civilian habitation. Therefore, site 403 is the only plausible location for a civil Batavodurum. Although the identification rests mainly on historical arguments, the relatively low location of site 403 and its proximity to the river make it acceptable. The traces of buildings and indications for industrial activities discovered during recent excavations, as well as the absence of really significant quantities of native artefacts, imply, however, that the Batavian capital was as ‘Roman’ as a settlement or even a camp could be. The material evidence indicates that, if the identification is correct, the Batavian capital was surely not ‘native’ and not even definitely ‘civilian’. In fact, on the present evidence it could more accurately be described as a large military vicus.

Such an interpretation does not conflict at all with a status as Batavian capital, especially where no such centre had existed in Nijmegen before the Roman Period. The combination is also the only way to explain why the capital should look like a military site or – at least partially – be one. It is evident that in the Early-Roman Period any site with V-shaped ditches and virtually nothing but Roman pottery and other indicators such as intelligible graffiti and military metalware is itself, or is directly related to, a military camp. The archaeological data thus suggest that Batavodurum/oppidum Batavorum, instead of (fortified) capital or stronghold of the Batavians, is more properly understood as either ‘in the land of the Batavians’, or as ‘of the civitas Batavorum’.

In the area outside Nijmegen, there are only two other sites which can be positively identified by one or more of these criteria. The first is site 126 in Meinerswijk, where the lowest levels of a small trial trench contained early-Roman pottery with a few native sherds. They also contained at least one military metal object and several sherds with parts of graffiti, one of them possibly indicating a legion (fig. 98).

The second is site 499 in Cuijk, where traces of V-shaped and other ditches and foundation trenches of (1981, 132) considers this also likely, on account of the distribution of early coins. On the other hand, there are early military defence works known from other towns (e.g., Frere 1978, 283 f.), so the possibility of a fossa Punica around site 403 does not necessarily point to a purely military occupation.


66 Recent excavations around the Kelfkensbos in Nijmegen have revealed the presence of several presumably early ditches at site 403, which makes it even more easy to accept at the very least an initial and/or partial military occupation. Van Es (1981, 132) considers this also likely, on account of the distribution of early coins. On the other hand, there are early military defence works known from other towns (e.g., Frere 1978, 283 f.), so the possibility of a fossa Punica around site 403 does not necessarily point to a purely military occupation.
buildings (barracks?) were discovered. The finds of the excavation have never been published in detail.

Apart from the six settlements discussed so far, there are another three sites where military camps could have been located: 117/8, 183, and 194. Of these sites, only 117/8 has yielded material which allows such a tentative identification. It is located in Driel, where a very large (c. 8.5 ha) ancient settlement soil occupies three parcels of land: the Baarskamp and Lizesland, which have been interpreted as one site (117), and the Marskamp (118), which is somewhat further to the northwest but probably forms part of the same settlement.

Site 117/8 is somewhat exceptional, because it must have been a settlement for several centuries before the Roman Period. This is, however, in no way an impediment to identification as a military camp. Site 117/8 is located on the channel deposits of a pre-Roman branch of the Rhine. This branch was still functioning during the Roman Period but as discussed in chapters 3 and 7.2 it was no longer the main channel. Driel is situated at the point where it joined the new main channel, which was formed during the Iron Age. Site 117/8 is thus located at a high point with waterways in three directions: a rather favourable situation if it was a military camp which functioned in the context of an area (the Insula Batavorum) used as an assembly area for troops and a base for offensive campaigns across the water.

Apart from the locational and rather general historical arguments, all conclusions on the nature of the early-Roman settlement depend on the finds because no excavation has yet taken place. Since the last war, a rather large amount of material has been collected at the surface of sites 117 and 118. Although one occasional observation has shown that at least part of site 117 is rather deeply stratified (c. 1.5 m), the collection of sherds contained about a dozen identifiable fragments of early-Roman pottery. For this reason the fort, if there is one, is most likely to be located at site 117 while site 118 represents the surrounding occupation, but this is nothing more than an educated guess. Together with one (countermarked) Augustan coin, these dozen sherds are all de-
finally early-Roman material. But in view of the exceedingly small chance of finding such material at the surface and the near absence of it elsewhere in the region, these finds can be regarded as the ‘tip of the iceberg’ and an indication of early-Roman military occupation. In addition to these arguments, the probability that site 117/8 was (converted into) a frontier-fort in the Middle-Roman Period may also be taken as support for this interpretation. As is evident from excavations in many forts, there is a strong continuity in the sites that were occupied by the army. Camps from the earlier period which were located in such a way that they could be incorporated as forts in the limes, were indeed used as such, for example, Vechten, Meinerswijk, Altalkalkar, and Xanten).

Because of this last consideration, site 194 may also be regarded as an early-Roman camp. The finds (brought up by dredging) include clear indications for a middle-Roman fort, as well as several identifiable early-Roman sherds. The context of the discovery does, however, not permit more definite conclusions. It is quite conceivable that the dredging should have yielded a higher percentage of early pottery if site 194 actually was a military camp during one or more of the offensive campaigns under Augustus or Tiberius. In addition, the available sherds do not necessarily point to such an early date. Arretine terra sigillata is lacking, and mortaria Haltern 59, cork-urns Haltern 91A, and even flagons Haltern 48 still occur a few decades later. If, however, site 194 is interpreted as a middle-Roman fort, these finds indicate that it must have been established at a relatively early date either at the end of Tiberius’ reign, under Caius, or at the beginning of the Claudian period.

The last site where an early-Roman camp must have been located is no. 183. This is based almost entirely on historical evidence and as a site (a physical phenomenon) it is hypothetical. Its exact location is unknown and its position on Appendix 2 is, though not completely arbitrary, little more than an indication. The arguments for its existence and location are outlined in paragraphs 3.3.3 and 11.3.2. Its presence somewhat to the south of site 182 is assumed in relation to Drusus’ dam at the fork of Rhine and Waal. The only archaeological find is a tombstone from the associated cemetery (fig. 17), dated to before AD 50, on which the dam (moles) is mentioned.

Unfortunately, confirmation of the existence of site 183 will be nearly impossible to obtain. The meander-belt is extremely wide around Lobith, and the constantly shifting channels have probably eroded all older deposits. In addition, the camp would not have had stone buildings, perhaps not even tile-roofs, the remains of which would have facilitated discovery.

8.3.2 Civil Settlements

In general, the information about the structure of early-Roman settlements in the river area is just as limited as about those of the Late-Iron Age. There are few excavations in addition to the list on p. 224 of sites which were occupied from the Late-Iron Age onwards, namely, Ede-Manen (3), Kesteren-Hoge Woerd (43), Heteren-Het Lage Land (93), Heteren-Uilenburg (95), Elst (105), and Overasselt-De Bulleenkamp (356). But except for site 105 they have produced no additional information compared to what has been said about late-Iron Age settlements. The investigations at sites 3, 43, and 356 were extremely limited, and sites 93 and 95 are of the post-hole swarm type already discussed.

Although the information from excavations is limited, it seems that the same types of settlements which existed during the Iron Age still constituted about all there was in early-Roman times: there are settlements with one or more houses of clearly identifiable types and there are the post-hole swarms representing a different form of house-construction. If there were any changes in the internal spatial organization of settlements, these have not (yet) been detected. The recognizable ‘standard’ house-plans from the Roman period are a further development from those of the Iron Age, as is demonstrated, for example, by the two- or partly two-, partly three-aisled houses from Oss, Druten, Ewijk, and Wijk bij Duurstede. These are characteristic house-types of the eastern river area and the Pleistocene soils of Brabant, and seem to have evolved from the Iron Age houses of the ‘Haps-type’. Traces of ‘Roman’ or ‘vicus type’ one-aisled houses have not been found in early-Roman native settlements. The only examples known so far were discovered on site 403 in 1981.

Apart from the gradually changing house-plans, a process which was probably going on during the Early-Roman Period, the settlements seem to have stayed more or
less the same. There is, at the moment, no real evidence for the introduction of new settlement-types or types of buildings, with the exception of site 403 which is not a native settlement. This does not imply that such a development did not occur before c. AD 50, for example in a settlement like Elst (105). The first temple there was built around AD 50, but a settlement (probably also a religious centre!) existed before that time, as is demonstrated by a layer with (secondarily deposited?) settlement debris underneath the building level of temple I. Site 105 is located at a central spot in the Betuwe, and the fact that the temple was built there is hardly coincidental. An early-Roman local centre of some importance was probably situated in the immediate vicinity of the spot where the temple was built, in an area which is at present almost completely built over. This does, of course, not mean that this important settlement was a native affair. It may have been because, after all, the site was already inhabited in the Late-Iron Age, but other excavations, for example at Grobbendonk (Belgium), have shown that the construction of temples may be directly related to a previous function of the settlement in a Roman context. Another new type of settlement which must have appeared in the Early-Roman Period is the military vicus: the settlement in the immediate vicinity of a fort inhabited by civilians, in particular artisans and traders. Structurally, these vici should not be difficult to identify as such, because their layout and the types of buildings can be expected to differ significantly from native settlements. In fact, most of the features and finds discovered at site 403 in Nijmegen are typical for this type of settlement. Although similar sites have not been found elsewhere in the eastern river area, they are known to have existed in adjacent regions during the Early-Roman Period, for example, in Moers-Asberg-Asciburgium and, perhaps, in Vechten-Fectio. There are, however, a number of sites where military vici could have existed, such as part of site 126 (possibly site 23) and site 194. It is not known if the vicus in Cuijk (500) already existed next to the early fort (499), but archaeological investigations outside the area of the fort have been very limited. For site 117/8, if it was indeed a fort, the presence of an early vicus (118?) will be difficult to establish without extensive excavations because of the long pre-Roman occupation of the site.

Chronology, location, and finds
Apart from the observed, expected, or possible structural differences between early-Roman settlements, which are rather meagre, the chronology and location and also the finds are other important clues for a possible differentiation. On the Holocene deposits, there is no significant difference between late-Iron Age and early-Roman settlements, with the exception of the Rhine-branch between a point south of Groessen (c. 198/436) to a point west of Meinerswijk (c. 187/442), which only became properly settled from the beginning of the Roman Period onwards, and thus presumably under the impetus of military demands. As discussed in chapter 3, this branch was just barely or not at all habitable during the Iron Age. Therefore, the few new and non-military settlements there could be different from the others, either as 'pioneer' settlements or because they started as a spontaneous or induced response to the establishment of military camps.

It is, however, questionable whether these settlements were any different from other native sites except, perhaps, as far as their legal status was concerned. There is only one settlement (23) which may have existed in the Early-Roman Period and which probably started as a direct result of the Roman military presence. Some of the finds are definitely pre-Flavian, but not necessarily datable to IA. They consist largely of wheel-turned pottery and the settlement is located very close to the military establishment at site 126. Although the two sites are located on opposite sides of the present-day Rhine (see fig. 61 and especially fig. 93), both were probably situated south of the Roman Rhine channel. If site 23 did exist in the Early-Roman Period, it could be the vicus of the early-Roman fort of Meinerswijk, or perhaps a wharf such as Valkenburg – De Woerd in its early stages. It is conceivable that spontaneous settlement in this area was not encouraged or even allowed by the military authorities. After all, the banks of the Rhine may have been military territory. In this respect site 132, on which only

71 Bogaers 1955, 42–3 and 174–5. Recent analysis of the bone material by R. Lauwerier has confirmed Bogaers' preliminary opinion on this subject. See JROB 1983, 33.
72 See De Boe 1977. Location indicated on fig. 80.
73 Fectio: Bogaers/Rüger 1974, 62–5, with further references; Van Tent 1976, 61–4; Kalee 1980; Asciburgium: Borchert 1974, 183–9, esp. 188.
74 Bloemers/Sarfatij 1976. See also below, p. 269.
native-Roman hand-made pottery was found, is interesting. It could well be that a native settlement which developed there 'in an unguarded moment', was not allowed to continue. The absence of early wheel-turned pottery is, however, perfectly understandable as long as a collection of sherds from a site is not very large indeed. Although the collection of site 132 is large enough to conclude that the ceramics are native-Roman and not late-Iron Age pottery, the material was recovered from a very restricted area and it is not possible to conclude that wheel-turned pottery is definitely absent.

As far as Pleistocene deposits are concerned, it is evident that the military establishments on the ice-pushed ridge in Nijmegen represent a completely new choice of location. The area is not very suitable for normal settlements, which are indeed lacking. The occupation of site 403 (and 417) can hardly be called normal, or indeed entirely (if at all) civilian. With regard to the coversands and loess, the real or seemingly near absence of occupation applies to all periods. The choice of location thus offers only a few clues for the differentiation between settlements, and the same is true for the chronology. In the case of the Nijmegen sites and also for Elst, the chronological data are related to different and new types of settlements. But in general such a connection to settlement type is impossible to establish. If there were at all Iron Age settlement types which did not continue throughout the Early-Roman Period, which is hardly likely because the period is too short, the available material evidence can only be used for differentiation in one direction, namely, to identify new settlement types which are not native.

As already mentioned earlier, the available non-native finds from settlements in the river area are very scarce, which agrees with the theoretical expectations (cf. p. 232). The only clear exception, site 117/8, has been interpreted as a probable early military camp. When the other sites are considered, a chronological differentiation has to be made first, because not all material is equally precisely datable. As can be deduced from chapters 4 and 5, there are 7 settlements on which definitely Augustan material was found. These are 3, 142, 234, 244, 300, 310, and 515. The recently discovered site 544 (see chapter 7.1) should also be included in this list. In addition, there are another 13 settlements with material which could, in most cases, be Augustan, but which is at least datable to IA, namely nos. 43, 48, 93, 95, 96, 100, 105, 123, 150, 151, 299, 356, and 362. Finally, there are 15 settlements with certainly pre-Flavian finds which could be datable to IA. These are sites 23, 62, 75, 135, 140, 214, 232, 239, 250, 298, 315, 341, 459, 487, and 500.

The nature of the finds in question is not really relevant, because there are almost always only one, two, or three artefacts involved. These are always potsherds, in a few cases supported by a fibula, or one or a few coins. The presence of these rare items, when considered for each site separately, can be easily attributed to chance. It is quite well possible that some early-Roman pottery landed in every settlement which existed at that time and an identifiable fragment may be accidentally discovered on every one of them. Whether or not such a fragment has any special significance can only be evaluated at the regional level where the distribution pattern and increase of early finds through time can be observed. The latter phenomenon is easily demonstrated by adding up the figures given above, which results in a total figure of 8 settlements for the Augustan period, 21 for IA and 36 for the pre-Flavian era. Although it is logical that the number of sites with material from a period should increase when the period in question is longer, the growth curve indicated by the figures 8-21-36 is, in a relative sense, still valid. This is because only for site 515 and possibly site 3 do the early-Roman sherds presumably mark the end of the habitation. For all the others they are either fragments of the first imported items in a pre-existing settlement or they date the beginning of a settlement, and in both cases the sites in question existed for a long time afterwards. Therefore, the curve can be taken to reflect the phenomenon of the increasing availability of (Gallo-) Roman material to people living in the region. But this is still a general and intuitively even obvious conclusion. When the total quantity of (Gallo-) Roman artefacts in the area is compared to that of Nijmegen, it is completely insignificant. It is not possible to estimate the real distribution of imports in the area from the curve, but it is evident that the observed increase in the pre-Flavian period is from extremely to very limited only. There is thus a fundamental difference between settlements with virtually only imported material (126, 194, 403, 406, 412, 417, and 499) and with virtually none, the surface collection of site 117/118 being the sole intermediate case.

It is justified to conclude that Roman material trickled
through only very sparingly from the military establishments into the region. The important question in the present context is to establish whether or not any conclusions can be drawn for the sites in the region: is there any chance of differentiating between the sites which did, and those which did not get such material.

In order to assemble a complete picture, all settlements and cemeteries with early imports are presented on a separate distribution map (fig. 68). It is quite clear from the distribution that in general the sites cluster in the middle of the map: around the axis Cuijk-Nijmegen-Meinerswijk. But when the structure of the landscape is taken into consideration, some differences emerge. The eastern part of the Overbetuwe is the immediate hinterland of one or more early camps on the Rhine, and precisely between those camps and the Nijmegen sites, with Elst exactly in the centre. The presence of some early imports in this area is, therefore, not likely to imply that the settlements involved are a special type of site. The same is true for the sites along the Rhine in general. The early military activities took place along two axes: one land route (Cuijk-Nijmegen-Driel/Meinerswijk) and one water route, along the Rhine. From the Overbetuwe, there were at least two routes going further north: one over water along the IJssel and one over land along the line Wageningen-Ede.

Although the Waal and the Meuse, and possible routes over land (see fig. 22 or 75), should not be neglected in this respect, these are the two routes which seem to be the most relevant for military operations and along which the archaeological visibility of some early refuse is not surprising. The same is not true, however, for the eastern part of the Land van Maas en Waal. The concentration of early material on the Pleistocene soils around Wijchen and the older Holocene deposits south of Ewijk is not so easily explained. It is west of the land route from Cuijk to Nijmegen and, although it is attached to the Waal and the Meuse, the concentration is between rather than at these rivers. As far as military operations are concerned, this area has no direct importance. The clear cluster of early and even very early imports is therefore more likely to arise from some intentional and directional process, rather than from an accidental and arbitrary one, even though the proximity to Nijmegen is undoubtedly very important, especially for the sites around Ewijk.

There are at least two processes which could be involved here: either a military one, involving the presence of military personnel in order to control the area, or a civil (or not directly military) one, involving reasons why the inhabitants of some of the settlements could obtain goods from the army. Whatever the real reason may be, if the interpretation of the distribution is correct it implies that certain settlements in the eastern part of the Land van Maas en Waal were of special importance. This observation is in accordance with the conclusions reached for the Late-Iron Age. Although no clear differentiation of settlements for that period was possible, it was concluded (at the regional level) that sites in the area around Wijchen were relatively more wealthy than elsewhere. Their importance may even have grown in the last century BC.

As mentioned at the outset, individual sites cannot be declared special just because an early-Roman sherd was found there. From the foregoing discussion, it appears that for most sites their location goes a long way to explain why such sherds were found on them. For the settlements around Wijchen such an explanation may not be enough, and they could indeed be something special. The apparent continuity of 'relative importance' in the same area supports such a conclusion (if soldiers were actually stationed here, it could be for this reason!), and it also enables the more precise location of the sites involved.

A cross-check with the 'richer' sites from the Late-Iron Age, shows that sites 239, 244, the closely related group 296-302, and possibly site 356 could qualify as exceptional settlements in both periods. The only site in the Betuwe which shows the same combination is no. 43. With the exception of sites 296-302 (especially 302 which, however, yielded no special items), none of these sites has been excavated. The investigations at site 43 were no more than very limited trial (1960) or rescue (1961) operations. All in all, it seems that the early-Roman native settlements can be divided hierarchically into two groups on the basis of their location and finds.

Conclusions
In contrast to the preceding period, the early-Roman settlements in the area show a clear diversification. The evidence is, in fact, such that instead of diversification it is more useful to say that there are two sorts of settlements in the area, which are virtually unrelated from several points of view, such as structure, location and material residue. The one sort can be properly called Roman, while the others are clearly native settlements. The gap between both is large, complete, and unbridgeable.
All this is, in principle, hardly surprising because we are dealing with an almost perfect difference in kind: on the one hand military camps, on the other native villages or farmsteads. The Roman settlements 126, 194, 406, 412, 417, and 499 are military establishments and site 117/118, with a long occupation history and nothing but surface finds, is likely to be one also. The nature of early-
Roman Elst (105) is unknown, but it is clear that the construction of a temple there around AD 50 was also a military affair, possibly related to the construction of the limes after AD 47.

What is remarkable and indeed very surprising is the fact that Batavodurum, the Batavian centre which can be identified as site 403 (or, very unlikely, as site 417), is also completely and utterly different from native settlements. Even though future research may well reveal some non-Roman components at both sites, it is impossible to call them native. The assumed existence of a Batavian capital depends entirely on information from classical sources, and it is doubtful whether anything short of a complete excavation of the entire area (no longer a possibility) or explicit inscriptions could have led to the same interpretation. The conclusion has to be that the Batavian capital was a Roman settlement and an implantation in the area. It did not exist earlier, nor did it emerge from a native central place. It grew out of a military establishment or at least in direct connection with such a site.

Apart from the capital and Elst, the vici around early forts represent a third form of settlement which is essentially military but in which the local population may have taken part. The existence of early-Roman vici in the area has not been demonstrated, but proof is impossible without excavations. If they did exist, there are at least three sites which could qualify, namely, 23 (next to 126), 118 (next to 117), and 500 (next to 499).

Under this entirely new, entirely Roman and to all intents and purposes military superstructure, the native settlement structure apparently continued relatively unchanged. The inhabitants of a few settlements seem to have had more access to rare, important or precious objects. Especially because of apparent continuity of these ‘richer’ settlements from the Late-Iron Age into the Early-Roman Period and their location in the region, this observation does not seem to be fortuitous. Sites like 43, 239, 244, 296-302 and 356 (?) can therefore be regarded as examples of settlements at the top of a very short settlement ‘hierarchy’, which are not likely to be very different from those at the bottom.

As the excavations in Heteren (sites 93 and 95) have shown, the post-hole swarm type of settlement also continued into the Roman Period. The proposed difference in subsistence strategy between such settlements and those with more easily recognizable house-plans thus also continued to exist during the Early-Roman Period.

To sum up, it seems that the profound changes which the arrival of Roman armies must have brought about in the river area are, as far as settlement typology is concerned, only visible in the creation of new, ‘Roman’ settlement types, not in the disappearance or change of existing ones.

8.3.3 Cemeteries

In contrast to the Late-Iron Age, cemeteries from the Early-Roman Period are not absent. As is evident from Appendix 2, the total number is still restricted (a maximum of 17 sites), but that is not surprising in view of the short duration of the period and the fact that cemeteries are not as easy to discover as settlements.

What is surprising, however, is that some of them are clearly not Roman, which means that at least some of the burials of the native population regained the archaeological visibility which was lacking for the preceding centuries. The difference between native and Roman – which in view of the conclusions of the preceding paragraph would seem to be almost the same as the difference between civil and military in the Early-Roman Period – can only be properly evaluated after excavation. With regard to the Nijmegen cemeteries, only part of the large graveyard under and around the present-day Museum Kamstraat (site 409) has been systematically investigated. It consists of a very large number of various cremation-burials, the common practice of disposal of the dead in the Early- and Middle-Roman Periods. In view of its location, site 409 should be seen as the burial-place belonging primarily to site 403 which, although considered to be the Batavian capital, is definitely not a native settlement. The same is true for the cemetery, which, apart from a limited number of pots, yielded nothing specifically native. As long as an encompassing analysis of all Roman Period burial practices is lacking, it is not possible to make definite statements about single burials, but it is justified to conclude that site 409 with its burial pits with Roman pottery and without peripheral structures is a Roman cemetery. Whether or not civilians (immigrants and/or members of the native population) formed the majority of the deceased cannot be decided, but it is certain that they were buried in a way comparable to (Gallo-)Roman cemeteries elsewhere, with Roman grave-goods (often with intelligible graffiti), and very likely on military territory.

The other Nijmegen cemeteries (400, 413, 414, 415, 418, and 419) are probably all smaller than site 409, but there

76 Bloemers 1979c.
Fig. 69 Nijmegen-Hulzen (site 396), excavation plan of the native cemetery, after Bogaers/Haalebos 19803, fig. 1: 1 ditch, 2 cremation burial, 3 post-holes.

is no reason to assume that they are any different. They have never been properly investigated.

In contrast to the data from Nijmegen, a few excavated cemeteries in the region offer a different picture. These are sites 396 and 490, to which can be added a recently excavated cemetery in Oss-Ussen. The data on site 490, which was partially excavated in 1923, are limited. Its size and the period in which it was used as a cemetery are not known exactly. It is certain that site 490 is a prehistoric urnfield with burials from the Early-Iron Age. As mentioned in paragraph 8.2.2, it may have been used continuously throughout the Iron Age, but this cannot be demonstrated with certainty. It is a fact, however, that the latest burial contains early-Roman material, which means that either the archaeological visibility lost after the Early-Iron Age was regained or, also plausible, that an older urnfield was used again. It should be noted that the burials themselves are all more or less alike: not all have pottery, but in no case have the cremations been deposited in a pit and they are all surrounded by a ringditch. There is no doubt that site 490 is a completely native cemetery.

The same is true for the recently excavated site 396. The nearly completely excavated cemetery (see fig. 69) consists of a large number of cremation burials, usually inside round or square ditches which are very close together. The cemetery in Hatert probably started about AD 25, and was continuously used to the end of the 2nd century. The cemetery in Oss-Ussen shows very similar structures and also started in the Early-Roman Period. A detailed analysis of these types of burials may well produce more insight into the social positions of the deceased and the social structure of the native population. In any case, they fall within the tradition of native burial practices. The only really new element is the (re-)introduction of grave-goods, especially pottery in some interments. Most of this pottery is (Gallo-)Roman, although native pots occur as well. In this respect, there is no difference from Roman cemeteries such as that on site 409. Graffiti, however, seem to be largely restricted to the Nijmegen cemeteries. Another important difference between the two sorts of cemeteries is the amount of early pottery. At site 409 it seems that almost everyone buried there in the Early-Roman Period received one or more imported vessels or other items as grave-goods. At the native cemeteries, this was the case with only a few individuals. All other burials either contain nothing but the

77 Site 396: Bogaers/Haalebos 1980a; site 490: Remouchamps 1924; Oss-Ussen: G.J. Verwers 1981. This cemetery is located just outside the area covered by Appendix 2, a few kilometres due west of square 166/419.
78 G.J. Verwers 1981, 36 and a/b. 25-26. The same is probably true for a cemetery near Zoelen in the central river area (pers. comm. R.S. Hulst). Another early burial ground has been discovered in Bladel (Slofstra a.o. 1982, 98-100), on the coversands in Brabant. The Veldhoven cemetery in the same area (see overview by Verhoeven/Ector 1984, 139-40) is probably later.
Apart from the burial sites considered so far, there are a number of other cemeteries indicated on Appendix 2 which have not been properly investigated and of which the interpretation is therefore mostly problematical. There can be little doubt that the tombstone from site 182 (fig. 17) represents a Roman grave or cemetery. Also the cemetery in Cuijk (site 501) is probably a military burial-ground. The other sites can, unfortunately, not be reliably classified. The cemetery at site 336 near Wijchen, which became very large in the Middle-Roman Period, may well have been a native cemetery like Hatert or Oss-Ussen. The cemeteries in Millingen (440) and Donsbruggen (454) cannot be evaluated at all. The remaining sites in Puiflijk (208), Wijchen (324), and Mook (385) are not even certainly early-Roman, although the latter two are at least definitely pre-Flavian.

Conclusions
From the excavated sites, it appears that there is a difference between native and Roman cemeteries. Because none of the excavated sites has yet been fully published, it is only possible to observe the most evident differences between the two. These are to be found in the different structure of the burials (and, by implication, in some aspects of the burial-ritual) and in the presence of Roman pottery as a normal feature or as an exception.

It is not possible to differentiate between military and civil cemeteries, unless these concepts are equated with Roman and native. The latter could well be the actual situation in the Early-Roman Period. It is possible to differentiate between small and large cemeteries. Although early-Roman native burials are only barely detectable, the continuity of early- to middle-Roman burial sites should have drawn attention to early cemeteries which could potentially be large. The only large cemetery ever discovered is site 409, with probably hundreds of early burials, and its location next to the only really large population-centre conforms to expectations.

To sum up, at least four different sorts of graveyards can be distinguished: one large Roman cemetery, several smaller groups of Roman graves, a few native cemeteries which show Roman influences in a very restricted number of burials, and an unknown number of completely native burial-places. All that has been said about late-Iron Age disposal-of-the-dead practices applies to the last category. Future research will undoubtedly reveal additional types of cemeteries or produce refinements to this rather crude differentiation. At this mo-

79 For an introduction to the theoretical concepts involved here, see Willems 1978, esp. 84-8.
8.4 THE MIDDLE-ROMAN PERIOD

The evidence presented in chapters 4 (fig. 23) and 5 shows that a maximum of 293 settlements existed during the Middle-Roman Period. Of this total number, not more than 38 sites are only probably or possibly Middle-Roman settlements. This means that the number of definite middle-Roman settlements is 255, which is considerably more than the numbers of definite late-Iron Age or early-Roman settlements, which are 155 and 142, respectively. This difference is, of course, primarily due to the finds which are, in contrast to the preceding periods, both abundantly present and accurately datable. Nevertheless, there is also a clear absolute increase in the number of settlements. As far as the total number is concerned, there will undoubtedly be some undiscovered settlements in the river area proper but these should be a negligible quantity. There may, however, have been a number of sites on river banks, which have been eroded. Also at least some of the Pleistocene deposits have surely been much more densely settled than Appendix 3 or fig. 62 show.

For the coversand-areas south of the Meuse in Brabant and along the western slope of the ice-pushed ridge south of Nijmegen, this is already a certainty. These two areas alone are likely to have had several dozens of settlements, as is demonstrated by the cluster east of Middel-laar, which was discovered by systematic field-walking in just a few years, and also by at least 19 known sites from Brabant which could not be included in the catalogue and Appendices. Virtually nothing is known about the loess-deposits between Nijmegen and Kleve, most of which are covered by the Reichswald. A really dense occupation there would probably have yielded more sites than the few which have been found so far, but the inaccessibility of the terrain for archaeological observations may well have left a considerable number of settlements undiscovered. The same is not true for the Montferland and the area north of the Rhine, not so much because these regions are beyond the limes but especially because large portions of them are less suitable for habitation. Given these rather general and vague considerations, it is impossible to reach a precise and reliable estimate of the original number of settlements. There are, however, certain limits which can be proposed. On the basis of what has been said so far, it can be taken for granted that at the very least about 100 settlements have either been eroded or, more importantly, are yet undiscovered. On the other hand, it has been established that, for example, in the most densely settled loess areas of the Kreis Berg-heim (Germany), the density of settlements approaches 1 site per km². The average is certainly less than that. Comparable figures of 1 site per km² after intensive field-walking were recorded in the Thames Valley (England).

Approximately the same can be said of the settlements on the Holocene river-deposits of our region. On the basis of data like these, and in view of the very diverse nature of the Pleistocene deposits compared to the loess and river clays, it is very unlikely that the maximum number of unknown settlements is more than 250. The theoretical upper and lower limits of the original number of middle-Roman settlements can thus be established at 350 and 500. For reasons to be discussed below, the latter figure is to be preferred. This means that the 255 sites to be considered here represent approximately 50% of the original number, which is a quite satisfactory rate of recovery.

It also means that the settlements which are available for analysis probably include the total range of variability. It remains possible that certain specific settlement types escape attention by processes of selective destruction or coverage, but it is very unlikely that an important settlement type can be overlooked. One example are the definitely selectively destroyed frontier-forts on eroded parts of the southern bank of the Rhine. These have still left many direct and indirect clues to their existence and location. Besides this, the chance of not noticing differentiation for this reason is minimal, compared to the overall difficulties in tracing settlement types, especially where this has to be done on the basis of surface finds only.

Fortunately, that is not necessary for all sorts of settlements. There is some historical information about specific sites as well as on types of settlements, and there are

80 Bergheim: Hinz 1969, 57 and Taf. 50 or 54; Thames Valley: Miles 1982, 63.
81 See paragraph 11.4.2, p. 416 f.
also quite a few sites of which sometimes considerable parts have been excavated. Unfortunately, virtually none of the most important excavations (those of the last 15 years) have yet been published completely, which means that detailed chronological and especially structural data on these settlements are not yet available. On the other hand, the preliminary reports usually give at least an impression of the structures and kinds of settlements involved. These excavations are of vital importance as points of reference in the discussion of differentiation between settlements.

Just as during the preceding period, there is a basic dichotomy between civil settlements and military establishments, which were necessary for the control of the frontier. And because of the definition of the Middle-Roman Period, which starts with the fixing of the frontier along the Rhine and ends with its disintegration, this dichotomy lasts during the entire period. From a chronological perspective, there are variations in the structure of the military settlement system as a whole and also in the development of individual sites, but except for a legionary fortress all different classes of military settlements are continuously present.

8.4.1 Military Settlements

Although a reasonable amount of data exists about military establishments in the eastern river area, these are not the primary source for discussing the different sorts of sites. The Roman military presence may be adapted to specific circumstances, but its organizational aspects are not specific for the region as such. They are determined by the overall structure of the Roman army and by the form in which that was expressed in the northwestern provinces. In order to gain an overview of the types of military sites which are or could be present in the eastern river area it is more profitable, therefore, to look at the range of possibilities first.

In addition to the legions the Roman army consisted of auxilia. The composition and organization of auxiliary troops changed with the passage of time, but they were organized mainly into infantry and cavalry regiments of nominally 500 men, the cohortes and alae quingenariae. Units of double size, the cohortes milliariae and - outside Germania Inferior - a few alae milliariae, also occurred, as well as a mixed form of infantry regiments with additional cavalry units of about 120 men, the so-called cohortes equitatae. There are several other auxiliary units, such as the irregular tribal troops (tumultuarii) which were especially important during the Early-Roman Period, and the later numeri and cunei, which seem to have been special task forces at a time when the regular auxiliary units became more and more indistinguishable from the legionary forces.

Although auxiliary troops were attached to legions, their main task during the Middle-Roman Period became the control of the frontier. Units were stationed in forts (castella) along the limes. Not all these forts were alike, their size depending on the strength and type of unit which constructed the fort. For example, the cohors II Brittonum milliaria equitata in Vechten needed a fort of c. 2.7 ha, while the fort in Zwammerdam (Period 3) measured only c. 1.2 ha, which implies that it probably housed only a detachment (vexillatio) of, in this case, a cohors equitata.

This last observation and, for example, a succession of forts such as found in Valkenburg, also indicate that the forts were built to fit the unit using it and not to a prescribed universal format. Therefore, the actual situation is that of an almost continuous size-distribution of forts. The same is true for legionary fortresses, but there is still a gap between relatively small fortresses, such as that in Nijmegen (period 4) which measured c. 15 ha, and the larger frontier forts. At the other end of the scale, there seems to be a similar gap between the castella and even smaller military establishments.

The limes system was not intended as a Maginot line, a defence against major attacks. It could cope with smaller incursions but it was mainly intended as a baseline for controlling the area beyond the border and as a supporting infrastructure for the forward defence, that is, offensive operations to intercept large-scale attacks beyond instead of at or even behind the frontier. In such

83 Although the regional level is too low for an evaluation of military settlements, for the empire as a whole it is too high because there were all sorts of variations in its different parts. Even between provinces as close together as Britannia and Germania Inferior and Superior, there are clear differences (Frere 1978, chapter 11).

84 Kraft 1951; Alfböldy 1968, chapter 3; Sebesta 1972, part 1; Holder 1980, chapter 1 and conclusion.

85 Haalebos 1977, 87.


87 Even this gap is closed in some instances, e.g., by the so-called ‘20–30 acre’ fortresses in Britain (Frere 1978, 253–4), although auxiliary forts there tend to be rather small again.

a system of defence, communication and transport networks are vital, which implies an adequate road system and a signalling system between the forts and fortresses. The *castella* were built at more or less regular intervals, but the exact location was also determined by the structure of the landscape. For surveillance purposes, and also because communication took place by fire and smoke signals, intermediate posts and watchtowers were necessary additional elements in the *limes* system. They are at the lower end of the size-scale and a few examples from Germania Inferior, such as the fortlets of Rheinhausen—Werthausen (c. 0.17 ha) and Neuss-Reckberg (c. 0.11 ha) are clearly different from even the smallest *castella*. The watchtowers are even smaller, of course.

As far as the typology of military settlements is concerned, there are only two additional types which should be mentioned. These are, first, the so-called *stationes beneficiariorium consularis*, which were posts along important routes and in communication centres. They were manned by detachments from legions in the province, under command of a *beneficiarius consularis*, and their main function can be described as police work. Unfortunately, the presence of *stationes* is known mainly through inscriptions from *beneficiarii*,*89* not through excavations.

According to MacMullen, the detachments lived in their own tents, in private houses, or in unfortified *mansiones*. The last remark is important because it distinguishes middle-Roman road stations from the late-Roman examples, which were always fortified, but also because it illustrates why it is still largely unknown what they looked like. They will be difficult to identify when one of the elements which are usually characteristic for a military site, namely, its defence-works, is absent. To some degree, the finds may offer a clue. *Stationes* in the hinterland can, perhaps, be recognized from the presence of military tile-stamps, but for the region immediately behind the frontier this alone is not sufficient, as witness the finds from the eastern river area (paragraph 6.5.1). It seems, therefore, that identifications of settlement sites as *stationes beneficiariorium consularis* can only be tentative as long as excavations are lacking, and even then the nature of the sites is not immediately obvious.*90*

The confusion about road stations is also illustrated by the excavation of a military establishment in Rheinberg, where a central wooden building was surrounded by a ditch. For this reason, it should not be regarded as a possible *Benefiziarierstation* but as a fortlet, watchtower, or signal station.*91* A second reason is the location of the site: in isolation along the *limes* road. Apart from the issue of defence works, small isolated military sites along the *limes* should, for reasons of terminological clarity, not be referred to as road stations. Their location implies a primarily military, not a police function. *Stationes* along the *limes* are only understandable in combination with fortresses, forts, or towns.*92*

The second and last type of military settlement which should be mentioned, is the naval base. The Rhine fleet (*classis Germanica*) is one of the better documented fleets of the empire.*93* Its headquarters were probably at Alte- burg near Köln, and there must have been other stations further downstream. Examples are Vechten and a (major?) harbour in Katwijk, but it is questionable if any specific naval stations existed, at least along the rivers. Vechten was also — indeed primarily — a frontier fort, and it is likely that most if not all forts along the Rhine had a landing-stage.*94*

To sum up, it can be said that five or six different types of military settlements have to be reckoned with: legionary fortresses, auxiliary forts (probably also serving as naval stations), fortlets, watchtowers, and road stations. These should be identifiable as military sites on the basis of the finds and, except for the road stations, by their structure, especially the defence works. Unfortunately, the latter criterion is only useful after excavation, which means that in several cases the finds are the only indication for the fact that a site may be military. It also means that it may not be possible to indicate exactly what kind of military site is involved. It is hardly likely that a legionary fortress would not be recognized as such, but the ancient settlements soils in the river area are not necessarily indicative of the size (which could point to the

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90 An example is the identification of the *statio* in Köln by Von Petrikovits 1960, 75–6.
93 See Starr, 1941, 141–52 for an account of its history.
type) of the settlement involved. The recognition of unexcavated military sites is further obscured by the fact that they are normally associated with non-military settlement(s), the canabae legionis and the military vici.

These have been excluded here, although they were in some respects also military sites. In practice, however, they were inhabited by civilians who came to live there as a spontaneous, but more often probably induced, response to the presence of a fort and the various possibilities which that offered. For this reason, the settlements associated with forts will be discussed below, together with the other settlements in the river area.

Although there are several difficulties in identifying unexcavated settlements as military establishments of some sort, the inventarisation of findspots in the eastern river area has produced several additions to the already known or assumed military sites, some of which are (partially) excavated. In order to present the available data in a coherent way, all these sites will be discussed separately, within the above-mentioned typological framework which is, in part, also a hierarchical ordering.

Legionary fortress

After the end of the occupation of the large early legionary camp at site 412, presumably in AD 16, there are no definite traces of a fortress in the eastern river area until one was built at the same site in AD 70. The current assumption is that no legion was stationed in Nijmegen between AD 16 and 70. There are, however, quite a few as yet uninterpreted and/or unreliably datable ditches on and around the Hunerberg, as well as two sites (403 and 417) which were occupied until c. AD 70 and which are very unlikely to be entirely – or at all – civil settlements. Pending further excavations, it is quite well possible that part of a legion was stationed in Nijmegen under Claudius and Nero on any of these sites. The fact that no less than two legions, the legiones V Alaudae and XXI rapax (which was replaced by XV Primigenia around AD 46) were stationed in nearby Xanten-Vetera until 69 offers the possibility of envisaging legionary detachments in Nijmegen. At least the legio XV Primigenia is also attested to by tile-stamps (fig. 50) and the evidence for military presence in Nijmegen before AD 70 is certainly too extensive to attribute it to a simple statio. All this is, however, no more than a possibility. It is equally well possible that Nijmegen was occupied by auxiliary units, such as the Batavian cohorts and ala, which may have been located in its own tribal area before AD 43 and, partly, also afterwards. In fact, sites 403 and 417 could well have started as camps for auxiliary troops on both sides of the legionary camp (412) and have remained in use after the departure of the troops stationed in that camp.

Whatever the solution of these problems may prove to be, it is unlikely that full-fledged legionary fortresses were located anywhere except on site 412, and the first tangible one is period 4 of the local sequence, occupied by the legio X gemina. The fortress was occupied by a succession of different troops from AD 70 until IIId, but with the possible exception of the early-Flavian or still older phases, it never housed an entire legion. Both periods 4 (c. 1.5 ha) and 5 (c. 16.5 ha; see fig. 77) are too small, which implies the permanent absence of one or more legionary detachments. For the other units which later occupied the fort, the vexillatio Britannica and a detachment of the 30th legion, it may have been too large, but apparently it was never reduced in size.

Auxiliary forts

Outside Nijmegen, there are no less than nine forts or possible forts in the area covered by the map, most of which are mentioned here that the stamps of the legio V Alaudae recorded for Nijmegen by Rüger (catalogue nos. 18 and 20) are unreliable (pers. comm. J.E. Bogaers).
them located along the Rhine. Only tiny parts of a few sites were ever excavated, so that many uncertainties arise. Nevertheless, the picture of this part of the *limes* as a whole is emerging clearly, and it is quite certain that the old and very unlikely idea of an unfortified Batavian *limes* is not correct. The evidence employed to identify (or reconstruct) sites of forts is varied: in the absence of sufficient data on size and structure, locational factors and finds are the primary sources of information. The classical sources are, unfortunately, of very little value in this respect. There are at least three good reasons why they can only be used afterwards, as secondary sources to be contrasted with the combined archaeological and geological data.

First, historical data such as distances between forts are not always unambiguous and they have occasionally been shown to be wrong. Second, in the absence of clear traces of Roman roads, it is not possible to calculate distances exactly, even though the essentially reliable reconstruction of the geological situation in the Roman Period, which is now available, offers reasonable compensation. Third, the number of fixed points (identifications of sites with names from classical sources which are beyond any doubt) is very low indeed. Even for Nijmegen-Noviomagus, which certainly is a fixed point, one can never be sure from which site the distances were calculated (sites 399 and 412 are almost 3 km apart).

For these reasons, historical data on specific sites provide information which cannot be used independently. This does, of course, not imply that they do not add valuable information once an acceptable identification has been reached. The mere fact that a site is mentioned at all on a map or in a book or itinerary may say something about its importance or nature, which may not yet or even never be archaeologically traceable. In the eastern river area, the following sites are possible locations of auxiliary forts.

*Site 37 Kesteren*

Kesteren is generally considered to be the fort Carvo(ne), mentioned on the *Tabula Peutingeriana* and in the *Itinerarium Antonini*. The available evidence has been compiled by Bogaers and shows that a military establishment in Kesteren is indeed very likely. As mentioned in the catalogue, the location of Carvo cannot be site 38, which was partially excavated and shown to be a settlement. Both sites 38 (a *vicius*) and 39 (a cemetery) have yielded enough military finds to assume that a military settlement must have existed in the immediate vicinity. Because there is no other settlement which could qualify as such south of the dike (*Rijnbandijk*) the conclusion must be that the missing fort was located on the southern bank of the Roman Rhine to the north of the present dike, an area which was eroded in post-Roman times. The fort and, incidentally, also the northern part of site 39, are therefore likely to have been washed away and site 37 is a hypothetical site only, even though its existence need not be doubted.

There is no real clue to the nature of the military establishment near Kesteren. Bogaers, and also Van Es, have considered the possibility that it may have been nothing more than a station of *beneficiarii*. This is based on a sherd with graffito *BE†eFICARIORVM* found on site 38. This could imply that site 38 is or includes a road station which would explain both the military finds and the relative lack of native material and, consequently, even remove the need to postulate site 37.

This is, however, rather unlikely because the presence of a station would normally presuppose rather than exclude a fort and it is difficult to see how an unprotected road station would fit into the *limes* system. Site 38 does not qualify as, for example, a fortlet or other intermediate frontier-post. Therefore, it remains very likely that the specifically military settlement in Kesteren is indeed missing and therefore eroded. It does not necessarily have to be an auxiliary fort, unless the identification with Carvo is correct. This is because it is improbable that a relatively insignificant element in the *limes* system would have been mentioned on the Peutinger map or in the *Itinerarium Antonini*. Although there is no reason why the generally accepted identification should be questioned, it is certainly not beyond all doubt. The available data on its location: 8 *leugae* (17.5 km) east of Levefanum (*Rijswijk-G.*) and 13 *leugae* (± 29 km) west of Castra Herculis (*Meinerswijk?*) or 22 (± 49 km) west of Harenatum (*Rindern?) are only sufficient for an approximate localization of Carvo. All that can be said further is that, in view of the geological possibilities, the distances and identifications do not disagree with each other.

*Site 18b Randwijk*

As shown in paragraph 3.4, one of the most important...
north-south routes through the river area runs from Herveld over Zetten to Randwijk and crosses the Rhine at Lexkesveer, after which there are three ways further north: one along the brook of the Heelsumse Beek, one along the Renkumse Beek, and a third route along the border of the high-lying coversand from Wageningen towards Ede. Especially the latter one seems to have been important during the entire period covered by this study.

This route not only crossed the limes road along the Rhine at Randwijk-Lexkesveer but, as proposed in chapter 3 (p. 68), it had itself probably military importance. This importance is rather obvious in the context of the forward defence system, even without specifically military finds or a direction towards an outpost-fort such as perhaps in Ermelo, but it is only an additional argument for attaching special significance to the area around Randwijk: no doubt any route crossing the Rhine, and thereby the Roman frontier, would have been guarded. In view of the archaeologically visible importance of the route in question it is, therefore, necessary to propose the presence of a military settlement of some importance also. The fact that its assumed location is virtually midway between Kesteren and Meinerswijk provides further support for this argument. A comparable situation is found in Cuijk (site 499), where the highway from Tongeren to Nijmegen crossed the Meuse and was guarded at first by a fort, later probably by a statio. But this crossing, located in the hinterland, was not part of the limes system. The crossing at Lexkesveer was part of it, and the need for troops here, both to guard the crossing and as a link in the entire chain of forts, would have been permanent.

Taken together, the analysis of the landscape, the habitation pattern, and the military situation indicate the presence of a military establishment near Randwijk, which must have been at least a fortlet, but more probably a fort. In view of this conclusion, there are two facts which need to be discussed. If there must have been a fairly important fort, then why has it left no archaeological or historical traces of its existence? The first of these is the most easily accounted for. The geological survey of the area around Randwijk indicates the presence of substantial post-Roman (channel-fill) deposits, occasionally as far as one kilometre south of the dike. With the exception of a very small area around the church in Randwijk, the Roman deposits seem to have been eroded, and therewith all traces of habitation. This area is not indicated on the Appendices because it is very small. It must be at least a deposit from the Roman Period because it was suitable for habitation in the Early-Middle Ages (site 71). Unless site 71 eventually proves to contain more than is known today, it is very likely that the military settlement near Randwijk was washed away and the remains now lie deeply buried under later deposits. This is, in fact, the same situation as in Kesteren and also in the Loowaard and Bijlandse Waard.

The only finds which may be associated with the proposed fort are one (or two) tiles with a military stamp, possibly a few sherds, and presumably secondarily used Roman building stone, all found on the Westberg in Wageningen, directly opposite Randwijk (site 18a). It is conceivable that this material was robbed from the fort before it was eroded, but that is merely a possibility which proves nothing.

As far as historical data are concerned, the absence of references to the fort is curious, at least when it is assumed that the current identifications of other forts are correct. If the reported distances are more or less reliable and correctly interpreted, it is not well possible to identify Randwijk with Carvo (instead of Kesteren). But then it has to be explained why a fort in such a prominent geographical location has not been indicated in the surviving sources, while others are. There are too many uncertainties involved to warrant a serious discussion of this problem (if it is indeed a problem!), but the absence of an obvious identification is not in accordance with expectations resulting from the discussion presented above.

There is, however, another source which could, to some degree, support the arguments for a fort near Randwijk, and that is the toponomy. Randwijk belongs to a group of very old place-names, with -wijk suffixes, which includes Poederooien (Podarovic), Vreeswijk (Fresiounuic), Zandwijk (Sandewioic), Wijk (bij Duurstede)
(Uuic), Rijswijk-G. (Riswic), Slijk Ewijk (Etucic), and Meinerswijk (Meginhardisvich).\textsuperscript{107} All these names are known from the early-9th to early-10th century. The origin of wijik is probably the Latin vicus and the Early-Medieval meaning of the term is primarily ‘emporium’.\textsuperscript{108} In Wijk bij Duurstede, and perhaps some of the others, wijik has a slightly different meaning according to Blok and Koch. However this may be, it is probable that these names refer to fairly important medieval settlements. Such sites often turn out to be directly related to important Roman sites, such as forts, and at least two of the above-mentioned settlements, Rijswijk-G. and Meinerswijk,\textsuperscript{109} belong to that category. Randwijk could thus be another case where the presence of an important early-medieval settlement is, as it were, caused by the location there of a valuable piece of property (for the king or nobility) in the form of the remains of a Roman fort.

\textbf{Site 117/118 Driel}

As was also noted by Modderman,\textsuperscript{110} the channel deposits between Driel and Elst and from there to Nijmegen provide an excellent north-south route through the river area. Although these deposits can no longer be regarded as belonging to the main Rhine channel in the Roman Period, a small branch of the river was still active between Driel and Elst. The Drielsche Veer is a crossing of the Rhine which may go back as far as the Roman Period, but there is no apparent route of any importance going further north.\textsuperscript{111} Nevertheless, Driel is at least a point where an important land and water route to the hinterland reached the Rhine frontier. Although there was a fort in Meinerswijk about 3.5 km to the east, Driel is a spot which is likely to have had a military occupation itself. It is very curious that the suggestions put forward by Modderman, who reasoned along the same lines, never received the attention they deserved. He was even able to identify a site which could qualify as a military site because of the relatively large amount of Roman material known at that time. This site is identical to our site 117, which is located at the confluence of the secondary river branch coming from Elst and the major channel of the Roman Rhine. Both the general and the precise locations are thus indicative of an important and probably, at least partially, military settlement. Because there has been only very little erosion of deposits in post-Roman times at this spot, the settlement (sites 117 and 118) has survived intact. In fact it has, so far, escaped almost all imaginable modern destructive activities as well. Most of it became grassland shortly after 1945 and part of it is now covered by greenhouses. The detailed soil survey\textsuperscript{112} indicates that it was quite large: about 8.5 ha. This could well be the approximate size of the settlement, because the entire area has almost constantly been surveyed over the past 40 years and Roman material was found almost everywhere. Moreover, although at least site 117 was occupied from the Middle-Iron Age onwards, the entire site was never fully occupied after the Middle-Roman Period. There are only a few Merovingian sherds which could point to a small settlement, and some occasional later material, which is completely insignificant. It is probable, therefore, that the size of the ancient settlement soil is directly related to the size of the Middle-Roman settlement.

Although exceptional, the size of the settlement itself does not allow any conclusions to be drawn about its nature. It is large enough to have contained an auxiliary fort and associated vicus but, in the absence of excavations, nothing can be said about the structure of the settlement. Fortunately, there are at least a few clues in the rather large number of finds. Most of these are surface-finds, although some material was collected after occasional digging activities by farmers. As far as pottery is concerned, it is important that site 117/8 has yielded predominantly Roman or Gallo-Roman wares. The general ratio between Roman and native pottery is c. 4:6, but the latter category appears to consist mainly of late-(and some middle-) Iron Age pottery. For the Roman Period, the percentage of native pottery is therefore quite small. As far as Roman wares are concerned, the

\textsuperscript{107} See Blok/Koch 1964, 50 and Blok 1965, 53.
\textsuperscript{108} For an overview of the relevant literature and theories, including a new and different one, see Schütte 1976, 141–64 and 196–7.
\textsuperscript{109} Rijswijk (G.): see Van Es 1984; Meinerswijk: see chapter 9. Note that in this and the following chapters there are two different places called Rijswijk, one (the fort) in the Province of Gelderland, on the Rhine east of Wijk bij Duurstede (Rijswijk-}

G., see fig. 130), and one in the Province of Zuid-Holland (Rijswijk-De Bult, see fig. 80).
\textsuperscript{110} Modderman 1949, 74–5, sub I.
\textsuperscript{111} Compare, however, chapter 3, 68, note 207 and see Hedinga 1984, 249, 263. See also Holwerda 1931 on the finds of site 125.
\textsuperscript{112} Zegers/Zandbergen 1958.
The tile fragments are fairly small, and none of them bears a stamp. Most of the other building material (fragments of tubuli and tuff) are also rather small, with the exception of a number of tuff blocks from a hole which was dug at site 117 in 1980. These include a broken but complete block of c. 63 x 63 x 24 cm, with a slightly conical hole in the middle (fig. 70; fracture not drawn). The hole is somewhat worn out to one side, and no longer completely round. It is evidently a pivot-stone, with something turning in it on one side only. Although the central hole is rather large (the diameter is 18 cm) this makes it likely that the stone served to hold the pivot of a door. It may have been broken deliberately to remove the door.\footnote{113}

In association with the tuff, four fibulae were discovered (fig. 71). No. 1 is a large wheel-shaped plate fibula with rich polychrome enamelling, partly in millefiori technique. It belongs to Ettlinger's type 42.3.\footnote{114} No. 2 is a hinged plate-fibula with openings and plastic 'trumpet'-decoration and no. 3 should probably also be called a hinged plate-fibula, although its shape looks like a mixture of certain types of enamelled bow-fibulae and knee-fibulae.\footnote{115} Of special interest is no. 4. It is a tinned knee-fibula with spring-cover, a bow which is trapezium-shaped in section and a transverse pin-catch. According to Böhme\footnote{116} this sort of fibula is almost exclusively found in forts (on the Upper German and Raetian \textit{limes}) and can therefore be considered a typical soldier's fibula. The same is true for the fibula no. 2.

As long as excavations are lacking, all these finds are insufficient to prove beyond doubt that site 117/118 is definitely a military settlement. But especially because they are mostly surface finds, they certainly support the proposition that it is. The presence of stone buildings and a large amount of rather precious pottery at such a prominent location along the \textit{limes} is, in itself, considered to be quite convincing. The absence of military tile stamps and equipment is, considering the find circumstances, not really significant. The only somewhat disconcerting fact is that as yet no graffiti have been noticed on the pottery, which are to be expected at a military site, especially when so much terra sigillata has already been recovered.

large number of terra sigillata sherds (over 200) is remarkable. They include many very small fragments and therefore point to some collecting bias but, on the other hand, such quantities have never been found on the surface of other settlements in the river area and, in most cases, not even during excavations. Also the presence of 'fine Nijmegen' ware, which seems to have been made primarily for the army, is worth noting.

\footnote{113} This is apparently the only practical method. For a discussion of the technical aspects of pivot-stones, see Gillam/Mann 1970, 3–5.

\footnote{114} Ettlinger 1973. For an almost identical specimen from Weissenthurm, see Eiden 1983, \textit{Taf.} 152, 1.

\footnote{115} No. 2: Böhme 1972, type 46c; no. 3: Ettlinger 1973, type 42.3, but see Van Buchem 1941, Pl. 14, 16–24 and Pl. 17, 20–5 respectively.

\footnote{116} Böhme 1972, 20–2, type 21b.
On the other hand, some of these features occur in the immediate vicinity, at sites 120 and 125. Site 120 is the findspot of (part of) a flagon bearing a graffito (fig. 72). It is an offering to an unknown deity from a certain Sallios, who belonged to the turma of Caius. A turma is a small cavalry unit, usually part of an ala. The metalware from site 125 is also undoubtedly of military origin. The interpretation of this find is dubious: it may be a hoard, lost booty, or also represent one or more offerings. But its military origin is hardly disputable, and it may not be mere chance that the finds can also be connected to a cavalry unit. 117 Both finds could mean that the settle-

117 Cf. Holwerda 1931. The graffiti M. Mutteni could point to a cavalry officer having several horses.
Fig. 72 Inscription on a smooth ware flagon from Driel (site 120). The text should be read (cf. J.E. Bogaers) as *Sallios C(ai) turm(a) s(olvit) m(erito)* and translated as 'Sallios, from the turma of Caius, has [by offering this flagon to the (unknown) deity] fulfilled [his vow], with reason'.

In Driel is the cavalry fort which Holwerda assumed to be the place of origin of the finds from site 125 or that a *cohors equitata* stayed there. It is, of course, not at all necessary to assume that site 117/8 is the location of a complete auxiliary fort. A smaller intermediate fort or even a fortlet, in combination with a fairly wealthy commercial settlement, is just as possible. It may be difficult to locate the actual fortlet without extensive excavations, although the surface finds indicate that the military settlement is most probably located on site 117 (the parcels known as Baarskamp, Lizesland, and Ruttens Hofstede). This is why site 117 is indicated as a (probable) fort and 118 as a settlement, presumably the associated *vicus*. Both sites were occupied during the entire Middle-Roman Period.

**Site 126 Meinerswijk**

The evidence concerning the fort in Meinerswijk is presented in chapter 9. The existence of a fort at site 126 was confirmed by a small excavation carried out especially for that purpose in September 1979. Before that time, it was only a hypothesis, based on some locational aspects and on the finds. This is a situation which is more or less comparable to the present state of affairs for the three sites in Kesteren, Randwijk, and especially Driel. Although this does not imply that the hypotheses on those sites are therefore also correct, it does supply additional support for them and it certainly proves the value of the assumptions involved. It is worth while, therefore, to outline the circumstances which led to the excavation in 1979.

Site 126 was discovered because it was indicated as an ancient settlement soil on the detailed soil survey map of the northern part of the Overbetuwe. The accessibility of the site for finding archaeological material was minimal, because it was all grassland (cf. Driel). Finds were collected in the spring, in molehills. They included 109 small sherds, of which only 3 were hand-made native ware. Among the 106 Roman sherds were no fewer than 14 pieces of terra sigillata. Such a low value for native ware and high percentage of terra sigillata are very unusual for sites in the river area (but they are paralleled by site 117/8). The finds also included dozens of tiny fragments of tuff (often with some mortar) and one of limestone, as well as 20 pieces of roof-tiles, none bearing a stamp. After some sounding for stone walls, several were found and one was investigated by means of a small test-pit, which yielded several large blocks of tuff.

The mere fact that site 126 is situated on the bank of the Rhine was, in combination with these findings, enough to interpret it as a probable fort. There are, however, several other locational aspects involved, concerning both the general and the specific location of the site. As far as its general location is concerned, the absence of a direct route to the hinterland makes the presence of a fort not as immediately obvious as in the cases of Driel.
and Randwijk. It is, however, important to note that Meinerswijk is located at the spot where the Rhine turns westwards, and therefore at the easternmost point where the river could be crossed to go north over land.\textsuperscript{119} It is also situated just downstream of the presumed Roman Rhine-IJssel fork, which provides a water route to the north as well. These factors may have played a part in the decision to build a fort in Meinerswijk, especially because that decision had already been taken in the Early-Roman Period. The presence of an early-Roman camp in Meinerswijk was demonstrated only after the excavation, but the sherds from the molehills included at least one early fragment (of Arretine t.s.) as an indication for early occupation.

The specific location of the fort became clear after detailed borings by the Netherlands Geological Survey which followed the discovery of the site (see fig. 18). It is located on the convex bank of a meander curve, and therefore at a relatively high spot. This meander had been cut off, so the fort was located south of the main Rhine channel and probably surrounded to the south by an oxbow lake. The latter may, at the same time, have formed a convenient harbour. It is clear that conditions such as these must have been very attractive, both for a base camp during the early-Roman offensives to the north and for a later \textit{limes} fort.

**Site 135 Huissen**

The evidence for a military settlement in Huissen is rather problematical. Although a small excavation in 1951 produced a fair amount of material, including several military tile-stamps, all of it was found in secondary position, under a medieval \textit{château à motte}, and the same is true for the recent finds at site 546. Bogaers\textsuperscript{120} interpreted these finds as material from a fort of unknown location, but with the assumption that it was in Huissen. The former interpretation is very likely correct, but the latter need not to be true. The Roman material may have been transported to Huissen in late- or post-Roman times, but there is no proof for that either: only the circumstances that possible sites of origin are close (126) or very close (194).

Although the Roman Rhine-IJssel fork cannot be located precisely, it is clear that Huissen was probably nearer to it than either site 126 or site 194. This could be an argument in favour of a military settlement in Huissen, but the presence of site 126 just downstream and site 194 immediately upstream of the fork would seem to be quite sufficient for control purposes.\textsuperscript{121} If Huissen was indeed a military settlement it is, in view of its location and the number of finds,\textsuperscript{122} also curious that so far not a single sherd of early-Roman pottery has been found there. But this is, of course, not \textit{a conditio sine qua non}, and the chances of finding recognizable early pottery are also small.

All in all, there are no convincing arguments for a military settlement in Huissen, but the possibility cannot be ruled out entirely (cf. Appendix 3). Because it is considered very unlikely, Huissen has not been indicated on figs. 6 and 130.

**Site 194 Loowaard**

During the 1970s, extensive dredging started at the eastern side of the Pannerdensch Kanaal, in the Loowaard. In the soil that was brought up, amateur archaeologists discovered large amounts of mostly Roman (but also later) pottery, as well as other material. Examination of

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{Fig_73_Military_tile-stamps_and_bronze_belt-buckle_from_Loowaard_site_194.jpg}
\caption{Military tile-stamps and bronze belt-buckle from Loowaard (site 194). The stamps are read as follows: 1 [XXIIPF], 2 LEGXXI[AC], 3 [r]. Scale 1:2 (Photos 1 and 3, IOGA Nijmegen).}
\end{figure}

\begin{table}[h]
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\hline
\textbf{Site} & \textbf{Loowaard} \\
\hline
\textbf{Description} & During the 1970s, extensive dredging started at the eastern side of the Pannerdensch Kanaal, in the Loowaard. In the soil that was brought up, amateur archaeologists discovered large amounts of mostly Roman (but also later) pottery, as well as other material. Examination of \\
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\end{table}

\textsuperscript{119} On the importance of the medieval ferry between Meinerswijk and Arnhem, see Verkerk 1983, 22.
\textsuperscript{120} Bogaers, in Bogaers/Rüger 1974, 73.
\textsuperscript{121} See also below, p. 260.
\textsuperscript{122} Not all finds could be examined by the present author, but any determinable early sherds would surely have been noticed during the excavation.
these finds immediately showed that the site could not have been a normal settlement and should almost certainly be interpreted as a military fort.

From the composition of the collections of pottery, which contain dozens of completely insignificant wall sherds including sub-recent ones, it is clear that pottery has been collected in an unbiased way. It is remarkable, therefore, that the Roman pottery contains a large number of terra sigillata sherds (well over 100) and, as a peculiarity, an extraordinary amount of smooth ware. In addition, a few sherds of ‘fine-Nijmegen’ ware may also be present but these are in such a bad condition that the determination is not fully reliable. Native hand-made pottery is virtually absent. The few sherds present are probably datable to the Roman Period, although there is one fragment, a large piece of a pot with a handle, which could be an early (!) Iron Age type.123

Even more convincing than the composition of the collections of sherds, is a number of other finds. There is building material such as tuff, indicating the presence of stone buildings, and an ample amount of brick, mostly tegulae and imbrices. Three fragments bear military stamps (see fig. 73), of which only one is clearly legible. It is a stamp of the legio XXII Primigenia. The second stamp is read as LEGIM[ ]AC and could be of the legio XXX, while the third is only a fragment. As far as epigraphy is concerned, the presence of two graffiti on terra sigillata is also important. At least part of the metalware mentioned in the catalogue, such as the belt-buckle (fig. 73.4), is also military. Only one item, an iron spearhead, presumably dates to the Early-Middle Ages and not to the Roman Period.

In view of these finds, there is little doubt that site 194 was a military settlement. Structural details or even a clue to the size of the place are, unfortunately, lacking. It is not even known from what depth the finds were dredged up. Quite a few sherds were, however, somewhat rounded off, so it is probable that the dredging did not destroy a site which was still in situ. It is more likely that the fort and later occupation traces were eroded by a post-Roman Rhine channel and redeposited at a lower level. Fortunately, the original geological situation of the site is quite clear. Although it is now at the eastern side of the river channel, the Pannerdensch Kanaal was dug in the first decade of the 18th century and site 194 is located on the western bank of the previous Rhine channel, as is evident from Appendix 3. The site is approximately midway between Meinerswijk (126) and Herwen-De Bijland (182) and, as already mentioned, probably immediately upstream of the Roman Rhine-IJssel fork.

Another factor which may have influenced the choice of this location is that there was probably a route to the south and west over the channel deposits around Door- enenburg that provided a reliable and direct connection to Elst and Nijmegen. If the reconstruction of the geological situation during the Roman period is correct at this point, the choice of location of site 194 becomes self-evident (fig. 75). If the argumentation is turned around, it can also be said that the presence of site 194 supports the reconstruction. If such a reconstruction had been available before dredging started in the Loowaard, the presence of a military settlement somewhere between Angeren and Groessen could even have been predicted on account of the favourable geological situation.

Site 182 De Bijland

The finds from the Bijland were also discovered as a result of dredging which started here even before World War II. A complete inventory of all finds could not be provided in the catalogue, but a summary and the relevant literature have been published by Bogaers.124 They include ample evidence for stone buildings and also military metalware and tile-stamps (fig. 74) of the legio I Minervia (Antoniniana) and the exercitus Germanicus inferior.

These finds indicate beyond any doubt that site 182 was a military settlement of which, thanks to the fortunate discovery of an inscription on the tombstone of the soldier M(arcus) Mallius (fig. 17), even the name, Carvium,

123 For the shape, see Kooi 1979, 49, no. 861.

is known. Other epigraphical data indicate that the probable auxiliary fort was occupied by the *cohors II civium Romanorum equitata*.

Site 182, which must have been eroded by a post-Roman branch of the Waal, was located near and, for reasons, explained in chapter 3 (p. 53), preferably just downstream of the Roman Rhine-Waal fork. The strategic importance of this location is obvious, but not only because of the water-routes. It should be remembered that the existence of a *limes* road implies a crossing of the Waal very close to the fork. The functioning of the *limes* system must have required adequate means to ensure a reliable way across the Waal during all seasons. These provisions, whether or not they included a permanent bridge or were indeed identical to Tacitus’ *agger*, as proposed earlier (chapter 3, p. 53), needed to be guarded as well.

**Site 450 Rindern**

The finds under and around the church of Rindern have, for a long time, been considered the remains of the auxiliary fort Harenatium (also: Arenatium and Arenacium). They testify to the presence of stone buildings and include military tile-stamps and inscriptions. An overview of the evidence and the relevant literature is provided by Follmann. 125

If the identification is correct—and there are no reasons to dispute that—Harenatium must have been a temporary legionary camp (of the *legio X gemina*) during the winter of AD 70/71. It is also attested to as an auxiliary fort, but it may have ended as a simple intermediate station. 126

The fort was probably located just upstream of the Rhine-Waal fork on the western bank of the undivided Rhine. The geological situation is such that a direct road from Arenatium to Noviomagus, as indicated on the *Tabula Peutingeriana*, is indeed quite plausible. As discussed in paragraph 3.4.2 (p. 69), there are even alternatives for this road: a very direct route over the Pleistocene fluval deposits around Mehr to Zyfflich and from there to Nijmegen, a route along the northern flank of the ice-pushed ridge between Kleve and Nijmegen, and a route along the western and the southern banks of Rhine and Waal over Millingen (see fig. 75).

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125 In Bogaers/Rüger 1974, 93–5.
126 Auxiliary fort: *Itinerarium Antonini* 256, 3; intermediate station: inscriptions *CIL* XIII, 8702 and 8703.
128 The exact size is unknown because the eastern part of the settlement was eroded by the Meuse in post-Roman times. The north-south axis measures c. 160 m.
129 Bogaers 1966, 68.
Fig. 75 Military settlements and sites discussed in paragraph 8.4.2 as possible locations of *stationes* or other installations such as signal stations, watchtowers, *mansiones*, or *mutationes*. The system of land and water routes is derived from fig. 22 and thus not intended as a precise reconstruction of the location of navigable channels or Roman roads. VM is the findspot at Viller Mühle mentioned in the text: 1-7 see fig. 58, 8 (possible) military settlements (squares) and other sites (circles) as described above, 9 water-route, 10 land-route. Scale 1:250,000.
along the limes. As far as possible limes stations at the frontier are concerned, there are only two sites which have not been mentioned before in this chapter, namely, 441 and 461.

Although site 441 (Millingen-Eversberg) must have been several kilometres to the west of the limes (fig. 75), it may have been a watchtower or signal-station. This is because small-scale excavations from 1885 to 1887 apparently revealed the presence of a Roman stone building, surrounded by an elliptical ditch. The reliability of these observations is, however, questionable. Later finds have confirmed the presence of a stone building, but the interpretation as a military site depends entirely on the credibility of the feature(s?) presented as an elliptical ditch. Only new excavations can resolve this problem, but for the moment the claims made for site 441 had better not be taken too seriously.

Horn considers site 461 (Qualburg) a possible Beneficiarius-station. That is not very likely, but a fortlet or watchtower in Qualburg is indeed a possibility, also because there was definitely a military site (no. 460) in Qualburg from the late-3rd century onwards. In addition, the distance between Harenatium (Rindern, 450) and Burginatium (Altkalkar) is large enough to expect intermediate posts.

The argument of distances may also indicate that one or more of the sites discussed above as possible limes forts, are actually smaller posts. It is, for example, difficult to accept that sites 117 as well as 126, 135 and 194, which are very close together (fig. 75), should all be full-sized auxiliary forts. The accumulation of possible forts on such a short stretch of the limes is, in fact, an additional argument for dropping the secondary finds from site 135 (Huissen) from further consideration, but it also indicates that sites 117 or 194 may eventually turn out to be an intermediate station of some sort.

It is not clear whether additional military sites are still to be expected along the Rhine. The sites discussed so far, the distances between them, and the flatness of the landscape may have been sufficient to allow a reliable communication system by fire or smoke signals and other means. The erosion by post-Roman Rhine channels, which has destroyed or negatively affected most of the evidence available today, indicates that if there were more military sites along the Rhine, the probability of discovery is rather low.

On the other hand, the geological situation also indicates that it could have been very profitable for the limes system as a whole, to have included outposts like watchtowers or signal stations at strategic spots on the high grounds north and east of the Rhine. Such posts have never been found, but the presence of military tile-stamps beyond the limes could be a significant indication for them. Although, for example, sites 1 and 7 do certainly not qualify as military settlements themselves and thus provide sufficient warning against a too hasty interpretation of tile-stamps as an indication for actual military buildings, the presence of military tile-stamps there is at least curious. The same is true for a number of stamped tiles from the Montferland, just east of the area covered by this study.

Unlike those from the river area, the presence of military tiles beyond the limes can be more readily interpreted as a direct result of activities by the army, just as the examples from sites along roads further to the south.

The location of sites 1 and 7 along a major route to the north is an acceptable explanation for the presence of military artefacts, but it does not answer the question whether they came from a site south or north of the Rhine. In the absence of indisputable evidence for actual military stations north of the Rhine, the concept of a rechtsrheinisches militärisches Nutzland as used by Rüger has to suffice the moment, although his argumentation to reject outposts in that area is not at all convincing.

The explicit orders of Claudius to Corbulo, instructing him to withdraw the praesidium cis Rhenum, very clearly...
refer to camps in the context of a new offensive campaign. They do not necessarily imply that no military posts beyond the Rhine were allowed any longer, especially not if they were designed to function in the context of the early-warning and intercept system that the *limes* actually was.

The last category of military sites to be treated here, are the *stationes beneficiariorum consularis*. In a discussion based primarily on the distribution of military tile-stamps and a straightforward interpretation of them, Rüger felt justified in suggesting a whole series of these *stationes* in the river area. This is no longer possible without additional specific arguments for each site. These are hard to supply in the absence of excavations and also because it is difficult to identify a type of site which may not have recognizable characteristic attributes.

The only firm case for a *statio* in the river area can be made for site 500 (Cuijk). As already mentioned above, the location of Cuijk at the spot where an important road crossed the Meuse, is the ideal type-location for a *statio*. Moreover, the presence of an earlier and also a late-Roman fort which were there for the same purpose testify to the importance that was attached to this crossing.

Of the other claims made for sites as *stationes*, Kesteren, Nijmegen, Huissen, and Qualburg have already been discussed. In Nijmegen, the presence of a *statio*, for example on site 399 at one of the town's exits, is altogether possible. The claims for sites like Zetten, Doorneweg, Niel (437), Wiljer-Hochstrasse, and Duffelward (see fig. 50) can be rejected out of hand, because they are based on stray finds usually in secondary position as well. Zyfflich (432), Viller Mühle,137 and Heumensoord (391) are indeed possible locations for a *statio*. As is evident from the geological situation, site 432 would be a suitable location to control traffic on a possible direct route from Nijmegen to the *limes* road, but there are too many alternatives to give this consideration much weight.

The finds from Viller Mühle are, if reliable, a stronger argument. A *statio* there would control traffic along and possibly across the Niers. Rüger138 mentions a *Niersübergang* but no motivation for it. If a *statio* controlled a route along the Niers, its position may have been determined by the border between the *civitates* of the Batavi and the Traianenses, as is suggested by the theoretical border indicated on figs. 3 and 135. The middle-Roman occupation at the site of the late-Roman *burgus* of Heumensoord is not well documented, but Bogaers' suggestion that it might have been a *statio*139 is altogether plausible. The site is located half-way between Cuijk and Nijmegen, and undoubtedly at or very near the road.

Although the presence of additional road stations can never be excluded, there are only two other sites at which the presence of a *statio* is plausible. These are the *vici* of Elst (105) and Wijchen (315) discussed below. Elst is situated in the middle of the Betuwe at the intersection of north-south and east-west routes. As a local centre, it may well have included a *statio*. The same is true for Wijchen, where a road station could control both the traffic over the Pleistocene deposits and that along the northern branch of the Meuse.

Finally, and in view of what has been said so far about small military posts, it should be realised that there is still another possibility for some of the sites involved. As far as they are located south of the Rhine, it is conceivable that they are not military posts at all but stations of the *cursus publicus*, the official imperial courier- and transport-service. This centrally organized government-service140 used stations, the *mansiones* (hostels) located at a day's travel from each other and the *mutationes* (staging-posts) at shorter distances.141 *Mansiones* (or *tabernae, praetoria, deversoria*)142 and *stationes beneficiariorum consularis* could be combined,143 but even without such a combination the official character of the stations of the *cursus publicus* surely allows one to expect 'military' material there (stamped tiles, horse-gear, and the like) and evidently the choice of location must have resulted from virtually identical motives.

137 See chapter 6, notes 252 and 254. Viller Mühle is located in square 200/413 on Appendix 3. See fig. 75.
138 Rüger 1968, 68.
139 See Bogaers/Rüger 1974, 81.
140 See Stein 1932, 55–6 and Bender 1978, 15–6. For our area, we know, e.g., of a *praefectus vehicularum per Belgicam et duas Germanias* (*CIL* VIII, 12020 = *ER* II, 578): the director, therefore, of a (late-2nd – early-3rd century) district encompassing several provinces.
8.4.2 Civil Settlements

As mentioned in the introduction, the known middle-Roman settlements probably represent at least 50% of the number originally present. For the river area proper, this figure is certainly much higher. Should there be no different forms of settlement which are specific to the Pleistocene deposits - which is hardly likely in view of the settlement types reported from other regions - a complete spectrum should be present in the available sites.

The analysis of civil settlements is not as straightforward as that of the military sites. Although they are also part of a larger superstructure and Roman concepts may be applicable to designate certain forms of settlement, there are also sites and structures which are specific to the region and not primarily related to the Roman occupation or a direct result of it. It is not advisable, therefore, to present a framework of Roman concepts and try to fit all settlements into that scheme. On the other hand, a description completely in neutral terms is unnecessary, both because of the foreknowledge on specific sites and of the obvious applicability of Roman concepts in several cases.

The clearest example of this situation is, of course, Ulpia Noviomagus Batavorum. In analytical terms, it should be called a regional centre, in functional terms a civitas-capital, in Roman juridical terms (probably since IIB) a municipium, and in a current technical/juridical terminology for urban settlements a city. Depending on one's point of view, there are still more terms which could be applied, but the key term is regional centre. By implication this indicates some degree of hierarchical organization of the settlements. The nature of that organization is not the subject of this chapter, but the fact that the regional centre has been identified as a civitas-capital is a source of valuable additional information about the position of the settlement in relation to others within and outside the region, and also about some of its characteristics which have not yet been or cannot be traced by archaeological means.

Similar opportunities are available for other types of settlements, although they rapidly diminish when the primary archaeological data are inadequate. This problem is especially relevant when forms of settlement are discussed of which no example has been sufficiently investi-
The concentration of the Roman administrative apparatus in Nijmegen makes it also likely that site 403 functioned as an economic centre, even though it may not have been situated at a very suitable location for that purpose. As already mentioned above, the locations of all early-Roman sites in Nijmegen must have been chosen with a military objective in mind. In fact, the shift of the regional centre from site 403 to 399, which took place

147 See chapter 2, note 21 and below, chapter 11, fig. 127 and p. 398.
after the events of AD 69-70, may be just as much or even more a result of the unsuitable location of 403 for a regional centre, than of its suitability for military purposes, which is usually considered as a motive.\textsuperscript{150} In any case, most of the area occupied by the old centre was no

\textsuperscript{150} E.g., Bogaers 1979b, 57: \ldots; mann kann sich vorstellen, dass die Römer auf der Stauchmoräne, in unmittelbarer Nähe des Lagers, nun keine ausgedehnte Zivilsiedlung mehr geduldet haben.
longer settled after AD 70. What remained of it (compare figs. 66 and 77) was a small settlement along the Waal, at first probably in connection with – if not as a part of – the canabae legionis of the new fortress at site 412.\textsuperscript{151} See figs. 76 and 77.

\textsuperscript{151} Cf. Bogaers 1960–61, 276.
\textsuperscript{152} See chapter 11, note 332, and also below, note 182.
\textsuperscript{153} See Bogaers 1979b, Abb. 63.

In view of its alleged importance, site 399 (fig. 76) has undergone surprisingly little in the way of archaeological investigation. A no longer entirely up-to-date overview of available data, current interpretation, and relevant literature is provided in the Noviomagus volume. Very recent excavations have provided new evidence in support of the interpretation given there.\textsuperscript{152} From the plan of the available traces of buildings,\textsuperscript{153} it appears as if the settlement did not grow naturally into a
town but was a planned development with a regular street-grid and therefore the deliberate foundation of a new administrative and economic centre. The latter function is expressed most clearly in its name, Noviomagus (Newmarket), which replaced the old name Batavodurum. It is generally assumed that site 399 retained the old name until it was granted the *ius mendinarum* by the emperor Traianus, which is an explanation for the honorary title *Ulpia*. Too little is known about site 399 to decide whether this grant was the official recognition of an established practice, which would seem to be a logical assumption, or the actual beginning of a new function related to the departure of the 10th legion, as proposed by Bogaers. In any case, the departure of the legion must indeed have caused a shift in diverting to the town those activities around the fortress that did not disappear with the legion. This assumption has been confirmed by the excavations of the *canabae*, which indicate a decline after IIa when the fortress was occupied by a detachment of the 30th Legion.

As far as a settlement typology is concerned, the fact that Noviomagus later also received a charter and became Municipium Batavorum is not particularly relevant. This took place in IIB or even later and had only formal, juridical consequences. If it happened after AD 212, when Caracalla issued his edict conferring Roman citizenship to all free subjects of the empire, the real significance must have been virtually nil. The old name (*Ulpia*) Noviomagus continued to be used after this event and, in fact, survives in the present-day Nijmegen. The more extensive designation *Ulpia* Noviomagus Batavorum is used on Appendix 3 to avoid multiple names, but it has never been attested epigraphically.

Because of the restricted amount of work done on site 399, there are only a few archaeological indications for the structure and function of the town and its institutions. These are supplemented by some scattered epigraphical evidence, but most of its characteristics have to be derived by implication from what is known about other civitas-capitals. As already mentioned, the excavated traces of buildings indicate that the town was a planned development, presumably with a regular street grid. The estimated size is c. 40 ha, but it is quite uncertain if this entire area was completely built over. In this respect it is worth noting that there are kiln-sites within it, such as those under the Maasplein and under the former starch factory along the Waal. These kilns are incompatible with a dense occupation in the immediate vicinity and should be outside the town, unless one is prepared to consider the possibility of potters' workshops on (still) empty insulae. The Maasplein kilns were in operation during the early 2nd century, so it is conceivable that the town either grew to its full size somewhat later, or that the area within the original perimeter only gradually or never filled up.

As far as this perimeter is concerned, there were until recently no other data than the distribution of finds. Recent findings mentioned above show that the town did indeed have peripheral defences like the walls around the nearby Colonia Ulpia Traiana or, as a later addition, Atuatuca Tungrorum and Forum Hadriani/Municipium A. Cananefat(i)um. Especially in view of these last two towns, it was to be expected that Noviomagus was also walled at some point in its history. The only clearly interpretable buildings inside the town are two adjacent Gallo-Roman temples: an indication for the religious function of the town as a central place. Remains of another structure, partially excavated in 1834, could point to another temple, a *thermen*, or to a so-called *cryptoporticus* in relation to a *forum*.

Whatever the true nature of these foundations may be, both a *thermen* and a *forum* were necessary elements of any Roman town and they should be present in Noviomagus as well. For the presence of a *forum* with a *curia*, a town-hall, there is also indirect evidence in the dedications of altars by three or four *decuriones* of the city-council, the *ordo decurionum*. Such an *ordo* must, by the way, also have been present when Noviomagus was not yet a *municipium* and therefore juridically a *vicus*. *Vici* could not have an *ordo* of their own, but the council of

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155 Bogaers 1960–61, 309–10. The event took place under Marcus Aurelius or Septimius Severus, possibly even as late as Caracalla's reign, but in any case before AD 227 (Bogaers 1972b, 9).
156 Maasplein: Daniels 1927, 90–2; starch factory: Daniëls 1955, 202–3. This kiln may have been used for the production of lamps.
157 The excavators originally interpreted the structure as a *thermen* (Brunsting 1949, 55), which is not at all improbable (Bogaers 1979b, 61), but a temple (Brunsting 1949, 57) or a *cryptoporticus* (Von Petrikovits 1955, 11) remain possible.
158 Bogaers 1960–61, 287–9 and note 146; Bogaers 1972b. See also chapter 11, note 336.
159 Rupprecht 1975, 44–5; Vittinghoff 1976, 89 and note 82.
decurions which governed the civitas had its seat in its caput, which could well be a vicus.

In addition to the designation 'Ulpian Newmarket' and the fact that at least two of the decurions who dedicated an altar to the goddess Nehalennia were traders, there are also other inscriptions pointing to trade and industry on and around site 399. These include a Nervian negotiator frumentarius (see fig. 136; CIL XIII, 8725 = ER II, 191) working in Nijmegen and evidence for a local collegium fabrum tignariorum (ER II, 203; not in CIL), a guild of carpenters (or artificers). Apart from the kilns mentioned above, the inscriptions are the only direct evidence for Noviomagus as supply and trading centre but they do indicate the presence of a market-place and the shops or workshops whose actual remains still await excavation. Likewise, the presence of a harbour need not be doubted, although it was presumably eroded by the river Waal when the northern part of site 399 was washed away. A second possible location for a harbour is site 403 which must have stayed in military hands after AD 70 as part of the canabae legionis. Although it may also have served the town, especially from the end of the 2nd century onwards, it is, as will be explained below, more likely that it always remained primarily military.

Although in this case the inheritance by the town of a part of the military infrastructure is only a possibility, there is at least one instance where a similar process can be demonstrated, namely, the amphitheatre at site 408. The amphitheatre was part of the canabae legionis belonging to the fortress (412), but in contrast to the rest of the canabae, the finds clearly indicate that this structure continued to be used even into the 3rd century. This implies that it was probably used for the entertainment of the people from the town and the region.

Apart from indicating yet another function of the regional centre, this probably means that site 399 never had its own amphitheatre. Because no decent Roman town could normally do without one, this suggests at least the possibility that there may have been other facilities lacking at site 399 as well. This is understandable for the period when there was still a sizable number of soldiers present but some amenities of town life may never have been available.

Secondary centres

There are a number of places in the region which surpass the level of the various sorts of small settlements which constitute the bulk of the sites presented in the catalogue. The difference between these settlements and the others is not only expressed in their size, but also in the presence of structures which are absent at other settlements and which imply a central or special function. For this reason, they have all been termed secondary centres, although there may be considerable differences between some of the sites included in this category. The best way to explore these differences more fully, and the sorts of sites involved, is to discuss first the term by which they are all indicated, namely the Roman concept of vicus.

Literally, vicus simply means 'dwelling-place'. In practice, the term is used for all kinds of settlement, from non-urban villages generally not exceeding 20 ha to small towns, covering up to 60 ha. It was not, therefore, an independent entity on its own, even when it was an urban centre and civitas-capital such as Ulpia Noviomagus. Civitas-capitals without a charter are, although de facto a town, juridically not different from other, smaller vici.

The legal position of the civitates and their capitals, which also gradually changed during the Roman Period, has been hotly debated during recent decades, but it is not the subject of the present discussion. A civitas-capital and de facto town can be described in the same way as a city, namely, as regional centre or central place, thereby avoiding all the intricacies connected with the legal position. It also sets it apart from the other vici, which did not have such a function.

For these other vici, the dependency of their status is obvious from the different meanings in which the word is used. A vicus can be a separate settlement site, functioning as a subordinate local centre for the civilian popula-

160 Bloemers 1979d, 56.
161 Von Petrikovits 1978, 118–9. For an overview of relevant literature and opinions, see also Crickmore 1984, chapter 2.
162 The latter only if one is prepared to accept the propositions regarding dependent vici under military control and on military land. See below, p. 268 and note 169.
tion. It can also be a ward of a town or city, and thus not even be a separate settlement. In a military context, *vicus* is used to describe the settlements attached to auxiliary forts or, for the large settlements (canabae legionis) near legionary fortresses, the constituent parts or wards of them.

From the different meanings of the word *vicus*, it appears that there are two ways in which the concept of *vicus* can be divided: it is either a ward or a separate settlement and it is either civilian or attached to a military site. The first distinction is relevant, although for a settlement-typology only those *vici* are considered which are separate and archaeologically observable entities. This excludes the possible wards of site 399, but not the spatially segregated *vici* of the canabae legionis around site 412 which can - to some degree - be treated individually.

A more fundamental distinction may be that between civil and military *vici*. At a structural level, both share many similarities. They may include several sorts of public buildings, such as a temple, an inn, and a bath-house, but also more humble public facilities such as shops and workshops. The houses are usually of the narrow rectangular strip-house type. At a functional level there may, however, be a difference. The military *vici* could undoubtedly function as a centre for the surrounding area, but their main purpose was to provide services for the soldiers. Regardless of the impetus for their origin, the civilian *vici* did not usually have such a limited purpose.

There are several different causes for the origin of a *vicus* but the presence of one such cause, for example a junction of roads, did not necessarily lead to a *vicus* there. *Vici* can thus be seen as developments which were successful not just because of one favourable circumstance, but because other important factors in the area, such as the presence of and distance from other *vici* or towns, size of the population, political, religious, ethnic, and a whole array of other circumstances, provided a stimulus. Therefore, civilian *vici* can generally be regarded as local secondary centres, tied into the regional network and providing a variety of services for the surrounding countryside, while military *vici* often seem to be more single-purpose, special centres.

The use of the term civilian v. military *vicus* may, however, not be completely adequate. On the one hand, it is possible that some civilian *vici* did not function as secondary centres but had a rather limited, for example, industrial or religious function. On the other, military *vici* may also have serviced a civilian hinterland, thus also being real secondary centres, as may have been the case in northern Britain. In effect, many or most of them continued to function as such in other areas when the frontier moved on and a military camp was no longer required.

This last consideration, forwarded most strongly by Frere, is one of the reasons why it is very difficult to determine on the basis of their location when, to what degree, and for what aspects of life *vici* serviced surrounding areas. The spacing of *vici* is influenced by military considerations, although perhaps less strongly than in Britain. But apart from this argument the usual non-random spacing of *vici*, whether ‘artificially created’ or ‘naturally grown’, is in itself already an indication for a function as secondary centres, as long as there are no convincing arguments to the contrary.

The alleged juridical and administrative status of military *vici* is at least one argument that is not convincing in this respect. Although traditional views hold that the settlements at the gates of forts were completely under military jurisdiction and control, this hypothesis has been seriously challenged. For several reasons, the so-called military *vici* were probably fairly independent and in fact normally under the nearest civil authority, while the larger canabae were at least partially independent. This does not exclude military influence, but it also does not set these settlements apart from the administrative and economic organization of the region.

On the basis of the foregoing arguments, it might be concluded that different kinds of *vici* should not be distinguished because they fulfilled a similar function under the same authority, namely, the civitas. Although they were undoubtedly all local (secondary) centres

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164 Because of these different meanings of the term a *vicus* in the sense of a ward can even be part of a *vicus* in the sense of a town (cf. Bogaers 1967a, 232).
166 Salway 1965, 1980. A different opinion is voiced by Sommer (1984, 51–2), who suggests that the military *vici* primarily serviced the forts with only a limited function for the surrounding native population.
167 Frere 1975. See also the comment of I. Hodder in the same volume. On the military origins of non-military *vici*, see also Webster 1966 and Vittinghoff 1970.
some, and most notably the military *vicus*, however, were also special centres. The difference between the two does, as will be shown below, indeed seem to conform to the division between ‘civilian’ and ‘military’ in the eastern river area.

When the sites in the region are considered, it turns out that the number of possible *vicis* is fairly limited, which is to be expected in view of the size of the area studied. As far as the forts along the Rhine are concerned, only one of the sites discussed in paragraph 8.4.1 can be shown to have an adjacent settlement worthy of the name *vicus*. In view of the present state of the evidence, which in general is rather meagre, that is hardly surprising. Further research will surely produce enough data for a reasonable evaluation of the ‘military’ *vicis* even though erosion may have destroyed part of the sites. At this moment, only a brief discussion for all the proposed forts can be provided.

The best information is that available for Kesteren (37), although the actual fort is still missing and probably eroded as mentioned earlier (p. 250). An adjacent *vicus* might thus have been eroded as well, but there is increasing evidence that it can be identified as site 38, where a considerable quantity of military material has been recovered. The excavations in 1968 and 1977 have not produced clear evidence for or against this interpretation, although it was supported by the military finds and the pottery assemblage (see paragraph 8.4.3). New excavations in 1984, however, produced additional evidence in the form of firm indications for strip-houses, more military finds, and even V-shaped ‘military’ ditches surrounding the site.

The acceptance of the now almost inescapable conclusion that site 38 is a military *vicus* produces two further implications. First, it is additional evidence for the assumed presence and location of site 37. Second, the military *vicus* in this case is hardly likely to have functioned as a local centre because it is fairly small, c. 3 ha, and indications for public buildings, or even buildings in stone, are absent.

For a settlement near site 18b in Randwijk there is, of course, no information at all. As mentioned before, site 117/8 in Driel is exceptionally large (c. 8.5 ha) and may be the site of a fort together with a *vicus*, which has been tentatively localized at site 118. In any case, even if one is not prepared to accept the presence of a fort, it is clear that here we have at least a sizeable settlement which is likely to be a *vicus*. Because of the lack of specific data, the structure and purpose of the occupation remain unknown.

For civilian occupation near the fort in Meinerswijk (126), there is some evidence on the other side of the present-day Rhine, namely, at site 23. The Roman Rhine was probably located immediately at the foot of the ice-pushed ridge under Oosterbeek (north of site 23) and must have eroded all or most of site 23 in post-Roman times. Dredging operations in 1953 yielded a number of artefacts, mostly sherds, which is all there is to indicate the site of a possible *vicus* (see fig. 93). The presence of tuff indicates that it may have included stone buildings.

The data from Huissen (135) indicate nothing specifically related to a *vicus*. Nor, at first sight, do those from site 194 in the Loowaard, but the quantitative analysis of the finds in paragraph 8.4.3 indicates that material from a temple might be involved, and that could indicate a *vicus*. For the moment this is, however, nothing more than an interesting speculation: part of the finds may also originate from a cemetery or be explained in another way. Data on *vicis* near the forts in De Bijland (182) and Rindern (450) are completely lacking. Only the middle-Roman settlement in Qualburg (461), which may be related to some sort of small military station, could possibly be considered a *vicus*. The very limited excavations by Von Petrikovits in 1937 do not provide any real arguments for further discussion.

The evidence from excavations elsewhere along the *limes* shows that civil occupation in relation to forts may vary from a few insignificant structures to large settlements with strip-houses, public buildings, workshops, etc. For the eastern river area, the data are too scanty and deficient to reach any definite conclusions. There are, however, several reasons to suspect that most, if not all, of the settlements involved were closer to the lower than to the upper end of the scale and that they functioned more as special centres to provide services for the army than as local centres for the native population.

First, there are too many military sites to suppose that they were all large auxiliary forts. As mentioned above, some may have been no more than fortlets and the smaller the number of soldiers, the smaller the ‘*vicus*’ is likely to be. The number of military *vicis* would also be

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170 JROB 1984, chapter II B, sub 2c.
171 Cf. note 170.
far too large in relation to the size of the entire area and the number of settlements involved, and their linear configuration along the Rhine is not very favourable for a function as local centre. Second, diverse evidence from elsewhere suggests a strong orientation towards the army. Third, there are other settlements, including the regional centre itself, which are close enough and in a much better geographical position to provide the necessary services to the native population.

For the region between Rhine and Waal, the Overbetuwe, it is quite clear that Elst (105) must have been the local centre, thereby removing the need to promote most of the presumed military vicini to that status. Site 105 may have been as large as 15 ha and, apart from the well-known temple (see fig. 129), it included several other stone buildings of which only parts have been recorded so far. It is located in the geographical centre of the Overbetuwe and can be easily reached from all directions. It must have been a cult-site for the entire region, maybe even for the civitas, but there is no reason to assume that this was its only function. The construction of the temple around AD 50 may have contributed to the growing importance of Elst, but it can also be seen as confirmation of its ‘natural’ status as a local centre for the Overbetuwe. Because of its strategic location, Elst is also a plausible location for a statio, which would control most of the traffic through the Overbetuwe.

In the region between Waal and Meuse, there is also only one site which can be interpreted as a vicus and local centre, namely, Wijchen (315). No part of this settlement has ever been excavated, but amateur archaeologists have recovered remarkable amounts of material from several closely associated findspots. The settlement may have covered about 9 ha, and on several findspots clear evidence for stone buildings was found. The settlement is conveniently located on the Pleistocene deposits, next to a branch of the Meuse. It does not appear to have a long pre-Roman history itself, but it is located in the area where the more important pre- and early-Roman settlements have been tentatively localized. It may, therefore, represent a continuation of some sort of traditional centre.

In any case, it is favourably situated in relation to traffic routes, which was a reason to consider it, just like Elst, as a possible location for a statio. Because of the lack of excavations, nothing can be said with certainty about public buildings. The ample evidence for building in stone does not constitute definite proof of their existence either, although it may certainly be taken as a first indication.

A final remarkable feature of the vicus at site 315 is that on all findspots quite considerable amounts of material from the 4th and 5th centuries, and also from the Merovingian Period, were found. Although presumably not unchanged, Wijchen must therefore have remained a centre of some importance after the Middle-Roman Period, in contrast to Elst.

The third site which can be interpreted as a vicus is Cuijk (500), undoubtedly the local centre for the area south of the Meuse. Just like Elst and Wijchen, it is situated at an ideal spot in relation to traffic routes. In this case the strategic importance of the site is very evident. Excavations have revealed the presence of a fort (499) in the 1st century and again in the Late-Roman Period. For the period in between, the presence of a statio is very likely indeed.

The vicus of the 2nd and 3rd centuries included the site of the earlier fort, but it also extended over a wider area. Its size is difficult to determine, but it probably did not exceed 10 ha. Excavations have revealed the presence of at least six stone buildings, including two Gallo-Roman temples and wooden striphouses, presumably shops. Even though the preceding fort must have provided a regular pattern, the recovered traces of buildings do not indicate a regular street grid. The fortification of Cuijk in the Late-Roman Period and the evidence for the early- as well as later-medieval occupation at the site show that, like Wijchen, it must have continued to be a local centre of some importance.

The only area that lacks a vicus as local centre, is the area to the southeast of Nijmegen. It is possible that Rindern (450) or Qualburg (461) functioned as such. At least in theory Qualburg would be in a favourable situation as a secondary centre, being located on the major route (the limes road) between Nijmegen and Xanten and, even more important, exactly midway between the two regional centres. It is located precisely on the theoretical boundary between the civitates of the Batavi and the
Traianenses (cf. figs. 3 and 4). It should, however, be repeated that the archaeological evidence is too scanty to test this proposition. In addition, even though the known distribution of sites is certainly deficient, the numbers of sites in the area and especially on the Pleistocene deposits may have been far fewer than in the river area. Thus, there may have been no need for a separate local centre.

The archaeological evidence indicates that each of the geographically separate regions of the Maaskant, Land van Maas en Waal, and Overbetuwe had a small vicus as its own centre. These secondary centres are situated at geographically very favourable locations in a semicircle around the primary centre. As far as the military situation is concerned, the location of each of them can also be interpreted as strategically important for communication and control purposes. Even though the military aspects of Elst and Wijchen are less obvious than those of Cuijk, they are no less certainly present, for example, in the military involvement in the construction of the temples in Elst and they may, in the final analysis, well prove to be the initial reason for the growth of these settlements into vicini.

Their main function can, however, not have been a military one. Elst and Cuijk were definitely religious centres and at least for Cuijk there is evidence that it was a centre for economic activities also. Presumably all three vicini were religious and economic centres, and also the sites of a statio. They may even have been administrative centres for their respective regions. Elsewhere it has been argued that the civitas Batavorum can be divided into five geographically separate entities, namely the Overbetuwe, the Land van Maas en Waal, the western river area (Nederbetuwe, Bommerwaard), the (largely) Holocene deposits south of the Meuse (Maaskant, Land van Cuijk), and the sandy soils of Brabant (Meierij van ’s Hertogenbosch). Each of these areas had a vicus as its centre. For the western river area the local centre could be Rossum, for Brabant it is obviously Halder near St. Michielsgestel. This matter will be returned to in chapter 11, but at this point it is important to note that these regions may well be identified as pagi, the administrative units into which a civitas could be subdivided. The fact that they each have a ‘capital’ of their own could support both the notion of pagi and the interpretation of these vicini as subordinate administrative centres. All in all, the evidence for ‘civilian’ vicini is — although limited — much better than for ‘military’ vicini related to frontier forts.

The only military vicini for which there is considerable evidence are the vicini which are the constituent parts of the canabae legionis around the Nijmegen fortress. These are sites 403, 407, 409, and 416, and in a way also site 433. The present knowledge about sites 407, 409, and 416, which is the result of the ROB excavations since 1972, has been summarized by Bloemers and need not be repeated here. It is remarkable that no houses have as yet been found. The buildings of site 407 seem to have a public, possibly religious nature: they may be related to the cemetery and burial monuments around them. Indications for industrial activities (iron slags) are, however, present as well. On site 409, the amphitheatre is also a public facility and the eastern ward, site 416, was clearly a centre for trade and industry. The large tile-works at De Holdeurn (433), some 4 kilometres to the east, were undoubtedly a military operation, although that does not exclude the presence of civilians nor the production of tiles for non-military purposes. Site 433 is a separate, industrial settlement, but it can also be seen as an outlying vicus of the canabae. It was in any case directly related to the fortress during the Flavian period and the early-2nd century. Later, it may have been a more independent settlement. After AD 175, the tile-works produced brick for the army of Lower Germany (exercitus Germanicus inferior).

As far as the interpretation of sites 407, 409, 416, and 433 is concerned, there are no problems because they were clearly special centres, primarily (but not exclusively) servicing the army. After the departure of all or most of the soldiers activities continued only at the tile-works and the amphitheatre. Although there are still substantial areas which have not been investigated, one has the impression that not too many people actually lived in the canabae directly around the fortress.

There is, however, one vicus which has not been mentioned so far, namely, the so-called ‘commercial ward’ along the Waal, at the site of the former Batavodurum. If our interpretation of Batavodurum is correct, that it was essentially a military site with a developing civil settlement around a military core, then it could be a logical...

177 Willems 1983a. See also chapter 11, 421–2.
178 On pagi and their centres, see also Crickmore 1984, 13, 24, 45f.
179 Bloemers 1979d. See also fig. 77.
development that part of the site continued to be inhabited by dependants of the soldiers after AD 70. Both the character of the sites directly around the fortress and the more favourable location of site 403 indicate that most of the canabenses probably lived along the Waal.

Although there have been some small excavations on this site, until recently only a limited number of occupation traces had been found.\(^{180}\) Part of the settlement must have been eroded by the Waal and the remaining portion is severely disturbed by late- and especially post-Roman occupation. It is noteworthy that the dating evidence indicates that most of the middle-Roman features belong to the Flavian period and the first half of the 2nd century.\(^{181}\) Although the site was continuously occupied it seems, therefore, that most of the activities ended around the same time as on sites 407 and 409, which is another argument to consider site 403 as a part of the canabae legionis, most likely a residential ward around a harbour and thus also a special centre. The continuing occupation from the end of the 2nd century onwards was presumably also related to military affairs. Unless the town had no harbour of its own, which is not very likely, the only plausible function for site 403 would seem to be that it served to ship the tiles produced at the Holdeurn.\(^{182}\)

**Villae**

As is evident from a comparison of Appendices 1-5, rural settlements of the Middle-Roman Period are more diverse than in any other period. As a group, they differ from the villages described as vici because they are smaller and do not have public buildings or shops. Their location is determined primarily by the habitability and exploitability of the land and most of the excavated structures are clearly farms or otherwise related to agricultural activities. Nevertheless, they are not all the same and there are substantial differences between sites, not only between excavated settlements but also between collections of surface-material.

In virtually every study on rural settlement in the Roman Period, a basic distinction is made between two major types of settlements: the villa, which is an adaptation of a Roman form of settlement,\(^{183}\) and the native village (or hamlet, or single farmstead), which is primarily a continuation of the traditional way of life, although it need not be unchanged. There is no reason to assume that the situation in the eastern river area was completely different from this, even though it is a frontier area where conditions may not have been the same as further to the south.

In order to be able to identify settlements as villae, it is necessary to review briefly what the concept of villa entails and, secondly, by which archaeological phenomena it can be recognized.

Exactly what constitutes a Roman villa is a subject that will probably be debated forever, but there are several basic aspects that are generally agreed upon. The first of these is that a villa is an exclusively rural type of settlement, but a second and related property of a villa is its dependence on towns. In his well-known article on their social and economic aspects, Rivet\(^{184}\) stressed the fact that villae were intimately associated with towns, which provided a market for their surplus production, another basic aspect of villae. They were owned by wealthy individuals, according to Rivet in principle by townsmen, and represented a large capital investment which had to bring some sort of profit. The estates, the villae and the land that belonged to them, were therefore themselves also property that could be bought and sold.

It is obvious that the application of the concept of villa to settlements in the river area will, by implication, have far-reaching consequences because of its presuppositions on the social and economic systems involved. If there are villae, then there has to be some sort of town-based cash-market system and there have to be people who have both the means and the desire to participate in such a system. In this chapter, we are not concerned with questions like that, but rather with an analysis of observable differentiation which may contribute to answering those questions. Nevertheless, villae are such typical means of exploiting the land that the question of their absence or presence in the river area cannot be postponed.

Fortunately, it is not too complicated to demonstrate the available data on the features and size of both sites and also by other information, such as the location of the large cemetery (398) south of site 399.

\(^{180}\) Van Tent 1973, 131-4. Van Tent’s interpretation of Roman terrace-wall(s) is not correct. Recent excavations are discussed in *fROB* 1983, 47-8 and 1984 (in press).

\(^{181}\) Van Tent 1973, 132.

\(^{182}\) Van Tent’s suggestion (1973, 134) that site 403 rather than 399 should be identified as Ulpia Noviomagus is very unlikely indeed (cf. Bogaers 1979b, 58-9). It is contradicted by

\(^{183}\) Hence the use of terms like Gallo-Roman or Romano-British.

\(^{184}\) Rivet 1969.
that, in principle, they may have been present. First of all, the continuous presence of troops must undoubtedly have led to an economy that was at least partially cash-based. In addition, the area had a town which, as Ulpian Newmarket, testifies to the presence of a regional market centre already by its name, and there were also local centres in each of the geographically separated areas. Without going into too much detail, and without having to suggest that the river area had a full-scale market economy, we can therefore assume that a basis existed whereby villae could function.

As far as people are concerned, the group of potential villa owners is restricted to those with adequate financial resources. Although members of a wealthy native aristocracy are the first to come to mind in this respect, there are other groups which could also qualify. These are retired soldiers who could be awarded land (and had saved enough money during their time of service) and also rich businessmen from towns. There may also be some overlap between these groups, for example, in the case of army officers who were members of the aristocracy. Direct (epigraphical) evidence on the ownership of villae is difficult to obtain and is sometimes ambiguous,185 but at this point it is sufficient to note that there is no reason to exclude any of the three sorts of potential villa owners for the river area.186 There is also no reason to assume that villae were absent for other reasons. All three groups are likely to have been strongly influenced by Roman culture - most of them may even have been Roman citizens - and therefore not averse to owning a villa. As long as the security of the area was not questioned, the investment in a villa could be a profitable enterprise, although it may not even be necessary to presuppose such capitalistic motives for all groups of owners. Depending on the degree of acculturation, it is also conceivable that the profit of the investment was more social than economical, especially in the cases of native owners such as veterans who had returned home and of members of leading families.

The main reason why a villa could be a status symbol as well as a source of income is the fact that it differed from a normal settlement. That is the second issue which has to be dealt with here: what are the attributes by which a site can be identified as a villa?

A villa cannot really be defined, but it can at least be described by some of its most important aspects. It can thus be said that it is, in principle, an agricultural settlement producing a marketable surplus for its owner's profit. The owner may live at the villa and direct it, have it administered by a vilicus (bailiff), or lease it to a colonus (tenant farmer),187 but in all cases it was centrally organized around a main house that exhibited a smaller or larger degree of Roman influence in shape and construction.

When attributes like this have to be derived from features and plans or, even worse, from surface scatters of ploughed-up rubble, several difficulties arise. A villa has two aspects, the pars rustica, where the agricultural activities take place, and the pars urbana, the living quarters of the person in charge. These parts may, however, take several different shapes and in some cases only the structure of the site may lead to its recognition as a villa. After all, native farmsteads also have an agricultural part and living quarters, but a villa characteristically exhibits a spatial segregation of both parts, with a main house as pars urbana surrounded by various other buildings.

This segregation can go further to the point where the main house, as villa urbana, stands on its own, the agricultural activities being carried out in other parts (archaeologically different settlements) of the estate. The main building itself exhibits parallel features, changing from fairly plain and small to luxurious and huge, and from structurally united to very differentiated. The latter point, which was made by Rivet,188 is important because the introduction of corridors and wings opens the possibility of segregating the master from this staff. The simpler plans are socially more comparable to the native farms.

The recognition of these different types of settlements which may be called villae depends primarily on excavations, of which there are at least a few in the eastern part of the Dutch river area. The most extensive one is that at site 214 in Druten. Nearly 2 hectares of this settlement were uncovered and it can be called a villa for several reasons.

First of all, the plan (fig. 78) clearly shows that the settle-
ment had a very regular layout with buildings arranged on three sides of a rectangular courtyard. Although several phases and constituent parts can be discerned, it is clear that this was a planned development in which different parts are combined into one centrally managed and organized whole. House no. 1 on the west side and the adjacent bath-house (2) can easily be identified as the residence of this central authority.

The separation between the pars urbana and the pars rustica is visible in the narrow foundation-trench between the large pit (hut) 16 and the round structure (5), later replaced by a wooden partition-wall with gate between...
buildings 17 and 8-9. In the first phase in which the *pars urbana* was fully developed (IIA), it consisted of the buildings 1, 2, 3, 4, 12, and 15. The farms and/or barns 8-10 and 17-20 in the eastern part constitute the *pars rustica* in its successive phases.

A second reason to regard site 214 as a villa are the constructional details of the buildings themselves. Although, as Hulst mentions, the two-aisled plans are clearly a development from the native tradition of house-building, some structures seem to have a *porticus* as an additional feature. There are, however, also one-aisled buildings which are not native and then there is the bath-house which testifies both to the wealth and the degree of its owner's acculturation to a Roman life-style. The bath-house is built in stone, together with a few other buildings and part of the main house. The latter is somewhat surprising, because the main house of a villa is usually to a larger extent, or completely, constructed of stone. This is not in the last place due to the presence of a heating system, which is also lacking in house 1. It must, however, have been present under the two western rooms of the bath-house, which essentially constitutes one unit with house 1. Both buildings had an interior decoration of wall-painting, a feature that was also present in the stone building 4 and the one-aisled house 12.

Although the native roots of site 214 are very clear and the villa may well have been a continuation of a late-Iron Age settlement situated on an adjacent, unexcavated parcel, there need not be any doubt as to its status as a villa. Roman writers like Varro and Columella would probably have recognized it as such, even though they would certainly not have been impressed by it. The socio-economic implications of the settlement plan are too clear to be misinterpreted, but the aspects of acculturation and wealth are only relevant in the context of the region. There is no point in comparing the villae from the river area to those further to the south in order to compose a list of attributes in the absence of which a settlement cannot be called a villa. Evidently, there are quite a few differences but they are in degree, not in kind. As far as they are centrally organized and, compared to the surrounding settlements, represent a significantly higher level of adoption of Roman norms and values, there is every reason to consider this category of farms in the river area as villae.

Unfortunately, large-scale excavations of sites similar to Druten-Klepperhei are lacking in the eastern river area. There are only three villae where some degree of archaeological investigation has been carried out, namely, Winssen (222), Overasselt (355), and Mook (377). In all three cases, the investigations were limited to small-scale excavations of the main building, in the case of Winssen not even enough to allow a reconstruction. The largely reconstructed plans of the buildings at sites 355 and 377 (fig. 79) are without any substantial context of surrounding features; only a few traces of adjacent structures have been partially excavated (355) or located (377). The main building in Overasselt measures 10 x 32 m and, in view of the position of the cellar, may have been three aisled. Although its construction is different, it is comparable to building 1 in Druten which measures 15-16 x 31 m, but includes a porticus. In any case, both houses are a very different category of villa compared to the large building in Mook (22.5 x 84 m). Certainly in the context of the river area, this is a monumental and luxurious structure and, even though the terminology is not entirely unambiguous, the excavator's conclusion that it must have been a *villa urbana* is entirely justified. There can hardly be any doubt about its status as the owner's residence and its scenic position high on the slope of the ice-pushed ridge is rather unfavourable as far as industrial and certainly agricultural activities in the immediate vicinity are concerned. It is more likely to be the centre of an estate, in this case conceivably extending over the coversand area to the south and including settlements such as sites 375 and 376.

Although the evidence on sites 214 and 377, and to a lesser degree on 222 and 355, is sufficient to allow definite conclusions, the level of information about other potential villa sites is very much lower. It is restricted to collections of surface material and incidental records of observations in the field. It is necessary, therefore, to develop qualitative or quantitative measures of surface material which permit identification of attributes specific to villae as opposed to other settlements. Unfortunately, as will be discussed in paragraph 8.4.4, the available data...
proved to be insufficient to establish any quantitative measures for this purpose. The only attribute which seems to be available is qualitative, namely, the presence of stone buildings at a site.

The application of this criterion does, however, pose certain problems. As was discussed in paragraph 6.4.2, the presence of stone buildings is deduced from the presence of stone, especially tuff, at the surface of a site. For some sites, namely, those with an important medieval occupation, the presence of Roman stone buildings cannot be assumed off-hand, because the material may have been brought to the site in post-Roman times. The presence of stone buildings may also indicate other types of settlement. In general, this is not a problem because it concerns the military sites, the civitas capital, and the vici discussed previously. But in addition to these sites, there may also be small military posts or, for example, rural shrines built in stone. Occasionally, this may lead to a faulty interpretation of the evidence but it can only be of minor importance.

It is much more relevant to determine whether the criterion of stone buildings is indeed suitable to differentiate villae from other rural settlements. If it is, then there should not be any villae without them or other settlements with them. In the absence of abundant excavation data, it is difficult to determine if such sites are indeed lacking. It is possible to eliminate the first possibility, occasional fragments of tuff or other stone-building material are sometimes found during excavation on sites without stone buildings, but these are not comparable to surface finds.

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Fig. 79 Plans of the villae at Mook (no. 1: site 377) and Over-asselt (no. 2: site 355), and of the building at Lent (no. 3: site 158): h = hypocaust, p = praefurnium, c = cellar. Scale 1:500.
villae without stone buildings, by accepting them as a conditio sine qua non. There is indeed much to say for such a definition because for a site to be called a villa it must exhibit at least a fairly large degree of Roman influence in shape and construction. Elements such as a cellar, a porticus, wall-painting, a heating system, or a bath need not all be present and do not all presuppose stone building, but they cannot all be absent. Site 214 is a good example in this respect. It is probably as native as a villa could possibly be, but it would still be recognized as such even if the characteristic, centrally organized layout were unknown. Conversely, it is hardly imaginable that someone would start such a planned, new development and at the same time refrain from all amenities of civilized Roman life and do completely without the necessary stone building.

The second possibility, a native village with stone buildings or a stone building which was not or did not become a villa, is the more difficult to evaluate. As far as the eastern river area is concerned, such settlements have never been excavated but there are several potential villae with a very long occupation history, often starting in the Iron Age. It is useful, therefore, to examine a similar and completely excavated site further to the west, namely, the settlement at Rijswijk-De Bult (see fig. 80). Unlike Druten, this settlement was not a planned development: it grew in successive stages from a single farm built in c. AD 30 or, more probably, c. 10 BC. to a maximum size of three households (houses and related structures) and – presumably – a temple, on an area of 1.85 ha surrounded by a ditch (see fig. 81). During the final (3rd century) stage, one of the houses, still at the same spot as the original single farm, was rebuilt in stone and two smaller square stone buildings (one at the place of the wooden temple) were erected. The house, with a plan reminiscent of that in Overasselt, has at least one room with a hypocaust and several with an interior decoration of wall-painting. It seems to have been used for living purposes only, because the normal area for stables is lacking. At the same time, and in addition to other changes, the existing partitioning of the settlement by ditches into separate yards is abandoned.

The interpretation of the data from Rijswijk is not as straightforward as in Druten, due to the long development, and it is further complicated by a possible function as a religious local centre. Nevertheless, during the 3rd century one person, quite possibly a descendant but at least in the line of successors of the first native farmer, controlled the settlement to such an extent that he could plan its reconstruction, build himself a fairly luxurious home, and relocate all agricultural facilities at the two other, subordinate, households. The settlement thus became a centrally organized whole, directed by a person purposely striving for a Roman standard of living and therefore, as Bloemers and Van Es did not fail to notice, was essentially a villa. There are still a few problems with this designation, because not all technical and social aspects of the concept of villa are readily applicable. The absence of a bath, for example, is conspicuous, but that may have been simply due to technical or financial difficulties. Also, the dominus of the main house may not have been the owner of the settlement, which is suggested by its rather gradual development. He may have been a primum inter pares, possibly a lineage-head, and certainly the village chief, but it is very questionable if he owned the settlement in the sense that it could be bought and sold.

It is conceivable that his descendants would have reached that position (if it is at all vital for the interpretation of the site) in the 4th century if developments had continued unchanged. As it is, they were interrupted about or shortly before AD 270, and the (embryonic?) villa that Rijswijk had become, could not develop further. It is, however, still appropriate to call it a villa. For the present discussion, it is especially relevant that the concept can only be applied during the final phase, from c. AD 200-270. Building in stone occurred at the same time as the other relevant changes and was directly related to them.

Although the Rijswijk example does not constitute incontrovertible evidence for the significance of stone buildings in unexcavated settlements, it provides strong support for the hypothesis that such buildings are evidence for a villa. In combination with the other settlements discussed so far, it also shows that there are, in the river area, at least three types of villae:

1 Type Mook: a large and relatively luxurious (Gallo-Roman) building newly erected during the Middle-Roman Period and presumably either the centre of a fairly large estate or a country house.

2 Type Druten: a modest and less luxurious (Gallo-Roman) (cf. Overasselt) or romanized native (cf. Druten)
building as the main house of a planned, new settlement during the Middle-Roman Period.

3 Type Rijswijk: a main building similar to 'type Druten' but heading a reorganized, pre-existing settlement usually beginning before the Middle-Roman Period. To some degree, it is possible to ascribe the unexcavated villae in the eastern river area to these types on the basis of surface material. Sites with occupation starting in the Flavian period or later would then be assumed to be comparable to Druten or, in special circumstances, to Moork; those with a longer occupation to Rijswijk. Such an exercise would, however, be of no great value because there are usually too many uncertainties involved. Not every villa site that begins in the Flavian period necessarily started as a villa. Conversely, older sites which seem to be occupied continuously (cf. fig. 23) may in fact have been deserted for several decades and then reoccupied to build a fully fledged new villa. Even the presence of material from all successive typochronological 'horizons' is no guarantee for absolute continuity because especially during the 2nd and 3rd centuries these horizons are rather broad. Nevertheless, it is useful to examine briefly those settlements which are presumably villae, in addition to sites 214, 222, 355 and 377.

Site 43, Kesteren—De Hoge Woerd. Very limited investigations in 1904, 1960, and 1961 produced no traces of buildings. In 1961, a Roman-Period well was discovered. The site was occupied from the Late-Iron Age onwards and yielded fragments of tiles (including one with a military stamp) and slate, tuff, and plaster with wallpainting. It is noteworthy that De Hoge Woerd is one of the few sites with some extraordinary late-Iron Age artefacts, presence of early-Roman pottery, and evidence for a villa. It may have grown into a villa fairly quickly, possibly during the Flavian period, for which the tile-stamp (probably of the 10th legion) provides some additional evidence.

Site 513, Kesteren—Broekdijk. Limited surface material found in 1981 on a previously known settlement soil included tuff, limestone, quartzitic sandstone with traces of cement, and tiles. The site was already occupied during the Late-Iron Age. The available Roman pottery is datable to the Flavian period and later.

Site 59, Hien—De Wuurdjes. A vast amount of material was salvaged during the stubbing of an orchard, but no excavation was carried out and traces of buildings were not found. The material included tuff, limestone (a.o. a possible fragment of a column), slate, tiles (one with an LXG stamp), fragments of opus signinum, and at least one tubulus. The settlement must have been a villa and the quantity of the finds permits its construction to be dated to the Flavian period, without previous occupation at the site. The settlement seems to have existed until the very end of the 3rd century, but not into the 4th. A single late-Merovingian sherd must be regarded as a stray find, possibly indicating quarrying for building material, not as evidence for continued occupation.

Site 80, Andelst—De Hoge Hof. The limited surface material includes some tuff and other stone. Evidence for pre-Roman occupation is lacking. On the other hand, there is late-Roman, 5th-century, and Merovingian material, as well as some later medieval sherds. The latter may have landed there through manuring in the Middle Ages, but they make an interpretation as villa less certain.

Site 81, Andelst—dorp. The large amount of mainly surface material indicates occupation from the Middle-Iron Age up to the present day. Although tuff was found at this site, a medieval context is just as likely as a Roman stone building. In the absence of more detailed data, the presence of a villa is a mere possibility.

Site 83, Herveld—De Woerd. There is only limited surface material, indicating occupation from the Middle-Iron Age onwards, with a hiatus in the Late-Roman Period. The tuff may be Roman but also later. Like site 80, it is possible that the late-medieval sherds were contained in manure, but that is not certain. In any case there is a 19th-century report mentioning tiles, tuff, and even pieces of marble.

Site 85, Herveld—De Legt. The surface material covers all periods from the Late-Iron Age onwards. There are also Merovingian weapons, probably indicating burials. The amount of late-medieval material is considerable, which makes the presence of tuff less significant. In addition to tuff and tiles, pieces of tubuli and Roman cement are present. Nevertheless, the presence of a villa remains doubtful.

Site 97, Homoet—De Hoge Woerd. The evidence is comparable to site 83, but the total amount of finds is even less and the presence of tuff not securely established. A villa is, therefore, quite uncertain.

Site 99, Elst—Grote en Kleine Zuiling. During soil removal in 1972, several sorts of building material were discovered at this site, including tuff, tiles, tubuli, cement, and slate. A limited number of sherds indicates occupation during the end of the 2nd and early 3rd centuries.
only. The material is certainly indicative of a villa, but the site is somewhat curious because an ancient settlement soil is lacking. As mentioned in chapter 3 (p. 74), it is possible that we are dealing with a very short-lived villa, which may explain the absence of the characteristic settlement soil. If that were true, it would be the only example of a clearly unsuccessful villa as an agricultural enterprise. Unfortunately, although the dating evidence supports such a notion, its quantity is too limited to be acceptable as proof.

Site 109, Raaijen–De Woerd. Apart from some surface material, most of the finds from Raaijen originate from a very small excavation in 1929, which was never published. The recorded features consisted of ditches and post-holes, but the finds include, in addition to various sorts of tiles, fragments of tuff and limestone, some of them identifiable as parts of ornaments. The apparently native character of the settlement (there are hundreds of sherds of late-Iron Age and Roman Period native pottery) may be a result of the location of the excavation trench, but it is possible that we are dealing with a villa of the ‘Rijswijk’ type. The excavation material also includes late-medieval pottery.

Site 212, Druten–Brouwerstraat. A fairly large number of sherds from this site is proof of occupation from the Late-Iron Age to the Middle-Roman Period, but presumably not extending into the 3rd century. Stratigraphical observations in 1971 showed three separate occupation layers, the lowest containing only native sherds, the middle one with Roman finds, including much tuff, slate, and tiles, and the top one with some early-, but primarily late-medieval pottery. Although the total collection of material contains quite considerable amounts of late-medieval pottery, the limited stratigraphical data indicate that the building material belongs exclusively to the Roman Period occupation. Traces of foundations are lacking, but the available evidence makes a villa quite probable. The settlement is located on the northern part of a small Pleistocene dune (donk).

Site 219, Deest–Grotestraat. The settlement was discovered during construction work in 1972. The salvaged material includes tiles, tuff, quartzitic sandstone, slate, opus signinum, and painted wall plaster. Although late-medieval sherds are present, the presence of a villa was established beyond doubt. In two adjacent construction trenches, the corner and an 18-m-long stretch of a gravel foundation was recorded. The dating evidence indicates occupation from the 1st to the 3rd centuries but the stone structure must have been built relatively late because the foundation cut through an earlier refuse pit filled with 1st- or early 2nd-century material. As on site 59, a single late-Merovingian sherd is a stray find, possibly indicating late-7th- or early-8th-century quarrying activities.

Site 220, Deest–De Hosterd. Surface finds in 1973 included some tuff which may, however, be medieval or related to site 219 only 300 m away. It is quite likely that both sites constitute one settlement but definite proof is lacking. In any case there is a difference in dating because site 220 yielded late-Iron Age pottery and site 219 did not.

Site 234, Ewijk–De Hoge Woerd. Especially during the digging of new ditches in the early 1970s, large amounts of settlement debris were collected at this site. The dating evidence indicates occupation from the Middle-Iron Age onwards; stratigraphical observations showed that the site was raised by 0.5 m only during the later Middle Ages. The tuff and tiles are not likely to belong to that phase and the substantial post-Roman clay cover may explain why the precise location of the Roman stone building has not been located so far.

Site 239, Ewijk–De Grote Aalst. After deep ploughing in 1977, and again in 1982, large quantities of material were collected and several concentrations of building material were recorded. These produced tuff and various other sorts of stone, including slabs of limestone and even marble, cement, opus signinum, tiles, tubuli, painted plaster, and, as a feature unique in the river area, pieces of a black-and-white mosaic. Even though this is by far the most luxurious villa in the river area known to date, the site itself was already settled during the Middle-Iron Age and continuously occupied until the end of the Late-Roman Period. Most of the pottery, however, dates to the period between c. AD 40 and 270. The settlement, like no. 43, belongs to the category with extraordinary late-Iron Age material, in this case Celtic coins (rainbow-cups), and some early-Roman pottery. In addition, the site has yielded an astonishing amount of late-Roman coins.

Site 245, Beuningen–De Hosterd. Only a modest amount of Roman finds is known from De Hosterd. Building materials include tiles, cement, and some tuff, but on account of the later medieval occupation on and around the site, the presence of a villa is doubtful.

199 Braat 1937, 23, mentions the excavation.
200 Van Kouwen 1972, 11, fig. 1. See also chapter 6, 184.
201 See below, 315 and 446–9.
Site 248, Beuningen-dorp. The situation is similar to site 245, although the building material is somewhat more numerous. A rich Merovingian burial was discovered at this site, which is otherwise characterized by an especially large ancient settlement soil (probably partly medieval) with several findspots of Roman material situated fairly far apart. Observations in trenches in 1969 allow the conclusion that some of the intermediate space was definitely not settled in the Roman Period.

Site 250, Lienden-De Reekstraat. On the large, elongated ancient settlement soil along the Reekstraat, several concentrations of habitation have been established, the earliest dating to the Middle-Iron Age. Roman material is found over the entire surface, but observations at the surface and especially in ditches dug in 1971 and 1973 indicated Roman stone buildings on the eastern part. The collected building material includes tuff, limestone, quartzitic sandstone, slate, tiles, and tubuli. Chronologically, the settlement is comparable to site 239, because there is evidence for occupation from the Middle-Iron Age onwards and most of the Roman finds date to between AD 40 and 270. There is also evidence for late-Roman and no indication for medieval occupation.

Site 275, Hernen-De Wijnakker. The site was only discovered in 1975. The considerable collection of finds indicates occupation from the Late-Iron Age to the end of the Middle-Roman Period. The late-Iron Age settlement could be a relatively special one because of the large numbers of La Tène glass bracelets discovered there: 37 specimens in the first year and currently over 100. The material includes tuff and tiles; borings in 1975, at the largest concentration of building material at the surface, indicated a stone building of approximately 11 x 25 m. Although recent finds include late-medieval sherds, these are not related to the building.

Site 291, Hernen-De Loffert IV. The site was occupied from the Late-Iron Age until the Merovingian Period. At one spot, stratigraphical information indicates a hiatus between two occupation layers, datable to the Late-Iron Age and the Roman Period. Although no concentrations could be recorded at the surface, the collected material includes tuff in addition to the more common tile-fragments.

Site 298, Wijchen-De Pas, Passerot I. The settlement is part of a conglomerate of sites situated fairly close together but with different datings (cf. chapter 5). A vast amount of material has been collected at site 298 alone, including an equally large number of tile-fragments, two of which had military stamps, of the vexillatio Britannica and the legio XXX Severiana Alexandriana. The other finds include, for example, Roman window-glass, but in view of the large collection only little tuff and no other building material. It is possible that building activities in stone were minimal, cf. the main house in Druten, but the absence of more tuff or other stone fragments in such a large collection is too curious to accept the presence of even a simple villa except as a mere possibility.

Site 341, Luen-dorp. Most of the fairly numerous finds were collected during sand-winning. They indicate late-Iron Age to middle-Roman occupation and include tuff, limestone, window-glass, and tiles, one with a stamp of the legio X gemina. Unfortunately, there are also clear indications for medieval occupation and in the absence of actual traces of Roman stone buildings the presence of a villa cannot be established beyond all doubt.

Site 358, Overasselt-Valenberg. There are only a few finds from this site, located only 300 m northeast of the excavated villa at the Scheiwal (site 355). They include mostly limestone, quartzitic sandstone, and numerous tiles, with only a few sherds. Although this material probably indicates a Roman stone building, it is also quite probable that it should be considered part of the same settlement as site 355.

Site 424, Beek-Ravenberg and site 425, Beek-Kalorama. These sites are located very close together (less then 100 m) but they stand on opposite sides of a steep valley that may not, however, be entirely natural. It is uncertain whether they should be considered one settlement. In any case, there is little doubt about the presence of middle-Roman stone building at both sites. The material includes tuff, cement, tiles (one with a military stamp y[)], hypocaust tiles, and a fragment of a brick tube. In view of the location of these buildings, they may well be comparable to the villa at Mook.

Site 426, Beek-Keteldal. The finds consist of several blocks of building material with cement, washed down the slope of the ice-pushed ridge in 1940. They presumably indicate a villa as on sites 424/425, but it has not been localized precisely. Datable sherds have not been reported.

Site 439, Millingen-Nieuw Zeeland. Although an apparently unsuccessful and in any case unpublished excavation was carried out here in 1936, little material is known. Nevertheless, there is a reliable report by M.P.H. Daniëls, mentioning a large quantity of tuff, tubuli, and tiles. It is possible that these materials were
in secondary position and related to a former chapel at this spot, but sufficient data are lacking. The presence of a villa at Nieuw-Zeeland is only a possibility.

Site 441, Millingen–Eversberg. This site has already been discussed as a possible military site (watchtower) which is, on the basis of the available evidence, not very likely. In any case, the 19th-century excavations did show the presence of a stone building of c. 12 x 15 m, which may have been part of a villa. The site also yielded evidence for late-Iron Age occupation, and in the Merovingian Period it was used as a cemetery.

Site 453, Kleve–Kanalstrasse. The little material discovered here during construction work in 1951 may indicate the presence of a villa because the finds include tuff in addition to tiles. The information is too scanty to warrant any definite conclusions.

Site 492, Escharen–Graafsche Raam. In addition to some Merovingian material, there are older reports on Roman building material, including tuff, from this site. If at all reliable, the finds could be related to a recently excavated early-medieval settlement somewhat further north. The available evidence, which could not be verified properly, is hardly enough to consider a villa a remote possibility.

Site 508, Middelaar–Witteweg. The fairly large amounts of material, gathered here during various sorts of construction work in 1980 and 1982, include tuff, hypocaust tiles, and numerous other tiles. Although some stray medieval and especially sub-recent material is present, the concentrations of Roman artefacts indicate the presence of one or more stone buildings. The settlement was occupied from the Late-Iron Age to the end of the Roman Period.

The available evidence shows that there are, in the eastern river area, a maximum of 34 villae. Among these are at least 18 sites or combinations of sites which can be interpreted as a villa with a reasonable degree of confidence based on finds as well as find-circumstances: nos. 43, 513, 59, 99, 212, 214, 219-220, 222, 234, 239, 250, 275, 341, 355/358, 377, 424-425, 441, and 508. As far as the location of villae is concerned, there are several different groups. Site 513 is the only villa which is close to the Rhine, or rather to the fort Carvo (37). Site 99 lies in the vicinity of the vicus at Elst (105) and simi-larly, sites 275, 341, and 355/358 are likely to be related to the vicus at Wijchen (315). Site 508 may be related to Cuijk (500) and the same may apply to site 377 but that villa may also belong to a special group with 424-425, located in scenic positions on the slopes of the ice-pushed ridges and conceivably related to Nijmegen.

By far the largest group, however, is related to the Waal (43, 59, 212, 214, 219-220, 222, 234, 239, 250, and 441) and thereby provided with a direct connection to Nijmegen. Although this does not necessarily imply a direct, let alone an exclusive relation with Nijmegen for each of the sites concerned, or an absence of such a relation for the other sites, the general distribution of villae is clearly indicative of important trade relations with Nijmegen. When the more doubtful villa sites are also taken into consideration, this pattern emerges even more clearly.

There is, of course, nothing really surprising about this, because the presence of Nijmegen as a social and economic focus in the area was one of the main reasons to assume that villae could be present. As such, their distribution is just another example of a pattern familiar from elsewhere. On the other hand, the fact that the theoretically expected distribution emerges so clearly is in itself additional justification for the criteria used to define individual sites as villae.

Other settlements
Unlike the types of settlements discussed so far, the remaining and by far the largest group of settlement sites is only partially identified by positive criteria, by the presence of certain characteristics. A large number of sites is included simply because they lack all characteristics as a result of the low level of information about them. Although the implicit assumption that special traits are indeed lacking will generally be true, it is, of course, also true that some cases will eventually prove to be quite different sites, such as villae, small military posts, or some other constructions.

For the present discussion of settlement typology, this problem is only relevant when individual sites have to be interpreted; the evaluation of different types of settlement depends entirely on sites which have been more thoroughly investigated.

Even for these investigated sites the traditional and almost unavoidable way of thinking is partly based on neg-
native argument. They are generally termed 'native' and that designation at least partially refers to the absence of 'Roman' attributes. In that sense, terms like humble, poor, less sophisticated, and the like quickly come to mind and are indeed often used. They reflect explicitly the same appraisal which is also encountered in more disguised or unintended forms such as the structure of the present chapter: the succession in which settlement types are discussed! In principle there is, of course, nothing wrong with an ordering of data along those lines which are the most easy to handle and are fruitful for analytical purposes. There is even nothing to be said against the use of terms like poor or humble as long as they are based on an analysis of absence or presence of distinctive attributes. The only trouble is that such terms are also used in a traditional type of archaeological reasoning, which is entirely focused on Roman culture and history and its more prominent manifestations in the provinces, and which is still very much alive. Within such a framework, not only is the presence of diverse native elements easily overlooked but the significance of their absence is not fully appreciated either. A clear example of this are the early-Roman settlements 403 and 417 discussed above. Whatever their true nature may prove to be, on the basis of the historical and presently available archaeological evidence not only the 'Roman-ness' but especially the utter 'non-nativeness' of both sites may lead to fruitful insights into their function in the region and the initial character of the Batavian elements in the river area.

The lack of native artefacts and the absence (403) or disputability (417) of native structures on these Nijmegen sites are, at the same time, two indicators which can be used positively in the recognition of 'real' native settlements. They depend, however, on a high level of investigation of a site. That may be obvious for structural details which can only be observed after excavation as long as remote sensing techniques are not successful. But it also applies to artefacts, usually limited to pottery. The significance of the relative proportion of native pottery in surface collections is very difficult to assess for several reasons. First, it is always very difficult and often completely impossible to separate Roman Period from late-Iron Age material, and then the fact that characteristic middle-Roman native wares are non-existent is even ignored for convenience. Second, as far as selective de-

struction is at all important, native ceramics disintegrate more quickly than Gallo-Roman pottery. Third, surface collections are definitely biased. Brightly coloured Roman sherds are more easily noticed than the grey or black native material and the experience of the collectors varies. Perhaps even more important is that sometimes the effort spent on collecting native sherds is definitely less than for Roman finds.

These practical difficulties are sufficient to reject all plans to use the quantity of native material as a basis for classifying a settlement, and then the major question of the implications of such a classification has not even been asked. For the Early-Roman Period, when wheel-turned pottery cannot have been widely available, it is evident that the presence of Roman finds or the near absence of native ceramics are suitable to identify the new 'Roman' settlements and possibly also those native sites whose inhabitants were the first to acquire the new materials, the implication being that they are the most important in a social and/or economic sense.

For the Middle-Roman Period, such a line of reasoning cannot be used. On the one hand, the occurrence of very precisely datable types of Roman pottery indicates a strongly increasing general availability, (cf. p. 240), and on the other the use of native pottery diminishes rapidly during the 2nd century (cf. paragraph 6.3). The significance of the ratio of Roman v. native pottery may be quite varied, but on the basis of the present data and on the regional level of analysis it is primarily chronological. It has been explicitly used as such (cf. chapter 6, note 202), which makes it unsuitable for comparisons between middle-Roman sites.

Just as information about structural details, regional data on native pottery can thus only be derived from conclusions reached at the site level of analysis. It is very unfortunate that, while several large or even very large excavations of native settlements in or very close to (see also fig. 80) the river area have been carried out during the past 15 years, virtually nothing has yet been published about the finds. Some of these excavations have only recently been completed, but for all of them there are fairly general preliminary reports which at least give some idea of the structure of the settlements involved. These are Ede-Veldhuizen (site 1; 7.5 ha), Bennekom-Achterstraat (site 8; 2 ha), Heteren-Het Lage Land (site 92; 1.35 ha), Ewijk-Ewijksche Velden (site 182; 2.1 ha), Oss-IJsselstraat (site 487; 0.5 ha), Oss-Ussen (33 ha), and Wijk bij Duurstede-De Horden (excavation still in progress, to date 11 ha). The excavation of Rijs-
wijk-De Bult (3.5 ha), which was also a native settlement until the end of the 2nd century, has been fully published.

An overview of the structure of middle-Roman native settlements has already been provided by Van Es. In accordance with his analysis, it is possible to conclude that real villages, in the sense of nucleated and structured inhabited areas, are absent among the native settlements south of the Rhine. There are, of course, the vici, but these are not native. Most settlements are fairly small, being single farmsteads or consisting of two or three contemporary farms, such as Rijswijk-De Bult before the 3rd century. There are also larger settlements such as those in Wijk bij Duurstede-De Horden and, presumably, Oss-Ussen. From a regional perspective, these places definitely represent a clustering of occupation. At the site level, however, clustering is not so obvious. The published data from Ussen do not allow discussion here, but Van Es' remarks on De Horden indicate that the site is a very loose conglomerate of single farms, and small groups of farms, dispersed over a wide area as far as it was suitable for habitation, the only unifying element being a parcelling system of ditches on habitable parts of the stream-ridge.

A settlement such as De Horden thus has a very open structure and does not exhibit features pointing to some form of central organization. It may properly be called a 'dispersed settlement' type of habitation. In the eastern river area, there are also a few conglomerates of sites which may well indicate such dispersed settlements. Examples are sites 230–232, or perhaps even 227–232, south of Ewijk, 296–303 north of Wijchen, and 507 and 525 in Middelaar. Although the excavations at site 232 were too limited to contribute much to such an interpretation, the evidence from De Horden at least suggests that a seemingly isolated small settlement may actually be part of a larger dispersed settlement. In theory, the lack of nucleation and ordering could indeed lead to 'one' dispersed settlement of more than one kilometre in length as is suggested by the chain of sites south of Ewijk (with many stray finds in between). In any case, the situation there is contrasted sharply by other chains of sites such as 138–144 east of Elst. These are a very clear example of small native sites separated by regular intervals of 600–1000 m so that there is no doubt that each site represents a different settlement.

The designation of clusters of sites as dispersed settlements is, unfortunately, much more tentative when it has to be based on surface collections or small excavations. De Horden has shown, however, that the dispersed settlement as a separate type of habitation does exist, and that it is not comparable to the large nucleated native settlements north of the Rhine, which can be properly called villages. The settlement of Wijster (see fig. 80) is the best known example of such a village, with a considerable number of contemporaneous farms arranged in a more or less orderly fashion on a limited area and some form of central authority located in one relatively large and wealthy farm (Herrenhof). There

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205 Van Es 1977, 120–5, esp. 125 and 1981, 158–81, esp. 159 and 169–70.
206 See chapter 5, site 231 (situation of the site) and p. 117, note 23.
207 I am grateful to W.A. van Es for discussing with me his provisional conclusions regarding his excavation of the site which was in its final phase at that time.
are comparable villages further to the north on the North Sea coast, but these are terp-villages and a site like Wijster could be a more suitable example because the nucleation on terp sites may to some extent be attributed to the particular problems posed by the landscape. But it is not even necessary to look for examples of large native settlements too far afield because they have even been found immediately north of the Rhine in the area covered by the present study. Although only provisional data are available (cf. chapter 5), the settlement in Ede-Veldhuizen (site 1) and possibly that in Bennekom-Achterstraat (site 7) should be included in this category.

It is important to note that the sharp division between types of middle-Roman settlements north and south of the limes thus also included native settlements, in addition to the obvious absence v. presence of non-native settlements. Possible implications of this conclusion are discussed in chapter 11.4.2.

Although the large village communities north of the Rhine represent an organized whole that goes beyond the smaller settlements which occur on both sides of the frontier, all are essentially composed of the same elements. These basic habitation units consist of a farmhouse with a number of related features which need not be present in each case, such as a granary, an outhouse or (north of the Rhine!) a sunken hut, and a well, and usually surrounded by a ditch or a fence. Two examples of such yards (erven), from Rijswijk-De Bult and Bennekom-Achterstraat, are illustrated in fig. 81, but others have been found over a much wider area and in various 'cultural' and chronological contexts.

As far as the eastern river area is concerned, virtually all middle-Roman native settlements probably consist of one or a few of such household-units. Examples are the farms excavated in Ewijk (232) and Oss-IJsselstraat (487), but in all probability also the post-hole swarms from Heteren (93 and 95), Zetten (75), Bemmel (143), and Ressen (150 and 151). After all, at the best known of these sites, clear groupings are discernable (cf. the quote on page 224 above).

Nevertheless, the continued presence of the post-hole swarm type of settlement into the Middle-Roman Period indicates the continuation of a difference between two types of small native settlements. For the proposed interpretation of this fact, namely, a difference in subsistence-strategy, it is very important that the small houses of the post-hole swarms apparently disappeared during the Middle-Roman Period. The settlement in Heteren (93) was deserted by the middle of the 2nd century and the same is true for that in Ressen-De Woerd (151).

The excavations in Ewijk (232) have shown that the Iron Age post-hole clusters were replaced by clear Roman Period house-plans situated at the edge of the terrain inhabited during the Iron Age.
The evidence from the trenches dug at Heteren-Uilenburg (95), Bemmel (143), and Ressen-Kerkenhof (150), cannot be reliably evaluated. Only Zetten (75) may provide a case against a proposed termination of the constructional tradition leading to the archaeological phenomenon of the post-hole swarm in the early part of the 2nd century. The finds seem to indicate more or less continuous occupation (cf. fig. 23), but it is possible to disregard this suggestion because the vast majority of the pottery consists of native wares and most of the sparse Gallo-Roman pottery is datable from IB-IIA. One might, therefore, argue that the site was abandoned around AD 150 to be reoccupied again only in the 4th century, and attribute the few sherds which chronologically could cover the intermediate period to the mid-2nd century. They are then taken to mark the end of the first occupation, which, in fact, conforms exactly to the interpretation of sites 93 and 151. On both these sites the very few possible later 2nd- to 3rd-century sherds are undoubtedly correctly interpreted by the excavators as marking the end of the occupation around AD 150 when these types (Niederbieber 89 or 104) first appeared. The only difference is that these two sites were never reoccupied. The somewhat formalized dating procedure followed in chapter 4 led to a suggestion of continuous occupation for site 75, which may not be justified in view of the above argumentation but which had to be maintained because of possible additional evidence in this case. 209

So far, the available evidence thus suggests rather strongly that the settlements with irregular house-plans did not continue throughout the Middle-Roman Period but that the last of them disappeared around the middle of the 2nd century. This conclusion as such supports the interpretation of these sites as a different type of settlement from those with recognizable house-plans, which continued to exist as can be demonstrated by excavations such as those in Ewijk (232) and Oss (487).

If the tradition of building small and irregular houses is indeed functionally determined by a specialized subsistence-strategy, then the reasons for the disappearance of this type of settlement should also be at least partly economic in nature. This is a matter that will be returned to in chapter 11, but it is clear that such reasons are not difficult to suggest. If native settlements which specialize in agricultural production finally disappeared in the first half of the 2nd century AD, the most likely explanation must be that they were replaced. The alternative that they were no longer necessary can hardly be true (at least when they are evaluated in purely economic and not in social terms) because the need for production of grain and other crops or of meat in the region around Nijmegen could only have increased and not decreased. Replacement can only occur when something better is available or, perhaps more to the point, when the need for something more effective arises, and that is precisely what can be observed. The villae in the eastern river area, insignificant as they may be from an architectural point of view, surely represented the centres of much more effective organization of labour and exploitation of the land. They were first established, or in other cases (and perhaps somewhat later) began to be formed as a result of a reorganization of existing settlements, in Flavian times. But the heyday of the villa system is in the 2nd century, and that must be the main reason for the disappearance of specialized native agricultural settlements. There must have been other reasons, such as the ‘pull’ of a flourishing town and vici and the increasing adoption of Roman norms and values, which perhaps motivated the rich Batavian to invest in a villa and the modal Batavian to at least no longer wanting to live in a crummy 9-post house. But all such reasons are related and can be attributed to a general process of social and economic development in the second half of the 1st and first half of the 2nd centuries which led to the rise of one settlement type and the disappearance of another.

Apart from the settlement-types discussed so far, there may be others which cannot yet be recognized as such. An example is the excavated site 158 in Lent,210 which is essentially a household-unit as described above: a house with associated features such as a granary and a well. The site was occupied from the Late-Iron Age (cf. p. 224) until the 3rd century, and may have been a single unit or have belonged to a larger dispersed settlement (156, 158). The excavated household-unit itself has been dated from c. AD 70-150 or a little later.

In contrast to other native sites, the building from Lent is a fairly large (11 x 27.5 m) one-aisled structure and represents a clear adoption of Roman building methods (see fig. 79). Similar buildings have been found else-

209 The terra sigillata sherd Drag. 37 (fig. 28, 56) from the Argonne belongs to the ware with ovolo G which might be too late to allow a dating around AD 150. The arguments for its

chronological position (Haalebos 1977, 119-20) are, however, not beyond all doubt.

210 Van Es/Hulst, in prep.

125 (285)
Fig. 82 The composition of the pottery assemblage at 23 sites.

<table>
<thead>
<tr>
<th>Site</th>
<th>Interpretation</th>
<th>Roman Pottery</th>
<th>Roman: Native wares</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td></td>
<td>Terra Sigillata</td>
<td>Colour Coated Ware</td>
</tr>
<tr>
<td>1 settlement</td>
<td></td>
<td>8 7</td>
<td>1 1</td>
</tr>
<tr>
<td>38 vicus</td>
<td></td>
<td>26 8</td>
<td>23 7</td>
</tr>
<tr>
<td>48 settlement</td>
<td></td>
<td>26 13</td>
<td>19 9</td>
</tr>
<tr>
<td>59 villa</td>
<td></td>
<td>21 7</td>
<td>31 10</td>
</tr>
<tr>
<td>75 settlement</td>
<td></td>
<td>2 3</td>
<td>1 2</td>
</tr>
<tr>
<td>93 settlement</td>
<td></td>
<td>2 3</td>
<td>0 0</td>
</tr>
<tr>
<td>105 temple (vicus)</td>
<td>1 1</td>
<td>1 4</td>
<td>5 3</td>
</tr>
<tr>
<td>109 settlement (villa?)</td>
<td>0 0</td>
<td>3 3</td>
<td>20 21</td>
</tr>
<tr>
<td>117/8 castellum + vicus</td>
<td>56 14</td>
<td>37 9</td>
<td>72 17</td>
</tr>
<tr>
<td>126 castellum</td>
<td></td>
<td>78 26</td>
<td>22 7</td>
</tr>
<tr>
<td>194 castellum + vicus</td>
<td>26 12</td>
<td>4 2</td>
<td>11 5</td>
</tr>
<tr>
<td>214 villa</td>
<td></td>
<td>56 6</td>
<td>53 5</td>
</tr>
<tr>
<td>232 settlement</td>
<td></td>
<td>71 10</td>
<td>40 5</td>
</tr>
<tr>
<td>234 villa</td>
<td></td>
<td>9 9</td>
<td>8 8</td>
</tr>
<tr>
<td>250 villa</td>
<td></td>
<td>17 11</td>
<td>13 8</td>
</tr>
<tr>
<td>275 villa</td>
<td></td>
<td>17 7</td>
<td>2 1</td>
</tr>
<tr>
<td>289 settlement</td>
<td></td>
<td>19 15</td>
<td>23 18</td>
</tr>
<tr>
<td>298 settlement (villa?)</td>
<td>17 9</td>
<td>21 11</td>
<td>28 15</td>
</tr>
<tr>
<td>315/6 vicus</td>
<td></td>
<td>29 8</td>
<td>14 4</td>
</tr>
<tr>
<td>341 settlement (villa?)</td>
<td>4 3</td>
<td>4 3</td>
<td>9 7</td>
</tr>
<tr>
<td>407 canabae (west)</td>
<td>210 14</td>
<td>30 2</td>
<td>79 5</td>
</tr>
<tr>
<td>412 castra</td>
<td></td>
<td>367 20</td>
<td>52 3</td>
</tr>
<tr>
<td>416 canabae (east)</td>
<td>868 17</td>
<td>246 5</td>
<td>238 5</td>
</tr>
</tbody>
</table>

where in association with villae, such as the wooden
building preceding the villa of Kaalheide (prov. Lim-
burg) or building II at the villa of Garsdorf, Kr. Berg-
heim (BRD). Comparable from a building tradition
point of view are also several one-aisled structures in
Druten (see fig. 78).

Although there are no arguments to consider site 158 a
villa, it is not a normal native settlement either. The
rather large granary associated with the building adds
further weight to this conclusion. But there are not
enough data for a clear interpretation of the site, let
alone for discovering others with the same characteris-
tics. We may be dealing with in some respects a 'roma-
ized' native settlement, with a kind of native proto-villa,
or with something else. In this respect, there may be

some direct connection with soldiers or civilians from
Nijmegen on the other side of the river. But we do not
know that and there is no way of testing hypotheses in
that or other directions. For the moment, we therefore
have no reason to consider site 158 a socially and eco-
nomically different kind of settlement, and thus a differ-
ent type.

But the settlement in Lent does illustrate that there may
be a much larger variety than we are able to distinguish
at present. For the moment, the analysis of native settle-
ments cannot go beyond the recognition of four different
types of sites. All of these are essentially composed of
one or more basic household-units. The differences be-
tween the types are either related to the ordering and
number of these units or to constructional differences
between buildings which (may) have social and economic
implications.

Type 1 Nucleated villages with a considerable degree of

211 Kaalheide: Brunsting 1950; Garsdorf: Piepers 1959 (see
fig. 80).
8.4.3 Quantitative Analysis of Settlement-Finds

In the introduction to this chapter, the question of quantitative measures has already been raised. It would be useful if it were possible to characterize a type of settlement by the relative proportions of various artefacts, especially different kinds of pottery. A first approach to this issue has been included in chapter 6, in the discussions of various types of pottery, building material, and glass. It was, however, based on raw scores (numbers of artefacts of type x at site y) in relation to other attributes of sites, such as the presence of stone buildings.

It is necessary, therefore, to examine more thoroughly the feasibility and significance of quantitative measures. The potential usefulness of comparing the relative proportions of different kinds of pottery on different sites has already been shown elsewhere, such as the histograms of pottery from York and from a number of Dutch settlement sites. Of course, it is possible to use other artefacts as well, but those are normally only available in sufficient quantities after excavation, and even then their numbers are often quite small. It is conceivable that increased use of metal detectors will produce a large enough data base of metal finds in the future, but it is not available yet.

The present analysis is thus limited to pottery, but in order to achieve useful results a few other restrictions must be imposed, namely, spatial and chronological comparability. The first of these conditions is easily met by including only sites from the eastern river area, the micro-regional level as defined in chapter 1. It would be very worth while to include other sites from close by or totals are: t.s. 83 (10%), colour-coated 47 (6%), Belgic ware 59 (7%), smooth ware 217 (27%), coarse ware 399 (50%), with N = 805 and Rom.: native Rom. sherds as > 9: < 1. It is clear that these new figures would only very slightly – but, as far as interpretations are concerned, in a positive way – influence further calculations below.

8.4.3 Quantitative Analysis of Settlement-Finds

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further afield for comparison, but such a step would introduce too many uncontrollable variables at this point such as distances from production centres, to mention only one important factor. For the same reason, the results of the analysis should not be compared unconditionally to those from similar quantitative studies in other areas.

Chronological comparability is achieved automatically here, because there are virtually no sites with sufficient data from the Early- or Late-Roman Periods. The point is, nevertheless, theoretically important because the same problems arising from a comparison between sites in different regions are met with settlements in the same area but under very different circumstances in the successive stages of the Roman occupation. Apart from this, chronological comparability also poses a practical problem. When all the pottery of a site is considered and minor errors resulting from incorrect datings of wheel-turned pottery are left aside, there are still the handmade native wares. It is already difficult to separate late-Iron Age from Roman Period native pottery, but it is impossible to differentiate between native pottery from the Early- and the Middle-Roman Periods.

For this reason, only the potential information contained in the relative quantities of wheel-turned pottery can be evaluated statistically. Although difficult to handle, it is nevertheless important for the final interpretation to know the ratio of wheel-turned (henceforth termed 'Roman' for convenience) to hand-made pottery at each site. These ratios are therefore included in fig. 82, in two columns: one for Roman Period settlements only (Roman: Native Roman) and one for settlements with a longer occupation (Roman: Late-Iron Age + Native Roman).

The data in fig. 82 were collected from various sources but, although some of them are only provisional, there is no reason to suspect that these are less reliable. The relative quantities have been calculated by counting rim-sherds, except for some categories of smooth ware: flagon neck-fragments and handles have also been counted as rims to reach a more balanced estimate of the percentage of smooth ware in relation to other Roman pottery, where breakage produces many more rim-fragments. Special categories of pottery which occur in minute quantities (cf. paragraph 6.2.6) have not been considered in fig. 82. The so-called 'Holdeurn ware', which is sometimes distinguished from other pottery, is not considered separately but (depending on the paste) divided between the smooth ware and the coarse ware, or omitted (the fine Nijmegen ware).

The number of sites in fig. 82 is limited, because the only sites included are excavated sites from which the data could be obtained, or surface collections with $N > 100$ for Roman pottery. Fortunately, however, most of the different settlement types as discussed in the previous paragraph are included.

Examination of the various Roman wares in fig. 82 immediately reveals that sites 105 and 289 are very different from the others. Site 105 scores extremely low on both terra sigillata and coarse ware, and extremely high on smooth ware. This anomaly may almost certainly be attributed to the fact that the finds are all from the excavation of the Gallo-Roman temples at Elst. It is not surprising that the pottery assemblage at such a site is different from all others.

Site 289 has a very abnormal percentage of colour-coated ware and far too small amounts of smooth ware. For a site without any special characteristics the percentage of terra sigillata is also surprisingly high, as well as the ratio of Roman to native wares, especially because they include late- and even middle-Iron Age material. In this case, there is no obvious archaeological explanation for these anomalies, so it is assumed that they result from a strongly biased sample due to chance discoveries or selective collecting.

Both sites will be omitted from further calculations. For the remaining 21 sites, the following measures have been obtained (figures based on percentages):

<table>
<thead>
<tr>
<th></th>
<th>Terra sigillata</th>
<th>Colour-coated ware</th>
<th>Belgic ware</th>
<th>Smooth ware</th>
<th>Coarse ware</th>
</tr>
</thead>
<tbody>
<tr>
<td>$\bar{X}$</td>
<td>9.86</td>
<td>5</td>
<td>11.24</td>
<td>24.19</td>
<td>49.90</td>
</tr>
<tr>
<td>Md</td>
<td>9</td>
<td>5</td>
<td>12</td>
<td>25</td>
<td>47</td>
</tr>
<tr>
<td>$S$</td>
<td>5.99</td>
<td>3.21</td>
<td>5.43</td>
<td>5.79</td>
<td>7.03</td>
</tr>
<tr>
<td>CV</td>
<td>60.75</td>
<td>64.20</td>
<td>48.31</td>
<td>23.94</td>
<td>14.09</td>
</tr>
</tbody>
</table>

219 On the correlation between rims and the total number of sherds, see Perrin 1981, 68.
220 The one- and two-ribbed handles which are especially typical for amphorae and two-handled flagons were counted as 0.5 rim.
221 The arithmetic mean $\bar{X}$ and the median Md are measures of central tendency. The standard deviation $S$ and the coefficient of variation CV, defined as $CV = \frac{100 \times S}{\bar{X}}$, are measures of dispersion.
The CV expresses group variability as measured by $S$ in terms relative to the central tendency of that group as measured by $X$. It indicates that only two categories, the smooth ware and the coarse ware, have a clear unimodal distribution with a relatively narrow range. In other words: although the sites in the analysis are very different, they all have a pottery assemblage composed of approximately 25% smooth ware and 50% coarse ware. The same cannot be said of the other three categories, which are flatly distributed and which, for the total population of sites, could even turn out to be composed of several different distributions.

The sample distributions of the percentages of smooth and coarse ware indicate that they are by themselves evidently unsuitable as discriminating characteristics of settlements. Thus, they do not have to be examined further, although a few extreme values require attention. As far as coarse ware is concerned, sites 275 and 341 have rather high percentages. These deviations may be caused by the relatively low value of $N$ in both cases. For smooth ware, the remarkably high value for site 194 should also be discussed.

It is possible that we are dealing here with a biased sample, caused by the find-circumstances. The material was brought up by dredging and large pieces of thick-walled pottery (amphorae, dolia, and mortaria), which are included in the smooth ware category, may be more likely to be brought up and discovered. Such pottery is indeed amply represented in the collection from site 194, especially mortaria. These have been counted separately and their relative frequency in relation to smooth ware is indicated in fig. 83.

In the calculation of the measures $S$, $\bar{X}$, and CV, sites 105 and 289, but also site 1, have not been taken into account. Although the general proportion of smooth ware on site 1 is average (fig. 82), it is clear that the percentage of mortaria is extraordinary low. Given the indicated $S$ and $\bar{X}$, the chance that a site where only 3% of the smooth ware consists of mortaria belongs to the same statistical population as those in the sample can be determined by computing the $t$-ratio. Because this presupposes a normal distribution and that condition is not satisfied by the distribution of percentages with data outside the 30--70% range, these have to be transformed. This can be accomplished by the arcsine transformation. The transformed percentages are indicated in fig. 83.

To ensure that a comparison between the percentages of mortaria at site 1 and those at the sites in the sample is really significant, a level of $\alpha = 0.01$ is called for. At this level, if the percentage at site 1 is indeed too small to be accidental ($H_0: \mu \geq 32.25$), the critical value of $t$ with 19 degrees of freedom ($df$) is $t = 2.593$. The $t$-ratio has to be computed by the formula for comparing a single variate to a sample. This gives $t = 3.863$ which considerably exceeds the critical value. The null hypothesis ($H_0$) can thus be rejected and we may conclude that there is a significant difference between site 1 and all the others as far as mortaria are concerned.

This difference is especially interesting because it cannot be attributed to a biased sample from site 1. Granted that one would feel more comfortable if the values of $N$ were larger, it is, nevertheless, very important that the

<table>
<thead>
<tr>
<th>Site</th>
<th>$N$</th>
<th>% of smooth ware</th>
<th>transformed %</th>
</tr>
</thead>
<tbody>
<tr>
<td>1</td>
<td>1</td>
<td>(3)</td>
<td>(9.97)</td>
</tr>
<tr>
<td>38</td>
<td>21</td>
<td>24</td>
<td>29.33</td>
</tr>
<tr>
<td>48</td>
<td>6</td>
<td>19</td>
<td>25.84</td>
</tr>
<tr>
<td>59</td>
<td>16</td>
<td>28</td>
<td>31.94</td>
</tr>
<tr>
<td>75</td>
<td>7</td>
<td>44</td>
<td>41.55</td>
</tr>
<tr>
<td>93</td>
<td>6</td>
<td>32</td>
<td>34.45</td>
</tr>
<tr>
<td>105</td>
<td>17</td>
<td>(33)</td>
<td>(35.06)</td>
</tr>
<tr>
<td>109</td>
<td>8</td>
<td>29</td>
<td>32.58</td>
</tr>
<tr>
<td>117/8</td>
<td>25</td>
<td>36</td>
<td>36.87</td>
</tr>
<tr>
<td>126</td>
<td>20</td>
<td>23</td>
<td>28.66</td>
</tr>
<tr>
<td>194</td>
<td>38</td>
<td>44</td>
<td>41.55</td>
</tr>
<tr>
<td>214</td>
<td>57</td>
<td>23</td>
<td>28.66</td>
</tr>
<tr>
<td>232</td>
<td>33</td>
<td>17</td>
<td>24.35</td>
</tr>
<tr>
<td>234</td>
<td>8</td>
<td>36</td>
<td>36.87</td>
</tr>
<tr>
<td>250</td>
<td>17</td>
<td>45</td>
<td>42.13</td>
</tr>
<tr>
<td>275</td>
<td>10</td>
<td>38</td>
<td>38.06</td>
</tr>
<tr>
<td>289</td>
<td>1</td>
<td>(9)</td>
<td>(17.40)</td>
</tr>
<tr>
<td>298</td>
<td>9</td>
<td>27</td>
<td>31.31</td>
</tr>
<tr>
<td>315/6</td>
<td>25</td>
<td>30</td>
<td>33.21</td>
</tr>
<tr>
<td>341</td>
<td>11</td>
<td>46</td>
<td>42.71</td>
</tr>
<tr>
<td>407</td>
<td>101</td>
<td>20</td>
<td>26.57</td>
</tr>
<tr>
<td>412</td>
<td>137</td>
<td>29</td>
<td>32.58</td>
</tr>
<tr>
<td>416</td>
<td>321</td>
<td>22</td>
<td>27.97</td>
</tr>
</tbody>
</table>

$\bar{X} = 32.25; S = 7.39; CV = 21.71$.

222 For a description see Sokal/Rohlf 1969, 386–7.
223 See Thomas 1976, 240–1. The formula is given as $t = \frac{(\bar{X} - X_i)\sqrt{n/(n+1)}}{S}$, where $X_i$ is the value of the single variate, $S$ is the standard deviation of the sample and $df = (n-1)$. 129 (289)
pottery at site 1 was collected during an almost complete excavation of an entire settlement. Acceptance of the reliability of the sample leads to an interesting archaeological explanation because, of all kinds of pottery, mortaria are probably the most intimately connected with a Roman (ized) kitchen. The fact that site 1 is not just a native settlement but also the only one north of the Rhine can hardly be a coincidence because native settlements south of the Rhine all have more or less average percentages of mortaria. Both statistically and archaeologically site 1 belongs to a different group of settlements and, although more data are needed to draw definite conclusions, this may well point to an aspect of acculturation that was strongly negatively influenced by the presence of the imperial frontier. After all, site 1 is only 10 km beyond the Rhine.224

To return to the problem presented by the relative frequency of smooth ware at site 194: it is clear from the data in fig. 83 that the percentage of mortaria is somewhat exceptional. The question whether or not there is a significant difference from the other sites can again be evaluated by computing the t-ratio. In this case, $H_0 = \mu \leq 32.25$. At a level of $\alpha = 0.01$ the critical value for $t$ with $19\, df$ is $t = 2.593$ and the computed value is $t = -1.228$ which is less than the critical value. Consequently, the number of mortaria at site 194 is not significantly different and there is no reason to assume that we are dealing with a biased sample caused by a disproportional amount of collected mortaria.

An alternative explanation for the high percentage of smooth ware may be provided by the data on site 105. That is a cult site, and site 194 is known to be a military settlement on the basis of the qualitative interpretation of the finds. Nevertheless, the dredged-up finds could be a mixture of material from a fort and a temple in the adjoining vicus. This interpretation implies, however, a whole series of assumptions which may not be valid and cannot be tested. It is also dependent on the representativeness of the sample from site 105. The N (= 88) is too low to be completely confident in this respect and there is no guarantee that the observed frequencies are generally typical for cult sites. On the other hand, the data were collected through excavation of the temples and there is no reason to assume any directional bias. Moreover, the discovery of a rural cult site in Born-Buchten (Province of Limburg) in 1976 also yielded an enormous amount of smooth ware.225 Perhaps it is also no coincidence that one other site in fig. 82, namely, the part of the canabae legionis west of the castra in Nijmegen (407), has 33% of smooth ware. As already mentioned in paragraph 8.4.2, the excavations at site 407 have yielded comparatively little evidence for trade and industry. They did produce evidence of burial monuments and of stone buildings which may have had some public function, perhaps of a religious nature, as could be suggested by a fragment of a gigantic mortarium (diam. c. 1 m) and their proximity to the cemetery (409).

The evidence from site 407 may also point to a different explanation for the high number of smooth ware vessels from site 194, namely, that it is caused by material from an eroded cemetery. Although this cannot be backed by quantitative data here,226 flagons, jugs, mortaria, and the like are normally present in very large numbers in cemeteries.

Unfortunately, any explanation for the percentage of smooth ware at site 194 will remain a mere guess because the original context of the finds is lost. All that is certain is that some extraordinary (post-)depositional process must have led to the observed relative frequencies and that in this case one should be grateful for the presence of some qualitative data which allow at least a partial insight in the nature of the site.

In contrast to the sample distributions of the percentages of smooth and coarse ware, those of the other three groups of pottery in fig. 82 have a wide variability, expressed by the high value of the coefficient of variation. This suggests that terra sigillata, colour-coated ware, and Belgic ware might be useful as discriminating characteristics of settlements.

In that case, there should be an association between the type of settlement and its percentages of these three classes of pottery. To test the presence of such an association, the settlements have to be ordered according to

224 Mortaria are not completely absent in some settlements further north, for example in Dalfsen, Province of Overijssel (Van Es/Verlinde 1977, 46, i. i. 5b), and in Wijster, Province of Drenthe (Van Es 1967, 174). See also chapter 11, p. 431.
225 See Bloemers 1977, 19–22 and Willems 1983b, 251–4; the excavation has not yet been published. Cursory inspection
some independent criterion. Although, as already noted, factors such as distance from production centres and chronology are constant for the sites discussed here, it is difficult to establish such a criterion because the reasons for different amounts of certain pottery products may still be varied. This could reflect the (in)ability to acquire some kind of pottery for social reasons (degree and direction of acculturation), plain preferences, or perhaps even specific rules (for example, 'the army gets first choice'). Therefore, the criterion for ordering settlements should be as general as possible and it has been defined as the 'accessibility rate to Roman pottery'. There is no completely objective way to operationalize this criterion, but there are a number of reasonable assumptions. These are based on the degree to which the inhabitants can be expected to participate in a cash-based market economy and the degree to which they are likely to be acculturated to Roman norms and values. For the eastern river area, the ordering is thus also based on the degree of affiliation with the Roman army which, in theory, could be cross-cut only by the one large town (from which no sample is available). For social and economic reasons, the purely military sites should thus rank first (412, 126) and the two vicus of the canabae legionis second (407, 416). Third are two sites (117/8, 194) of which the finds are probably partly of a fort and partly of the surrounding occupation. Site 38, which must be closely related to a fort, and 315/6, which has been interpreted as a vicus and a secondary centre, should rank higher than rural settlements and have therefore been lumped into a fourth category. These rural settlements could be considered a fifth group, but they can be divided into those that are definitely villae in the sense as discussed earlier (59, 214, 234, 250, 275) and those that are not, or not definitely (1, 48, 75, 93, 109, 232, 298, 341). The inhabitants of the villae, insignificant though the buildings may be compared to the rich establishments further south, are nevertheless more likely to have access to Roman pottery: they are more likely to want it and to be able to afford it. The result is thus a hypothetical ordering in six groups which should at least reflect in descending order the range of options in the acquisition of pottery which are open (for whatever reason) to the inhabitants. The percentages of terra sigillata, colour-coated ware, and Belgic ware can also be ordered from high to low. Because we are dealing with an ordinal scale of measurement, a non-parametric measure of statistical correlation has to be employed, for which Spearman's $r_s$ is adequate. It measures the degree to which rank-orderings along two variables match.227

According to the most adequate formula, which takes into consideration the large number of ties, the association between $X$ and $Y_1$ is $r_s = .72$. The association between $X$ and $Y_2$ is $r_s = .08$ and between $X$ and $Y_3$ it is $r_s = -.63$. These values of $r_s$ indicate a considerable positive correlation between the site orderings and those of terra sigillata, very little when colour-coated ware is concerned and a rather large degree of negative correspondence with Belgic ware. It is possible to test $r_s$ for statistical significance, which requires establishing a level of significance ($\alpha$). In order to be of any real use, the percentage of a certain kind of pottery should be rather strongly related to (characteristic for) a certain type of settlement. Therefore, a significance at the level of $\alpha = .01$ is preferred over the more commonly employed level of $\alpha = .05$. The significance of $r_s$ can be assessed by establishing the $t$ statistic, using the normal approximation to Spearman's rank-order coefficient:228

$$t = \frac{r_s \sqrt{n-2}}{\sqrt{1-r_s^2}} \text{ with } df = (n-2)$$

The values for the $r_s$ of $Y_1$, $Y_2$, and $Y_3$ are $t = 4.52$, $t = 0.35$, and $t = 3.54$ respectively. For a one-tailed test with $\alpha = .01$ and $df = 19$, the critical value is $t = 2.539$, which implies that there is a significant positive correlation between $X$ and $Y_1$ and a significant negative correlation between $X$ and $Y_3$.229

A test for statistical significance as employed here pre-

227 The normal formula for $r_s$ is $r_s = 1 - \frac{6 \Sigma d^2}{n(n^2 - 1)}$ in which $d$ is the difference between rank numbers for each variable. The measure varies between 1 (perfect match) and 0 (no match at all). Sometimes ranks are the same for several units (e.g. 126 and 412 are both 1; 59 to 275 are all 5). When there are many such ties, another formula has to be used:

$$r_s = \frac{\left[\frac{n^3-n}{12} - \left(\frac{\Sigma t_i^3-t_s}{12}\right)\right] + \left[\frac{n^3-n}{12} - \left(\frac{\Sigma t_j^3-t_s}{12}\right)\right]}{2 \sqrt{\left[\frac{n^3-n}{12} - \left(\frac{\Sigma t_i^3-t_s}{12}\right)\right]\left[\frac{n^3-n}{12} - \left(\frac{\Sigma t_j^3-t_s}{12}\right)\right] - \Sigma d^2}}$$

$t_s$ is the number of units that, when ordered along variable $X$, scores the same; $t_i$ indicates how many units are tied.

228 See Thomas 1976, 401-5.

229 Although irrelevant for the present discussion, the two values of $t$ are even significant at the level of $\alpha = .001$. 

WILLEM J.H. WILLEMS / Romans and Batavians, Ch. 8 The Sites: Typology
supposes random sampling. Although for our data randomness can only be assumed, it is important to note that the test as such is not inappropriate because it is directional. If, for example, the surface collections as such were biased, they could only contain too much terra sigillata – which is always picked up and easy to spot – or too little Belgic ware, which could conceivably be overlooked. A negative bias for terra sigillata or a positive bias for Belgic ware, which cannot even be easily identified as such in the field, are completely impossible. This implies that, should some of the samples in fig. 82 indeed not be randomly collected, unbiased samples would produce lower values for terra sigillata and higher values for Belgic ware in the lower rows of fig. 84 and thus even more significant correlations!

Conclusions
It is clear that quantitative measures are a potentially very valuable tool in the evaluation of sites and will certainly lead to unexpected new insights as more and larger samples become available. It is possible to test most results for statistical significance, which is an advantage over the qualitative measures usually employed. Although the present number and size of the samples still leave much to be desired, they have provided useful new information.

In particular, there is a statistically very significant correlation between the ordering of sites on fig. 84 and the relative amount of terra sigillata. This implies that the more military a site is, the more terra sigillata is found on it, and provides a criterion to interpret the nature of sites
in the eastern river area. The town may be an exception to this, because 'degree of affiliation to the army' was not an independent criterion for ordering the sites but the result of ranking along lines of acculturation and economics. The town is, however, so obviously different from other sites that a faulty interpretation is impossible. Although not discussed in detail here, the data in fig. 84 have also been subjected to a discriminant analysis to investigate if the ordering of sites relevant to all three variables (Y1, Y2, and Y3) simultaneously would provide additional information. For the analysis the sites were divided into four groups (rows 1-4, 5-8, 9-13, and 14-21 in fig. 84). The results showed, first, that the total variance of all variables was almost completely determined by the first function of which terra sigillata proved to be the most influential component. It also showed that the first group (1-4) is clearly distinct from the others, among which there are varying degrees of overlap. The fourth group (14-21) can also be discriminated, but the second and third group are not very uniform. These results agree with the above conclusion. The two definitely military sites and the two vici of the canabae legiones are indeed clearly discriminated from the others by their pottery assemblage and, to a lesser degree, that is also true of the native settlements in the fourth group. The overlap between the last three groups is to some extent caused by the fact that not the total pottery assemblage of all sites could be used, but only the Roman pottery. Therefore, the percentage of terra sigillata was calculated relevant to the total amount of Roman pottery and not to all the pottery from a site. For some sites this does not make any difference. As is evident from fig. 82, the percentage of terra sigillata at the four highest ranking sites (412, 126, 407, and 416) will not at all be influenced if native pottery were included. The same is true for sites 194, 59, and 298, while the influence on the figures for sites 38, 48, 214, and 341 will only be slight. The terra sigillata proportion of the total amount of pottery used at the other settlements during the Middle-Roman Period will be lower than the percentages indicated in fig. 82 to varying degrees. It is clear that consideration of native pottery does in fact increase the significance of the correlation between quantities of terra sigillata and the ordering of settlement types. Of the first seven sites on fig. 84, which are interpreted as military, closely related to the army, or a mixture of both, only site 38 has a relatively low percentage of terra sigillata. That may imply - as was also indicated by the discriminant analysis - that it ranks too high in the ordering of sites. The motivation for this relatively high rank is its presumed close affiliation to a fort in Kesteren: it may have been a vicus. However, as already mentioned earlier (p. 269), the excavated features increasingly support this interpretation and so do the finds (cf. note 216).

For the other six sites at the top, the percentage of terra sigillata exceeds 10% and, as far as they are purely military, it reaches 20%. All except one of the remaining sites have percentages below and mostly far below 10%, the highest non-reducible (by including native pottery) percentage being 9% for site 298. The only exception is site 48 with a percentage of terra sigillata that is reducible, but only to 12%. This may imply that site 48 is not the simple native settlement it has been assumed to be, unless the sample is biased. Other finds from this site, especially a fragment of terra sigillata with part of a graffito and a military tile-stamp EXGERINF, which are not by themselves significant, might point to some military function of the site, perhaps a watchtower or signal station. Unfortunately, no structural details are known, although it is certain that the settlement never had any buildings in stone.

Unlike terra sigillata, the ordering of settlements is not significantly correlated with the incidence of colour-coated ware. The values for the different settlements oscillate around a mean of 5% but the variability, as expressed by the CV of 64.20, is very high. As this varia-

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230 The relative frequencies of pottery from the probable fort in Rijswijk-G. (Van Es 1984, 279) illustrate this point. Recalculated in the groups as employed here, the percentages are: terra sigillata 22.7%, colour-coated ware 12.7% (?), Belgic ware 5.2%, smooth ware 24.4%, coarse ware 35.1% (?). Only the ratio of Roman to native ware, which is 64:4, could be curious if it were all from the same site and/or contains no late-Iron Age pottery (both are uncertain). The percentages are based on all sherds, not on rims as they are here.

231 The computer analysis (SPSS package, see Nie a.o. 1975) was provided by A. Voorrips (IPP, Amsterdam). I am very grateful for his assistance not only in this respect, but especially for his critical comments on various drafts of paragraph 8.4.3, which have greatly improved it.

232 See, however, note 216 above.

233 Only the percentage of site 117/8 will be reduced by native pottery but this reduction is limited because most of the hand-made wares are pre-Roman (cf. p. 252).
chapter 5 is much more than for any other period. These 78 sites include 12 cases for which the interpretation as burial site cannot be conclusively demonstrated (nos. 10, 33, 82, 84, 110, 155, 208, 241, 346, 456, 496, and 503) and 1 special case (no. 378) which is not definitely a Roman finds spot. Most of the evidence from all these sites has been collected through chance discoveries, looting, or incidental and very restricted rescue operations. The number of properly excavated sites is very small: there is only one completely excavated cemetery (site 396) and there are five cases where at least part of a cemetery could be investigated properly (sites 39, 305, 398, 409, and 491). Of the excavations, those on sites 305 and 491 have been published in final form. For the other four, the references in the catalogue are preliminary or partial reports. Although many of the other burial sites are also published in one form or another, often just descriptions of the objects or of the circumstances leading to their discovery, there are only a few detailed reports (for example, on site 474) which at least contribute to our knowledge of individual burials. This state of the evidence clearly does not permit a sophisticated differentiation of all known burial sites, but at the regional level of analysis the available data give a reasonable insight into the variability of burial sites and practices, as well as their relation to other sites. The discussion of burials from the Early-Roman Period (paragraph 8.3.3) showed that there was a change in native burial practices which can only have been induced by the Roman conquest, as well as the introduction in the area of Gallo-Roman cemeteries, at that time still directly related to the centre of the Roman military and administrative apparatus. The native ways of disposal-of-the-dead changed in a way that increased their archaeological visibility. Of course, this development continued and can easily be monitored during the Middle-Roman Period. As indicated by the evidence presented in chapters 4 and 5, the number of graves datable to before AD 70 is still limited, but it increases rapidly during the Flavian period, even if the Nijmegen cemeteries are not taken into account. During the 2nd and 3rd centuries, it reaches a peak.

The interpretation of this chronologically straightforward development is, however, rather complicated. It is, for example, very difficult to determine to what extent native burial practices were changed. It is altogether feasible that part of the native population continued to be
buried in the archaeologically almost untraceable ‘late-Iron Age’ way. For all we know, this might even be a very large part, although the (rare) firm associations of small native settlements with cemeteries indicate that it was probably not. A good example are the clearly associated sites 93, a post-hole swarm type of native settlement, and 530, an accidentally discovered cemetery of unknown size with cremation burials. An additional difficulty in the evaluation of the native component in burials is the fact that aspects of the native burial program such as cremation of the body, deposition in a grave pit, surrounding the grave by a ditch, erecting a barrow over it, and the like occurred over a very wide area. They are native but they are also encountered in Gaul or, for that matter, in Italy. Although future analysis may provide useful insights into this problem, it seems that for the moment the introduction of grave goods in native burials remains the only really tangible form of change. As soon as this change had spread more widely, which must have been the case towards the end of the 1st century, what is native and what is not are difficult to separate archaeologically.

In itself, this observation may be taken as an example of a process of acculturation. But such a conclusion is as obvious as it is valid and requires the detailed research at the level of the cemetery and individual interment for which sufficient published data are lacking. At the regional level, however, it is possible to recognize some cemeteries which are native.

Firm associations with native settlements constitute one of the clues. In addition to the example of sites 93 and 530 mentioned above, there are a few additional cases: 130–131, 152–153, 395–396, 458–459, 473–474, and 493–494. For various reasons, several other associations are less reliable. The most interesting case is that of the completely excavated site 396 (see fig. 69) which shows at least one typical form of a native cemetery. Unfortunately, the associated settlement is not very well known. The same is true for the similar cemetery at Schaijk (site 491), where several findspots indicate a native settlement in the immediate vicinity. Another similar cemetery and a native settlement with recognizable house plans have, however, been fairly completely excavated in Oss-Ussen. Finally, a fourth example from the central river area, in Zoelen (see fig. 80), should be mentioned here as well, although in this case there are several possibilities for an association with a nearby settlement (all native).

Although they are very much alike, there are also differences between these cemeteries. The published plans of sites 396 and 491 show that the number of graves with, compared to those without, round or square ditches varies considerably. There is also evidence indicating that ditches may be completely absent in some cemeteries. Site 530 could be an example, but the rescue operation following its discovery did not lead to a complete excavation. A small cemetery from Wijk bij Duurstede provides a better example, but unfortunately it cannot be said to be definitely associated with a native settlement.

This last conclusion also indicates the limitations of identifying native cemeteries by association with native settlements. A much better insight would be gained if it were possible to identify native cemeteries by, for example, a typical general composition of the finds or the absence or presence of certain grave goods. Even looted or destroyed sites might then yield more information. The lack of published material does not allow many remarks on this subject as yet, but the ongoing research on excavated sites does suggest that at least some systematic differences of this kind are indeed present. The only possible clue that should be mentioned here is the presence or absence of native pottery in graves. A provisional list of cemeteries with native pots includes 22 of the 77 sites: 163, 208, 292, 306, 324, 331, 336, 348, 353, 368, 373, 389, 396, 398, 409, 413, 418, 428, 454, 491, 501, 530, and 543. Evidently, there are a number of cemeteries in this list, most notably nos. 398–418 in Nijmegen, which are definitely not native. The presence of native pots as such thus obviously implies nothing for a cemetery, although it could be relevant for interpretations at the level of individual burials, especially during the Middle-Roman
Period. The alternative approach of considering the relative quantity instead of presence-absence of native pots does not appear to be very promising either. There are no sites with more than a few such vessels and these are, if at all, normally datable to the 1st century.  

In paragraph 8.3.3, the continuity of some prehistoric urnfields into the Early-Roman Period has been considered. For the Middle-Roman Period, at least sites 306, 319, 324, 331, 346, 393, 454, and 543  are also spatially related to prehistoric burials. This might identify them as native, although it should not be forgotten that the people buried there were not at all necessarily inhabitants of native villages. Even immigrants into the region are likely to have shared the common urnfield heritage and thus not to have necessarily been averse from burial at such a site. If there should be a more than accidental relation between sites 411 and 409 as mentioned in paragraph 8.2.2 (p. 231), continuous or renewed use of the same area to bury people does not identify the Roman Period cemetery as native because, even though quite a few Batavians may have been buried at site 409, it is certainly not a native cemetery. It is, however, just as likely that a relation between sites 411 and 409 does not exist. The same may be true for site 419, with several prehistoric barrows (and site 543) in the vicinity. Furthermore, the development by the army of the entire area in Nijmegen between the Valkhof (403) and the Kops Plateau (417) does not seem to have been conducted with any respect for what was present there. This is exemplified by an urnfield and several older Bronze Age and Neolithic barrows on site 412, which were ruthlessly levelled because they were in the way.  

In addition to the continuous or renewed use of urnfields, there is yet another 'prehistoric' way of disposal-of-the-dead practised during the Roman Period, namely burial under a barrow. These tumulus graves are also native, but they are different from the native burials discussed above. Instead of belonging to the least acculturated group of the native population, they are usually considered to be the graves of wealthy and highly acculturated villa owners. The only possible instance of such a tumulus in the eastern river area – it has never been investigated and thus might be prehistoric – is site 378, close to the wealthy villa of Mook (site 377). Several kilometers to the southwest, in Esch, and only a few kilometres east of our area, in Moyland, other tumuli were excavated (see fig. 80).  

At least for the eastern river area the tumulus graves, if they occur at all, are probably not native in the sense of being directly connected to traditional burial practices. They are more comparable to other wealthy burials to be discussed below, and clearly the northernmost examples of a practice which was concentrated in southern Belgium, especially west of the Meuse.  

Essentially, the difficulties with the concept of 'native' in the discussion of tumuli, but also of the other burials discussed so far, reflect the ongoing process of amalgamation of burial practices and thus of the religious and social concepts behind those practices. As far as the religious and ethnic background is concerned, we may never be able to unravel completely exactly what the origins of observed phenomena are. On the other hand, the process of amalgamation undoubtedly resulted fairly quickly in a new set of internally consistent standards. Death and burial are too important social events in any society to be performed as well as their significance for the community.  

It is relevant to note that amalgamation is a very precise term to describe what happened. In paragraph 8.3.3 the Roman influence on native burial practices has already which the reported finds have been included in the catalogue under site 383.

244 Esch: Van den Hurk 1973, 1975, 1977, and 1980; Moyland: Isings 1959. The interpretation of the latter group is, however, quite uncertain. The relative poverty of the graves and the large number of them suggest that the low barrows belonged more probably to an exceptionally well-preserved cemetery comparable to sites 396 or 491. Nevertheless, at least the one burial described by Isings could indeed be a tumulus grave.

245 See e.g. Van Doorselaar 1967, general map.

246 See e.g. Willems 1978.
been mentioned because, for the Early-Roman Period, it is the most striking phenomenon. But it is also clear that the burial program of the society in the river area during the 1st–3rd centuries was not the result of a one-sided process. Indigenous norms and values contributed to it, as well as those of other non-Roman groups. The end-product can, therefore, not be considered the result of 'romanization', even though the entire development was caused by the Roman conquest and Roman values undoubtedly set some important standards.

The latter is, of course, the reason why, at the level of cemeteries, similar developments can be seen in widely different places in the Roman empire. There are adequate recent overviews of these developments, with particular reference to the province of Germania Inferior. 247 Although the variation in burial types is large, the cemeteries as such are difficult to analyse typologically. Apart from the 'native' cemeteries discussed above, all burial sites contain larger or smaller numbers of pits with – as a rule – the cremated remains of the deceased. The size of a cemetery obviously depends on the size of the community burying its dead there. In itself, the differentiation in large and small on Appendix 3 is not particularly relevant. It is only an impressionistic approach to size and thus not even entirely reliable. There are, however, at least a few instances where size is, or could be, relevant.

The most obvious cases are the burial sites at the lower extreme of the size scale, namely the single graves. There are quite a few of those in the river area: sites 35, 44, 82?, 153, 157?, 163, 174, 208?, 263, 327, 332, 344, 354, 372, 378?, 382, 387, 393, 431, 434, 455, 456?, 458, 462, 474, 505, 517, and 529. Some of these sites may have been part of a cemetery, together with other known burials in the vicinity (e.g., site 382) and there surely are others which were in fact part of an undiscovered or destroyed cemetery. The remaining cases of true isolated burials are thus fewer in number, but there is no reason to doubt their existence. They are definitely a separate type of burial site because they are not cemeteries: theoretically they could be considered a category of 'isolated find' (cf. fig. 5), which was defined in chapter 1 as a findspot representing a single activity. Such an interpretation, faulty as it may seem to be, could be appropriate, but it cannot be considered here because of the lack of a regional burial analysis and knowledge of the total burial program of the society involved. That program may well have included a whole series of circumstances under which the deceased should be buried in isolation, and conceivably in a specific manner. 248 Most societies do indeed have such rules and, if they pertain to the isolated burials discussed here, emphasis on the isolated find aspect would not be very fruitful.

In the absence of further analysis one might, however, also envisage simple coincidences leading to isolated burials. These would then be just another category of findspot reflecting human use of the landscape for which interpretation as isolated find is not inappropriate. Although admittedly of little practical use, the discussion of single graves shows that a typology of burial sites can proceed on formal characteristics but its relevance will need to be substantiated further. Apart from the isolated burials, there are as yet no definite reasons to use the size of cemeteries for typological differentiation, although it could be relevant in combination with other factors. For example, the huge number of barrows in Moyland (see note 244) is one of the reasons not to interpret them as tumulus graves in the sense of the Belgian group. Conversely, rich burials in tumuli, which are probably correctly interpreted as interments of wealthy villa owners, occur only in small groups such as in Esch (or isolated!). Typologically, such family (?) cemeteries are comparable to other rich rural burials or cemeteries, such as interments in sarcophagi or square walled enclosures.

The latter have not been found in the eastern river area 249 but they do occur, possibly as a separate segment, as part of the large main cemetery (site 398) of Ulpia Noviomagus. 250 These excessively wealthy burials are presumably themselves a characteristic of a large urban cemetery. Such cemeteries are always located outside towns and alongside roads, where monumental burial fa-

248 Examples for such a situation are provided by the four isolated burials from Rijswijk (Bloemers 1978a, 216–9) which are also unusual as inhumations instead of cremations. Moreover, two are extended, one is partially and one fully flexed.
249 For an example, see Von Petrikovits 1956, fig. 1 and 6. A possible example in the river area might be site 440. Although no structural details are known, some finds from this cemetery are very exceptional.
250 See Bogaers/Haalebos 1984. It is possible that the spatially segregated group of 6 large, ditched enclosures in the native cemetery of Oss-Ussen (see Bloemers/Louwe Kooijmans/Sarfati 1981, 113) is in some ways comparable.
offices were designed to have an impressive effect. Such roadside burials often led to elongated cemeteries stretching out over several kilometers. This may also be true in Nijmegen, where a 3rd-century burial in a sarcophagus (site 397) could in fact mark the southernmost extension of site 398.\(^{251}\) Remains of burial monuments are also known from Nijmegen,\(^{252}\) but actual foundations, apart from the recently discovered wealthy burials at site 398 mentioned above, have only been found at site 407, the western ward (\textit{vicus}) of the \textit{canabae legionis}. Undoubtedly intended to reach a maximum visual effect, two of such monuments were built at the very brink of the steep slope of the ice-pushed ridge.\(^{253}\) The monuments at site 419, of which only possible foundations are known,\(^{254}\) may have stood in a similar position and alongside the road from the fortress (site 412) to the \textit{limes} in the east, if that should indeed be its direction (see chapter 3.4 and below, p. 301).

Discussion of the Nijmegen cemeteries automatically leads to another possible distinction between burial sites, namely, civilian and military cemeteries. It is relatively easy to differentiate the Nijmegen sites in this way, because nos. 409, 413, 414, 415, 418, and 419 (+ 543?) are clearly chronologically and spatially associated with the military installations and related sites, while no. 398 (+ 397?) belongs to the town, no. 400 being an intermediate case. It is less easy to determine whether this differentiation has any relevance as far as cemeteries are concerned. They were used for a special group of the population and thus, for example, more likely to include unusual burials or an exceptionally large proportion of male interments. In practice, however, they may not have been very different.

First, there are no indications that normal soldiers’ graves are different from those of civilians, but this is again a matter that can be evaluated only after burial analysis. For example, the (compulsory) membership of burial clubs may or may not have led to special burials or a characteristic layout of the cemetery. Second, there is no reason to assume that ‘military’ implies that only soldiers were buried at a cemetery. Not just the deceased members of senior officers’ families, who actually lived in the military establishments, but also the wives or concubines and children of other soldiers, as well as other inhabitants of the \textit{canabae legionis}, could be buried at military cemeteries.

On the other hand, although the significance of a distinction between military and civilian cemeteries cannot be fully appreciated, it is certainly not a priori irrelevant at a regional or any other level of analysis. At the very least it is worth while to know where soldiers were buried because of the implications for the interpretation of the regional pattern of sites. The absence of special characteristics for military cemeteries, whether it is real or the result of the low level of information, does not cause great difficulties for classification. The interpretation of the Nijmegen cemeteries on the basis of their location is an example, but other clues are not lacking. The inscriptions on tombstones, such as the one erected (at site 419) for \textit{L}ucius Clemens, soldier of the 10th legion (\textit{CIL XIII}, 8734), are the most obvious source of information. They are, however, only very seldom found at cemeteries and more often in a secondary position. Typical military grave goods, which would have been very useful, are also rare. They are not lacking, for example, some specific types of fibulae or an occasional phalera, but weapons in military graves are almost completely absent,\(^{255}\) probably because they were returned to if not reclaimed by the army.\(^{256}\) The only objects of unmistakably military origin which occur more frequently are those which can be identified as such by a graffito. In view of the arguments provided in paragraph 6.5.2, a high percentage of objects, usually pottery, with owner’s marks is very likely to indicate a military population. But it is not always necessary to argue by comparison in this case, because there are often some graffiti with additional information such as the function of the owner (\textit{eques}, \textit{vexillarius}, \textit{signifer}) or his unit (\textit{turna} of ..., \textit{centurio} of ...).

\(^{251}\) For another interpretation, see Van Buchem 1955, 33.

\(^{252}\) Brunsting 1948, 78: \textit{spolia} from the Valkhof.

\(^{253}\) See Bloemers 1979d, \textit{Abb.} 50, 10 and 11, and \textit{Abb.} 55. See also fig. 12: the monuments were excavated before the construction of the large office-building.

\(^{254}\) Brunsting 1948, 78.

\(^{255}\) Cf. Breeze a.o. 1976, 81. Note, however, that graves with weapons as such are not as exceedingly rare as is often assumed, cf. the compilation by Van Doorselaer 1965. As convincingly argued there, the weapons in this case are not indicative of a military burial but of a high status burial entirely in a civilian context. This conclusion was once again confirmed by the recent discovery of very rich burials at site 398 mentioned above. Grave 8 contained weapons (Bogaers/Haalebos 1984, 3) and together with a previously discovered rich grave at the same site (Brunsting 1937, 184) it shows that the only weapon graves from Nijmegen occur in the civilian cemetery of the town, not in the military cemeteries!
Apart from Nijmegen, there are only two other cemeteries with burials of soldiers. For site 501 (Cuijk) there is little information, but it is evident that the cemetery is not only spatially but also chronologically associated with the fort (499).\(^{257}\) It is thus quite likely to have initially been a military cemetery.

The other site qualifying as such is no. 39 in Kesteren. As mentioned in paragraph 8.4.1, the fort in Kesteren has not been located (site 37 is a hypothetical site) but the indications for its presence are very strong, site 39 being one of them. The evidence for military burials at site 39 is constituted by a number of graffiti, at least one of them certainly military.\(^{258}\) Furthermore, the cemetery is extraordinary because of the presence of 19 horse burials. An occasional horse burial is not uncommon on both sides of the limes,\(^{259}\) but these are usually found in isolation, in or near settlements rather than cemeteries, and may also be offerings if not simply the disposal of the carcases of diseased animals (kereng) in some cases.

The practice of burying horses in a cemetery seems to be exotic; the only area in Europe where it occurs more often during the Roman period is the Danube basin (eastern Austria and Hungary), in the Noricum-Pannonian area as well as the Sarmatian area on the other side of the limes.\(^{260}\) It is tempting to connect this observation with the fact that in Flavian and Trajan times most of the auxiliary soldiers which were not recruited in the Rhine provinces are known to have been of Danubian origin.\(^{261}\) Although such an interpretation is highly speculative, it is at least probable that the horse burials do indeed indicate military activity: archaeozoological investigations of the bones has shown that the horses were significantly different from the much smaller native horses during the Roman period and even later, and comparable to the horses from the fort at Valkenburg Z.H.\(^{262}\)

Conclusions
Examples of the variability of disposal-of-the-dead practices in the Middle-Roman period have only occasionally been mentioned in the above discussion. They are amply documented in the overviews by Van Es or Bechert (cf. note 247) and, given the present state of the evidence, only incidentally relevant to the general differentiation of cemeteries attempted here. The number of well-excavated and published sites is still too small to draw reliable conclusions, although it is clear that differences at the regional level do exist; but in view of the evidence, it is more appropriate to discuss these in relative terms rather than discrete types. Only size offers a criterion for distinguishing discrete types at the extremes: there is one very large urban cemetery, site 398, with an estimated number of 12,500 graves. At the other end are a number of isolated burials, the significance of which is largely unknown.

The other burial sites vary in size from very small groups of graves to as many as the 4000 interments estimated for site 409, but most of the somewhat larger cemeteries probably contained hundreds instead of thousands of burials (no. 39 (incomplete), 120; no. 396, c. 250; no. 491 (incomplete), 63). As far as these 'middle-range' cemeteries are concerned, there is a difference between those that are more and those that are less native, disregarding for the moment the possibility of some sites where most of the deceased were still buried according to the archaeologically hardly detectable late-Iron Age practice. Site 396 could be an example of the former; it belongs to a native settlement. The small remnant of site 305, perhaps part of one of the cemeteries belonging to the vicus in Wijchen (site 315), does not seem to have native characteristics. These are presumably also absent on the remaining Nijmegen cemeteries, which can be expected to resemble site 409 and other Gallo-Roman cemeteries that normally appear as dense clusters of pits.

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256 Possible exceptions are mentioned by Breeze a.o. 1976 (Camelon) and Gechter/Kunow 1983 (Mehrum). The grave of Mehrum, however, was definitely the burial of a native, probably a chieftain but perhaps a soldier of an irregular unit or employed at an individual basis as a scout. The weapons in question only resemble Roman weapons, and the same is true for the Camelon burial discussed by Breeze a.o. 1976. Weapons of Roman soldiers were normally bought back by their units at the time of discharge or after death (op. cit., 93–5). This also accounts for the multiple owner's names which sometimes appear on the same piece of equipment.

257 The finds could not be studied in detail. Cursory examination of the material in the RMO did, however, reveal that it includes a fair proportion of pre-Flavian pottery (a.o. terra sigillata) and the suggestion (Bogaers/Rüger 1974, 84) that the cemetery belongs only to the 2nd- and 3rd-century vicus of Cuijk (site 500) is not correct.

258 JROB 1974, 23.


261 Alfeld 1968, 103–4. Most of them were, however, Thracians (see also chapter 11, p. 403).

262 Prummel 1983b.
with cremation burials. On the other hand, the provisional report on the military burial site 39 also mentions three graves surrounded by a ring-ditch which suggests native burial practices there as well. The smaller cemeteries of only a few to several dozens of graves will presumably not be very different from the larger ones. The only known exceptions, the small graveyards with interments of wealthy villa owners, have not been found in the eastern river area, although they may well have been present. Sites 378 and 397 are possible instances, just as the vanished grave of M. Traianus Gumattius, of which only the tombstone remains (site 57).

In addition to the above remarks aimed at demonstrating differentiation, the limited success of these attempts already indicates that the clear differences observed for the Early-Roman Period are not so easily established for the following centuries. For a period in which processes of acculturation must have caused profound changes, such a result is hardly surprising.

8.4.5 Isolated Finds

Most of the isolated finds on Appendix 3 are simple stray finds: findspots of sherds, coins, or other Roman artefacts. To some degree, these are useful as an additional illustration of man’s use of the landscape, but quite a few of them have dubious locations: a reconstructed findspot or a location in a possibly or certainly secondary context. The last category includes a few finds which are important for other reasons, such as the (military?) material from sites 18a, 135, and 546, or a few altar-stones with inscriptions (128, 422, 445), and a fragment of a milestone (427).

Some of the above, as well as several other isolated finds, have already been discussed in preceding chapters where they provided valuable information. Examples are the military graffiti and metalware from sites 120 and 125 in relation to the probable fort in Driel and the possible significance of the net-sinkers from site 199 for the dating of the river IJssel as a branch of the Rhine. There are, however, a few isolated finds which have hardly or not at all been mentioned.

Isolated (usually stray) finds are in general useful additional evidence for the reconstruction of land and water routes, if not for the actual roads (cf. paragraph 3.4). The finds from site 242 in Ewijk are a good example of a direct relation, in this case with a secondary road. The fragment of a milestone (site 427) is almost certainly re-
lated to the major road from Noviomagus to Harena-
tium. The milestone (*CIL XIII, 9162*) was erected
under Trajan, probably around AD 100, and perhaps in-
dicates the actual road because it was found close to the
early-medieval *via regia* in Beek, which is known to have
been a Carolingian villa. If, as is often true elsewhere,
this *via regia* was originally a major Roman road, this has
implications for the alternative trajectories of the road to
Harenatium as outlined in paragraph 3.4 and would
provide support for a direct route from Nijmegen over
Beek, Zyfflich, Niel, and Mehr to Rindern.

The Roman barge from Druten (site 211) is, of course,
evidence for the water route along the river Waal. Final-
ly, the finds from site 125 should also be mentioned. In
addition to the military aspects of the metalware dredged
up there, it may well indicate that the Drielsche Veer ex-
isted as a Rhine crossing (ford or ferry) in Roman times.
Somewhat more to the east there are, incidentally, sever-

263 *The villa Bechi*; see Leupen 1977.
264 See Ulbert 1969a, 102 and 118–9, who disagrees with the
265 Brunsting 1937, 175 and Daniëls 1950, 14–5.
266 Daniëls, *op. cit.*, 16.

An interesting and special category of isolated finds are
the hoards. Two of these, sites 22 near Wolfheze and 26
in Arnhem, are indicated on Appendix 3 by a separate
symbol. In the older literature there is sometimes talk of
a third hoard north of the Rhine, in Quadenoord (prob-

bly site 21), but the available information, summarized
by Byvanck (ER III, 164), only confirms that relatively
many coins may have been found there.

There is, however, at least one other true hoard known
from the eastern river area, namely, 50 denarii found in
a pot near the slaughterhouse in Nijmegen (site 399).265
In addition, the composition of the 17th-century coin-
collection of the Smetii has been taken as evidence that
it contained the coins from a hoard hidden around AD
270 and quite probably in Nijmegen; together with what
was probably part of another Nijmegen hoard buried
around the same time, there is thus evidence for two
more hoards without an exact findspot.266

A collection of 121 denarii and antoniniani, allegedly
found in Berg en Dal (around sites 424–433), should also
be mentioned here. Daniels considered this a hoard,267
but in view of the obscurity of the find circumstances,
the composition of the collection (Republic–Postumus),
and the way in which it was acquired, that interpretation
is rather doubtful. Somewhat more reliable is yet
another hoard from approximately the same area, name-
lly the find of three silver saucepans discovered ‘near the
windmill of Beek’ in June 1806.268 The representation of
the goddess Cybele on the handles has led to specula-
tions about the possible religious nature of this hoard. In
any case, the total remaining weight of 478.7 grams of
fine silver (the original weight was probably more than
600 grams) indicates a considerable economic value.

Finally, three stray finds of aurei, which might also be
considered a hoard by themselves because of their value,
have to be included. The first is an aureus of Trajan
found near Elst (site 146); the exact findspot of the sec-
ond, an aureus of Nero, is unknown. It was reported to
have been found near Herpen (c. 172/420), in the vicin-
ity of site 488. A third aureus (also Nero), was recently
found near Escharen.269

Without regard to the aurei and the saucepans, the
hoards can be divided into two major chronological hori-
zons. The two hoards from Nijmegen and the possible
hoard from Berg en Dal are datable to the period around
AD 270. Their interpretation in the light of historically
known events is self-evident. That is not true for the

268 See Daniëls 1955, 291 and Vernaseren 1956. A picture
of the finds also in Van Es 1981, 260, fig. 201. The findspot is
probably in square 191/426 on Appendix 3.
269 Boersma 1963, 55, no. 53d (Herpen) and Verwers 1983,
39–40 (Escharen)
hoards from sites 22, 26, and 399, which belong under the Severian dynasty. The latest coin from the hoard of (Renkum-) Wolfheze was probably minted in AD 197, the one from the Arnhem hoard between AD 222 and 235, and the one from the Nijmegen hoard in AD 208. It is possible that all, but especially the first and the last, are comparable to a number of hoards dating to the same period from the northern part of the Netherlands. These are regarded as the result of unrest and migrations in Germania Libera which, by the end of the 2nd century (after AD 180), also provoked unrest in the northern Netherlands and thus the hoarding of coins. Whether or not our hoards can be attributed to the same phenomenon remains uncertain. Van Es, following Glasbergen, assumes that substantial repairs and rebuilding activities were carried out under Septimius Severus. In itself this may indeed imply the presence of a threat, although others argue for an essentially peaceful period at the limes of Lower Germany that lasted a few decades longer. This would provide a context for the Arnhem hoard but not for the other two, but perhaps the true context was a very different one.

8.5 THE LATE-ROMAN PERIOD
A first glance at Appendix 4 (or fig. 63) is sufficient to confirm general expectations: compared to the previous maps, this one is virtually empty. The nearly 300 settlement sites of the Middle-Roman Period appear to have been reduced to a mere 76 inhabited settlements during the Late-Roman Period and of these not more than 8 probably and 8 only possibly. The situation thus agrees with what can be found in any textbook, namely, decline and depopulation. Whatever else we can say about the Late-Roman Period in the river area, here or in later chapters, does not alter this basic fact. It is necessary to keep this in mind, because there are indeed quite a few other circumstances which need to be considered.

From a comparison of the gross numbers of middle- and late-Roman settlements, the first conclusion is that apparently their total number was reduced to 25%, disregarding the size and nature of the settlements involved, as well as matters of continuity of occupation, geological change, and the like. Compared to other regions and notably to the ‘nearly empty’ western part of the Netherlands, that is an extraordinary high number. For the south, no figures are available. They may be comparable or lower but certainly not higher, although some regions in the loess areas in adjacent Belgium and Germany were also relatively densely settled. Precise data for the northern parts of the Netherlands are also lacking, but even there the evidence for late-Roman settlements is meagre compared with the preceding centuries. Comparing the eastern river area to other regions is not without its risks. First, the chances of detecting sites in

Site 194 has been indicated as an isolated find (see below, p. 310) but there remains a (remote) possibility that it was actually occupied.

272 Van Es 1981, 46 and Glasbergen 1947, 305. Glasbergen’s conclusion was based on inscriptions supposed to have been found in Katwijk (CIL XIII, 8828) and Roomburg (CIL, 8824). On the curious and complicated history of these inscriptions, see Bogaers 1976.
273 See e.g. von Petrikovits, in Bogaers/Rüger 1974, 16, and 1978, 88–9. He does not assume any real hostilities until the reigns of Caracalla and Elagabalus and proposes two inscriptions from Vechten (CIL XIII, 8810 and 8811) as possible corroborating evidence. Although the conclusions in chapter 9 are necessarily provisional, it is worth noting that the substantial rebuilding in stone in Meinerswijk (‘period 5’) has also been dated to the same period. See also chapter 11.4.
274 See chapter 11, note 427.
275 Of the 76 settlement sites, 5 do not appear as such in Appendix 4. Sites 59 (see p. 167), 156, and 538 were definitely inhabited. No. 59 was omitted by an error, 156 and 538 because the information was not yet available when the map was printed. The same is true for the probable settlement site 536.
general and thus also those of the Late-Roman Period are equal to those in some and much better than in most areas elsewhere. Second, there is the matter of the sensitivity of archaeological indicators: the recognition of late-Roman artefacts, and especially pottery fragments, for what they are. This depends on the availability of reliable typochronological series and these are better for wheel-turned than for hand-made wares, especially for the regions south of the Rhine where the study of late-Roman hand-made pottery is still in its infancy.\textsuperscript{280} Unless clearly identifiable northern forms are available, there is thus an almost complete dependency on wheel-turned wares, most or all of them imports. It is evident from the data presented in chapters 5 and 6 that the eastern river area did not lack those imports. In comparison to quantities of middle-Roman wheel-turned wares, those of the Late-Roman Period are small. But compared to other regions, they are very large indeed.

In view of these arguments, the question arises whether the relatively high number of settlements in the eastern river area should be explained by a relatively large population during the Late-Roman Period or by the fact that the inhabited settlements can be better recognized as such because of the nature of the finds, or, perhaps, by both. This is a matter that will be returned to below and in chapter 12, but it is clear that different socio-political and military situations will have led to differences between areas, both in the size of the populations and the flows of goods to those areas.

The available evidence at least suggests that the eastern river area was relatively well supplied. The actual numbers of settlements must even have been higher than the maximum of the 76 mentioned earlier. The reasons for that situation are summarized in chapter 4.2 (p. 92): late-Roman sites are underrepresented because they have in fact only been recognized as such on the basis of hard evidence, the presence of recognizable late-Roman wheel-turned potsherds. In addition to what has been said there and in the discussion of the finds in chapter 6, it is also useful to repeat here that several kinds of late-Roman pottery are simply unrecognizable as such when one is dealing with small fragments. Some of the coarse ware types from the late-3rd and early-4th centuries are very difficult to distinguish from their typological predecessors (see also note 278). Late-Roman terra nigra-like wares may also not be recognized unless typical rim or base fragments (for example, of the cups Chenet 342: fig. 39) are present. Fragments of colour-coated ware, with the exception of red-brown slip ware, are yet another example.\textsuperscript{281}

Without implying anything directly about population densities, the conclusion must be that the minimum number of late-Roman settlements is certainly one quarter of those in the Middle-Roman Period. Given the estimates for middle-Roman settlements (p. 246), this would mean that the total number of late-Roman settlements in the eastern river area can be estimated at a minimum of 90 and a maximum of 125 or slightly higher, but the uncertainties in such an estimate are considerable. It is, for example, not at all impossible that the percentage of surviving sites on Pleistocene deposits is higher than on the Holocene river deposits, where archaeological reconnaissance is much better.

On the other hand, considerations like that are somewhat superfluous because other indications, such as pollen diagrams, confirm that the total number of sites cannot have been all too large. It is worth while, however, to note that the general distribution of sites over the landscape is at least somewhat different from the preceding period, because they are more evenly spread over the Pleistocene and Holocene deposits.

This does not imply that they are more evenly spread over the entire region as well. With the exception of the sites north of the Rhine and along the evidently still existing route to the north from Randwijk/Wageningen towards Ede, there seems to be a clustering within a 15 km radius from Nijmegen. It can hardly be a coincidence that the formerly densely settled areas in the west, notably around Kesteren and Druten, have yielded no evidence for late-Roman habitation despite intensive fieldwork. Almost all late-Roman settlements are either continuously inhabited or reoccupied middle-Roman sites, which makes it even harder to believe that no late-Roman material was found on the western sites unless it indeed never existed. The general situation is in fact the same as that in the central part of the Dutch river area. Although new regional inventories will surely modify this picture, the above conclusion is still in agreement with Modderman’s findings.\textsuperscript{282}

The 4th-century habitation, or – to be more precise – the evidence for 4th-century ming-up in 1955, 34. Inventories of the central part of the river area are being prepared by C. Blommesteijn and W.J. van Tent (see Van Tent 1978a, 212, esp. fig. 10, 8, and 1984)
The concentration of sites in the eastern part of the eastern river area is an interesting phenomenon in itself, but it is not the subject of the present discussion, which should provide some insight in the different forms of settlement.
Although perhaps less clearly than in the preceding periods, it is still possible to separate military and civil settlements. These will be discussed in the following paragraphs.

8.5.1 Military Settlements
The organization of the Roman army was changed during the 3rd century to cope with the changing political and military circumstances. These changes were, of course, reflected in the distribution as well as the structure of military establishments. The most important trends in the development were, first, an increased mobility of the troops (more cavalry) and the creation of

mobile armies normally not stationed at the frontier but in the interior. Second, the creation of heavily fortified, siege resistant, and self-contained strongholds at the frontier occupied by increasingly static forces (*limitanei*, *ripenses*) and of various sorts of other strongholds, as well as the fortification or abandonment of towns in the interior. Important concomitant developments were a change in offensive and defensive armament and the disappearance of differences between military and civil settlements, which had for so long been scrupulously — though not always successfully — kept separated.\(^{284}\) In addition to fairly clear-cut cases it is thus sometimes difficult to decide whether one is dealing with a fortified town with a garrison of soldiers or with a military fortification. Fortunately, such a distinction is not vital because the function of a settlement within the late-Roman system of defence-in-depth should not be measured by 1st- or 2nd-century criteria.

While the field armies of the 4th-century were either moving around or kept in garrisons inside towns in the hinterland, the forces at the frontier became increasingly immobile from the late-3rd century onwards; they were farmers with a vested interest in the land as much as soldiers. Compared to the East, this policy was apparently not as thoroughly effectuated in the western part of the Empire, and in particular not in Germania Secunda.\(^{285}\)

For example under the tetrarchy (Diocletian and successors), all army units were probably still stationed on the frontier. Nevertheless, in the 4th century the same policy was also practised in Germania II and *ripenses* were living with their families in heavily fortified strongholds. Their function was no longer to prevent or to counterattack, but to defend, to hold their positions, and to delay the enemy. This led to the construction of heavily fortified strongholds on the frontier as well as further inland, as hard-points in the defence-in-depth system. Incursions along major roads were prevented or delayed by road-forts, either newly built small forts (*burgi*) or fortified and contracted settlements (see fig. 143).

The development of late-Roman fortification techniques has been amply documented and need not be treated in detail here. In general, it can be said that there were various improvements, such as thicker walls, cleverly constructed gates and protruding towers, and larger and deeper ditches. Wherever possible, new strongholds were located in an easily defensible position.

In the area covered by this study, there are quite a few settlements which were, or are likely to have been, part of the late-Roman defence structure of Germania II (see also chapter 12). Presumably the largest site is that in Nijmegen.

### Site 403 Nijmegen-Valkhof

In the preceding parts of this chapter, the early- and middle-Roman occupation of this site has already been discussed. During the Late-Roman Period it seems to have been the only remaining inhabited site in Nijmegen. Although a few finds indicate that some people were probably still living at site 399, the town — Ulpia Noviomagus — had ceased to exist as such before the end of the 3rd century. The inhabitants who remained in Nijmegen almost certainly moved close to the defensible Valkhof site. As pointed out earlier (p. 233), this site is the only one that combines a high, easily defensible position with a location at the river Waal: all other formerly inhabited sites were either in a more open and low-lying position or very high up the ice-pushed ridge and rather far removed from the river.

Although many questions about the late-Roman history and structure of site 403 still need to be resolved, recent excavations have finally provided some really useful data.\(^{286}\) A simplified plan of site 403 and the related late-Roman cemeteries (sites 405, 410, and perhaps 413) is provided in fig. 86. It only presents the 4th-century features; although some traces of middle-Roman and late 3rd-century ditches have also been discovered, these cannot yet be interpreted.

During the first half of the 4th century, the Valkhof was protected by two systems of ditches. The largest perimeter is indicated by two parallel, more or less V-shaped ditches, each more than 4 m wide and 1.5 m deep, encompassing an area of approximately 500 m long and maximum width of approximately 300 m. These ditches, which cannot have remained open for very long, fell into disuse at the latest shortly after AD 350 as is indicated by a coin series terminating with three *Fel Temp Repara-

\(^{284}\) See MacMullen 1963, esp. chapters 4 and 6; Von Petrikovits 1978, 239–40.

\(^{285}\) See the remarks by Luttwak 1976, 190 and Von Petrikovits 1978, 221.

T10 coins from the upper part of the fill. No traces of a wall were discovered and, although not impossible, it is improbable that a substantial wall ever existed along these ditches.\textsuperscript{287}

The inner ditch encompasses a maximum area of c. 5 ha. It is very large (see fig. 87), in one section 14–15 m wide and at the point of the V-shaped section 5.6 m deep. It contained among other material blocks of limestone from an early-Roman column (fig. 67), presumably used as \textit{spolia} in 4th-century constructions. Unfortunately, no traces were discovered of these constructions, but in view of the very considerable post-Roman disturbance at the site that is hardly surprising. It is thus also not surprising that in the few instances where traces of the wall along the ditch could have been found none were observed, but there can be little doubt that there was indeed a wall. The coin evidence from the large ditch indicates that it, too, fell into disuse shortly after AD 350, although the coin series of the Valkhof site in general (see figs. 141 and 142) reaches to the end of the 4th century.\textsuperscript{288}

There is as yet no clarity about the relation between the two ditch systems. The outer double ditch may represent a first line of defence (for example, to keep away siege engines and artillery), but it could also precede the inner ditch and represent either a first attempt at fortifi-

\textsuperscript{287} The inside of the inner ditch did yield some limestone, tuff, and other building material (\textit{JROB} 1981, 33) which could – but not necessarily – point to some sort of construction.

\textsuperscript{288} See Daniëls 1921, 29–31; Haalebos 1976, 204–5.
cation of the site or a temporary safety measure while the main defences were being built. Both interpretations agree with the fact that the double ditch was open for only a short period of time and was probably not accompanied by a wall, at least not by a well-built and heavy one. In addition, clear indications of habitation between the outer and inner ditches are lacking, although the current state of the investigations does not permit definite conclusions in this respect. A fair amount of late-Roman material was salvaged from the area between the ditches before World War II.  

Whatever the real situation may prove to have been, it is clear that the Valkhof was an important and large fortification at least during I VA. On the basis of the cemeteries, the population has been estimated at a minimum of 600 people. Although its role in the late-Roman defence system need not be doubted, there is additional evidence in the form of tile-stamps of a *numerus Ursariensis* (fig. 138) and a, presumably late-Roman, stamp of a *legio XXII*. The first unit was probably formed in III B and is attested from various forts, including site 460 (Qualburg). The *legio XXII*, sometimes with the title C(constantiniana) V(ictrix), is known to have engaged in building activities at several frontier-fortifications along the Rhine under Constantine and his sons. All the available evidence thus points to the late 3rd and especially the first half of the 4th centuries for the defence works discovered so far. But the occupation did not terminate afterwards, as is also indicated by the evidence from the cemeteries 405 and 410.

**Site 460 Schneppenbaum-Qualburg**

The evidence from Qualburg need not be discussed in detail here. The limited excavations in 1937 have provided clear evidence of a late-Roman fortification, presumably to be identified as Quadrriburgium. The main feature was a ditch c. 16 m wide, probably similar to the Valkhof ditch, although it could not be completely excavated. The ditch was no longer open after c. AD 300. No wall was excavated, but it evidently belonged to a late 3rd-century fortification, probably occupied by the *numerus Ursariensis*.

The occupation continued throughout the 4th century (and later) which, although no features have been excavated so far, agrees with the identification as Quadrriburgium, one of the series of frontier fortifications, according to Ammianus Marcellinus reconstructed by Julian in Germania II and the northern part of Germania I.

**Site 499 Cuijk**

The fourth and last of the major late-Roman fortifications in the eastern river area is Cuijk, undoubtedly the Ceclum of the *Tabula Peutingeriana*. From north to south, it measures 110 m (inside the walls), from east to west the distance is probably similar but it cannot be established because the eastern part of the site was eroded by the Meuse. The fort, evidently a stronghold intended to secure the Meuse-crossing of the road from Tongeren to Nijmegen, was built under Constantine and reconstructed or repaired later, presumably under Valentinian I. The first phase had an earth-and-timber wall 4–5 m wide, replaced in the final phase by a stone wall only 1.5–1.9 m thick with projecting half-round towers and buildings on the inside against it. A deep and wide ditch is absent in Cuijk. Instead, the fort was surrounded by two par-
parallel V-shaped ditches only c. 2 m wide at a distance of 5–7 m and 14–15 m from the wall. The end of the occupation of the fort is unknown, but it may have been shortly after AD 400. Habitation of the site as such, however, continued (uninterruptedly?) into the Early-Middle Ages.294

Of the major late-Roman military sites discussed so far, two (126, 460) were frontier fortifications. A third (403) may have had a similar position at one time and have functioned as a road-fort at another. The fourth (499) was primarily a road-fort. The remaining sites are either rather uncertain as late-Roman military strongholds (123, 135, 194) or of much smaller size (391–466).

**Site 466. Asperden**
The *burgus* in Asperden may be interpreted as a watchtower. The published plan295 indicates a wooden construction and later rebuilding in stone of a central tower (15.6 x 15.6 m) with massive walls, surrounded by a c. 1 m thick wall of 40 x 40 m with round, half-projecting towers and two small V-shaped ditches 2.5 m wide and nearly 2 m deep. The structure which, as a road fortlet, controlled a route along the river Niers, was constructed under Valentinian I and existed until shortly after AD 400.

**Site 391 Malden-Heumensoord**
The *burgus* at Heumensoord, which may have been a former *statio beneficiariorum consularis*, was probably also a watchtower. The published evidence296 indicates two phases, a wooden construction surrounded by an earth-and-timber wall, and later perhaps a stone building and wall.
The ditch (2–5 m wide and a maximum depth of 2 m) enclosed an area of only 33 x 33, later reduced to 22.5 x 22.5 m
As a road-fortlet, the *burgus* was undoubtedly intended to control the route from Cuijk to Nijmegen. It must have existed already in the late 3rd century and functioned throughout the 4th century.

**Site 123 Driel-Oldehof**
The Oldenhof is situated at a short distance to the west of the probable *limes* fort in Driel (site 117/8). In contrast to the latter site, which did not survive the 3rd century, the material from the Oldenhof indicates more or less continuous occupation, but the vast majority of the finds is datable to the 4th (especially late 4th) and 5th centuries. Much of it is described and illustrated in chapter 6.

These finds, and the location of the site, have led to the proposal that we might be dealing with a late-Roman stronghold.297 The most obvious reasons for a shift of the centre of the occupation in Driel from site 117/8 to 123, or rather a concentration of all remaining habitation at the Oldenhof, are indeed military considerations. For example, a watchtower would be more useful if it were closer to the Rhine. However, as already indicated in the earlier publications, definite clues as to the military nature of site 123 are lacking. The finds were salvaged by amateur archaeologists when the Oldenhof was being built over in 1973 (no Roman Period features could be observed). They indicate that the inhabitants were amply supplied with wheel-turned pottery throughout the 4th and 5th centuries, but they do not include definitely late-Roman military artefacts. The presence of a late-Roman watchtower or fortlet in Driel thus remains just a possibility, with regrettably little chance for further investigations in the foreseeable future.

**Site 135 Huissen**
As already mentioned in chapter 8.4.1, the material from Huissen has all been found in a secondary context. It has been assumed that the finds were transported to Huissen by the river Rhine from site 194 or by human agents from sites 194 or, perhaps, 126.

There is, however, no certainty in this respect, and therefore a middle-Roman military settlement in Huissen could not be ruled out completely. That is even more true for the Late-Roman Period, because a substantial part of the finds from Huissen is late-Roman. If they have been transported to Huissen in the Middle Ages from Meinerswijk, there is no problem. If the material originated from the Loowaard, which is more likely, there is a difficulty because site 194 has not yielded clear evidence of occupation in the Late-Roman Period. An alternative explanation could, therefore, be that the
material was originally transported to Huissen not in the Middle Ages but during the Late-Roman Period to build a burgus there from the remains of the fort in the Loowaard. A reason for such an enterprise could be that the shifting Rhine channel had turned Huissen into a site better located for military purposes or, less probably (see below), that the fort in the Loowaard had already been partly eroded.

All such considerations are, of course, speculation and will be hard to prove. The evidence from sites 135 and 546 testifies only to the secondary position of the Roman finds. The presence of an auxiliary fort some 3000 m upstream in the Loowaard is a good reason to consider a middle-Roman fort in Huissen very unlikely. For the Late-Roman Period, such an indirect clue is lacking, which does not strengthen the case for a late-Roman stronghold but at least it does not weaken the already feeble arguments in favour of it.

**Site 194 Loowaard**

As mentioned above, evidence for occupation of the fort in the Loowaard in late-Roman times is absent. The one or two fragments of pottery dating to the late-3rd or 4th centuries have been interpreted as stray finds. In view of the large quantities of material available, the possibility of a late-Roman stronghold at site 194 can be ruled out. Indications for reoccupation from the 5th century onwards are, however, present, which indicates that the site as such was probably not eroded until the Late-Middle Ages.

In addition to the sites mentioned so far, it is useful to examine some other possible fortifications. Indications at site 12, to be discussed in paragraph 8.5.3 (p. 317–8), have already led to the suggestion of a stronghold in the vicinity. In addition, one might argue for a continued existence of the hypothetical frontier fort in Randwijk (18). Although the distribution pattern on Appendix 4 could be interpreted as evidence, it should be remembered that its existence could only be deduced from the necessity for a military post there in the middle-Roman system of preclusive defence. In the late-Roman system of defence-in-depth it may have been useful, but it is not essential. Thus, a continued existence cannot be accepted without additional evidence that it was really there.

Two other possible strongholds are discussed in paragraph 8.5.2. First, there is the vicus in Wijchen (315) which was, relatively speaking, still thriving and surely not without defences. As a major settlement on a branch of the Meuse and at the beginning of a route to Nijmegen (cf. § 3.4.2, p. 69), a military function is not unacceptable. Second, a former villa near Ewijk (239) may have been fortified in some way and have served as an observation-post along the Waal, at the same time blocking the road over the southern levee (cf. below, p. 315 and chapter 12).

Admittedly, the above discussion shows only one thing clearly, namely, the desperate need for excavations at some of these sites. They have, nevertheless, been plotted on fig. 63. The resulting picture is not without its problems, but it shows at least a potentially fruitful explanation of the relatively dense occupation in the eastern part of the eastern river area, which so markedly contrasts with the entire area further to the west. That occupation may well have survived only because it could depend on an early-warning system and on defensible refuges in time of need.

Fig. 63 is, however, not entirely satisfactory. It gives an impression of Nijmegen lying as a spider in its web of outposts, which is probably not true. Even given the paucity of the data, the essential differences between phases from the late-3rd to the early-5th centuries should be taken into account. A modified picture is therefore presented in figs. 88a and b, which at least discriminate the phases before and after c. AD 350. They are based on the evidence from the sites discussed previously, the excavation of site 12, and the finds from sites 239 and 315 discussed below. The nature of much of this evidence does not permit exaggerated claims as far as the implications are concerned: it has, indeed, been pushed to its extreme. If the sites are indeed what they are suggested to be, then Nijmegen was at first not so much a spider in a web but itself temporarily de facto a frontier fortification with outposts only at the direct routes of access to it. After c. AD 350, and preferably after AD 359, the situation is then changed into a triangle of major fortifications at sites 126 (Castra Herculis), 460 (Quadriburgium), and 499 (Ceuclum), with outposts at the routes of access into the area in between.

However tentative fig. 88 may be, it is important to realize that the main elements in the models (the data on sites 126, 403, 460, and 499) are the most secure although, of course, in no way indisputable. The outposts, whether beyond all doubt (391, 466) or uncertain (12, 123, 239, 315), are important in order to detail the reconstruction but they are not crucial for a general interpretation. That interpretation should necessarily also in-
Fig. 88 Possible late-Roman military and/or fortified sites in the eastern river area. Above (a) the situation before and below (b) that after c. AD 350: 1–7 see fig. 58, 8 major fortification, 9 minor fortification, 10 route.
volve developments outside the eastern river area, and therefore further discussion will be postponed until chapter 12.

8.5.2 Civil Settlements

Compared to the civil settlements, the available evidence on military sites is very good indeed. For the entire area south of the Rhine not one single late-Roman civil settlement has ever been excavated. The situation north of the Rhine is better, but in general the information about late-Roman settlements is very meagre. This, of course, seriously hampers an attempt to differentiate between them or to detect relevant changes with respect to the situation in the Middle-Roman Period.

In addition to some specific changes, such as the disappearance of settlement types like the town or the villa, other developments may have taken place. An important consideration, which has often been commented upon in the literature, is that in addition to a decrease in population there was also a concentration of those people who remained in larger and preferably defensible settlements. Although there are indications that this occurred at the level of rural settlements as well (see below), and it is difficult to imagine single farmsteads or loose clusters of them in the eastern river area, the data are too limited to allow a clear picture of the actual situation.

In accordance with the legend on Appendix 4, it seems useful to distinguish between the largest and, because of the finds or their position in the preceding period, more important settlements and the others.

Large settlements

It is not possible to identify a regional centre without running into difficulty, or indeed to determine whether or not such a centre ever existed. Although site 399 was probably not completely deserted, there are no indications of a substantial occupation and thus no reasons to assume that it retained any of its former functions. The erosion of its northern part by the river Waal may have already started during the Late-Roman Period and the rest of the town may have been more an industrial settlement, cannibalizing itself and providing a cheap source of building material for the region.

If anywhere, a regional centre should be located at site 403, on and around the Valkhof. It is logical to assume that at least initially all remaining central functions and officials converged there. It was the largest surviving settlement in the region and there is no objection (cf. note 284) against considering it a fortified civil settlement as well as a military stronghold. While the latter function is easily understandable it is, however, more difficult to discuss its function as a regional centre.

It has been assumed that a civitas Batavorum still existed until the mid-4th century.298 This is indeed the most plausible interpretation, but it can only have been a shadow of its former self in many respects: western and northern parts of the former area may not, or not continuously, have been under the control of the centre. That centre itself may well have been a civitas in its late-Roman meaning of town,299 although a smaller one than Tongeren, Köln, or Xanten. The new archaeological evidence as outlined above does support the proposition that the Valkhof and surroundings were indeed a centre and the site of a major fortified settlement until the mid-4th century after which the civitas Traianensium (Xanten) and a possible civitas Batavorum no longer existed.300 The distribution of late-Roman settlements, clustered as they are in the eastern part of the eastern river area around Nijmegen, may to some extent confirm this proposal. To some extent, because it is only of limited value due to the dating evidence for individual settlements.

That evidence is usually not accurate enough to indicate precisely when they were occupied, which, for the present purpose, is a vital question. It is possible to differentiate the schematic datings of chapter 4 somewhat further by indicating the (few) sites that were almost certainly continuously occupied from the late-3rd into the 5th centuries. Those that were definitely inhabited in the late-4th and 5th centuries can also be indicated without problems but, unfortunately, it is not possible to reach reliable conclusions on the most important question, namely, which sites definitely were and which definitely were not occupied during IIIId–IVA.

For the second half of the 4th century, there is no longer evidence that anything like a regional centre still existed.

vanck 1943, 619 and Von Petrikovits 1978, 217) from the fact that late 4th-century sources (Ammianus Marcellinus XV, 11, 7 and Notitia Galliarum, VIII = ER I, 561) only list the civitas Agrrippinensium (Köln) and the civitas Tungrorum (Tongeren) and no longer mention either Nijmegen or Xanten for Germania Secunda.

298 Von Petrikovits 1978, 217. Bogaers (1960–61, 311) does not express a clear opinion on the subject. For late-Roman civitates and administration, see e.g. Jones 1973, 712–31. See also chapter 12, 434–6 and 442–4.
300 That is at least the conclusion usually drawn (e.g. By-
Site 403 was still occupied and therefore almost certainly fortified in some way, but the excavated ditches were largely filled up and no other features have been discovered so far. Although, of course, still in the centre of the cluster of inhabited settlements which, for this period, can be more confidently identified, its importance may well have been reduced to that of a road fort. In any case, the archaeological evidence seems to agree quite well with the historical evidence: Nijmegen is not listed among the settlements refortified by Julian in AD 359, as were Quadriburgium (site 460?) and Castra Herculis (site 126?).

Both these two sites may have been large settlements in addition to military strongholds, but the archaeological evidence is too limited to draw any conclusions in either case. They may also have functioned as local and, under the circumstances, not necessarily secondary centres for the areas around them. For site 126 there is, however, an alternative in site 123. Especially if that should turn out not to have been a military establishment, it must have been a fairly important civil settlement although it may not have been very large. The size of the ancient settlement soil and the distribution of the finds (excluding late-medieval material) indicate a settlement of perhaps 4 ha there, but the part inhabited during the Late-Roman Period was probably smaller than that. The total lack of late-Roman finds from Elst, while not excluding some hitherto undiscovered form of continued occupation, indicates that this former vicus and secondary centre for the Overbetuwe (the region between Rhine and Waal) no longer functioned as such.

The same is not true for the former vicus and secondary centre for the region between Waal and Meuse, site 315 in Wijchen. The quite considerable amounts of late-Roman pottery, coins, and other artefacts, indicate continued habitation over a fairly large area. Although findspots like the ‘Tienakker’ and ‘Oude Ravensteinseweg’ have, unfortunately, never been excavated, there is ample evidence that the settlement continued, perhaps only in a relatively modestly reduced form. The intensive, but necessarily incidental, efforts of local amateurs yielded no convincing traces of a wall, but there is every reason to believe that several stone buildings were still used. A golden crossbow-fibula from site 316 (fig. 89) is another indication for the status of the settlement, even though it is a stray find and perhaps to be interpreted as a hoard. Roman crossbow-fibulae (drieknoppenfibulae, Zwiebelknopffibeln) are an indication of office (Amts- tracht) and golden specimens are restricted to very high officials indeed. The Wijchen fibula, however, is not Roman but a Germanic, so-called Stützarmfibel. The close relationship between the two fibula-types may point to a similar symbolic function.

Like Wijchen, the former centre for the area south of the Meuse, in Cuijk (site 499/500), probably continued to function as such. The fortified site 499 alone may have measured some 1.2 ha, but there was also habitation outside its walls in (the remains of) the former vicus of Cuijk (site 500).

In addition to the settlements mentioned so far, there is yet another large settlement, namely, Ede-Veldhuizen (site 1) located north of the Rhine. It has not been indicated as a large settlement on Appendix 4, because such an indication would suggest that site 1 is also some sort of central place like Wijchen, which is possible but not at all certain.

Ede-Veldhuizen is a large example of the nucleated villages with a considerable degree of internal structuring discussed earlier (p. 283–4). The total excavated area amounts to 7.5 ha and, although publication of the excavations and an analysis of the various phases still has to be awaited, the main period of occupation and therefore presumably the largest is the late-Roman settlement. It is possible that Ede-Veldhuizen shows a development towards a hierarchically organized community such as comparable settlements in Wijster, Flögen, and Fédersen Wierde. These settlements, to which perhaps also belongs the only partially excavated settlement of Bennekom-Achterstraat (site 7), present a problem of interpretation.

On the one hand, they may be the central places of the increasingly complex society developing beyond or at

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301 As already observed by Haalebos (1976, 205), it is almost inconceivable that Julian should not have devoted attention to Nijmegen if Castra Herculis had been located on the Waal (as Bogaers 1968: Castra Herculis = Druten?). Nijmegen would then be located between Quadriburgium and Castra Herculis. The identification of the latter place with site 126 removes these difficulties (see also chapter 9).

302 See Van Buchem 1966 and Keller 1971, esp. 171–3 for an overview and further references.

303 The find is discussed in detail by Haalebos, in press.

the perimeter of the Roman empire. On the other, it is possible that the unstable political and economic situation in the Late-Roman Period led to an increased need for self-sufficiency and concentration (and thus to the need for a more hierarchically organized society), which led to such large settlements being the normal settlement type instead of the exception at the top of a settlement hierarchy.

At the moment, it is still difficult to decide which of the two situations conforms best to the actual situation. Research over the past decades in the northern part of the Netherlands and Germany has, however, yielded more arguments in favour of the last interpretation. A settlement like Wijster, for example, shows a trend towards self-sufficiency, especially in Period II (c. AD 225-300), 305 and of considerable growth during the late-3rd and 4th centuries which has at least partially been attributed to the arrival of immigrants rather than population growth only. 306 In addition, there seems to be a lack of really small settlements in the same period, but this is quite possibly misleading. 307 More important, however, are the findings for the subsequent Early-Middle Ages, which indicate again less centrally organized villages 308 while smaller settlements are not lacking.

There are, thus, reasons to believe that there was a general trend towards large, nucleated villages with a visible hierarchical social structure, starting in the 3rd and diminishing again in the 5th centuries. 309 This does not necessarily imply that they disappeared completely nor that we may expect to find only those types of settlements. Consequently, it does not imply that a village such as Wijster and perhaps others, such as Ede-Veldhuizen, are nothing special. It only means they were perhaps less extraordinary than might be assumed, being the most pregnant examples of a general trend that lasted for about two centuries.

In this way, the village of Ede-Veldhuizen can be evaluated against the background of the (especially Late-) Roman Period settlement on the 'free-Germanic' sandy soil of the northeastern Netherlands. 310 With the possible, though not very probable, exception of site 123 in Driel, it does not seem to be paralleled by any of the large settlements south of the Rhine. The excavated settlements there do not even look similar. As far as our limited knowledge allows us to say (see below), the characteristic large farms with house and byre under one roof and situated on separate yards within the settlement (cf. fig. 81) do not even occur there. Whatever further hypotheses one may seek to prove, they should thus take into account that perhaps the Rhine continued to be a rather sharp divide between types of habitation. In one way, it was even sharper than before: it is hardly imaginable that such large settlements as Ede-Veldhuizen and perhaps some of the other sites could have existed or, rather, would have been allowed, so close to the Rhine in the preceding period.

Other settlements
Some of the settlements north of the Rhine were partially excavated, but only site 7 (Bennekom), which is comparable to Ede-Veldhuizen, was at least adequately investigated. The excavation of site 17 was a by-product of the excavation of the cemetery (site 16) in Wageningen and yielded only some uninterpretable pits and post-holes. Similarly, the small excavation at site 21 (Quadendorp) produced some Roman Period finds and features, but the investigated area appeared to be mainly late-medieval.

The same dismal picture is offered by the results of excavations south of the Rhine, in those settlements which are considered to have been inhabited during the Late-Roman Period. There are only 5 of those, namely, nos. 75, 109, 143, 150, and 399. The excavation at site 109 was so fragmentary that it has never even been published, 311 and the work at site 399 has so far only produced late-Roman finds, no features. 312 That leaves sites 75 (Zetten-De Hoge Hof), 143 (Bemmel-De Heuvel), and 150 (Ressen-Kerkenhof), none of which

307 The settlements of Denekamp and Dalfsen in the Province of Overijssel, for example, do not seem to be 'very extensive settlements in the style of Wijster' (Van Es/Verlinde 1977, 41). See also Waterbolk 1979, 5 for similar remarks on the settlements of Peelo, Noordbarge, and Rhee in the Province of Drenthe.

308 Waterbolk 1980, 199.
309 The latter development undoubtedly being related to a general discontinuity of settlements (though not of habitation in general) shortly after AD 400. See Van Es/Verlinde 1977, 38-41 and Waterbolk 1979, 12-3.
311 Braat 1937, 23.
312 The same is true for the largely excavated settlement at Wijk bij Duurstede- De Horden (pers. comm. W.A. van Es).
yielded interpretable late-Roman house-plans, although a renewed analysis, especially of site 75, might still produce some clues. As mentioned above, large houses of the type common in the area north of the Rhine have not been found.

It is clear that under these circumstances a discussion of late-Roman settlements can hardly offer any useful new data. None of them is likely to have been very large, but even the question whether they might represent a more concentrated form of habitation cannot be answered. For all we know, the habitation may also have had a rather temporary and makeshift character, with people living there to till the soil but always ready to move to a nearby larger and defensible centre at the first sign of trouble. Such people are, of course, reluctant to invest in well-built homes.

The same situation may not only apply to the continuously or reoccupied native villages but also to former villae. For the same reasons that they were considered to have existed in the river area during the Middle-Roman Period, villae had undoubtedly ceased to function by the end of the 3rd century. Even in the south of Germania II, on the loess areas of the civitas Tungrorum and the civitas Agrippinensium, where quite a few villae survived, their owners must have had a hard time.

Nevertheless, some of the sites interpreted as villae in the Middle-Roman Period, were still or reoccupied in the Late-Roman Period. Of course, this does not necessarily imply they were organized and functioned in the same way as before, but they still may represent a different type of settlement. Of the sites in question, nos. 80, 81, 85, 109, and 298 need not be discussed because they are not definitely identified as villae. The villa from Overasselt was excavated but still produced only a few late-Roman sherds; it was, although in view of the very limited excavation in retrospect not on reliable grounds, considered to have been uninhabited (cf. Appendix 4: isolated finds). That is certainly not true for the remaining sites, nos. 59, 234, 239, 250, 291, and 508, which have not been excavated but where sometimes considerable numbers of late-Roman artefacts were collected at the surface.

Site 59 is different from the others because it was already deserted in around the end of the 3rd or the beginning of the 4th centuries at the latest. Perhaps significantly, the others are all located south of the river Waal but, in view of the distribution of villae in the 2nd-3rd centuries and the five possible villae mentioned above (4 in the Overbetuwe) that may not really mean anything. All five sites were inhabited in the late 4th century, but only for sites 234, 239, and 508 are there indications for occupation during the late-3rd and early-4th centuries as well.

The most interesting settlement is Ewijk–De Grote Aalst (site 239), where the surface finds indicated—in the context of the eastern river area—an extraordinary luxurious establishment (cf. p. 279). It may not be a coincidence that an equally extraordinary series of late-Roman coins has been found there (see figs. 141 and 142). For a few years after its reputation had been established, the site enjoyed intense and not entirely wholesome attention paid by coin hunters with metal detectors, which surely is part of the explanation. But other sites have been examined by the same people, in all but one case (no. 244, Beuningen–De Heuve) with much less result and it cannot (yet?) be assumed that comparable quantities were normally lost at all other sites.

Within the context of the eastern river area, comparable amounts of currency have only been found in Cuijk (somewhat less, but found before the age of the detector) and Nijmegen. This may well indicate some sort of function comparable to those two sites, the precise nature of which remains unknown for the moment. Instead of attributing the finds to continued use by a wealthy villa owner, such an interpretation is much more realistic. The site may have been a defensible refuge for the people living there and in the surroundings (sites 226, 234, 244, 250), perhaps with central and protected storage facilities and (at the same time?) a watchtower. It may have been selected both because of its comparatively luxurious nature and its position close to the river Waal.

All this does, of course, in no way mean that the people living at site 239 did not also work the land or that they may not have tried to uphold some standards of villa life. The same may be true for the other villa sites as well. Two of these (234, 250) are close to site 239, a third (291) is close to Witzen (315), and the fourth (508) directly opposite Cuijk (499). For all of them, we must assume that a convenient refuge was close at hand, but this is again an observation whose significance cannot be evaluated. Perhaps there were even in our area people foolhardy enough to invest in a villa in a relatively peaceful period and the coin evidence from a site such as De Heuve (244), which did not even have stone buildings, may also be indicative of economic activity. But except for

313 See, e.g., Von Petrikovits 1978, 244.

314 See below, figs. 141 and 142 and p. 446–9.
site 239 there are no reasons to assume that settlements at the site of a former villa were any different from those at other sites. It is not even certain they still lived in a stone house, as long as archaeological proof is lacking.

8.5.3 Cemeteries

Just as in the case of the settlements, the number of known late-Roman cemeteries is far smaller than in the preceding period. Relatively speaking, it is even smaller than that of the settlements, because the maximum number is only 10, which is nearly 13% of the number during the preceding period. Compared to the settlements (25% surviving), the number of burial sites is definitely too low. There is a convincing explanation for this phenomenon, namely, the change in burial practices. Cemeteries in general are not as easily discovered as settlements, but late-Roman cemeteries are even more difficult to find than middle-Roman ones. Those are almost invariably cremation burials in relatively shallow graves which are frequently disturbed and thus discovered during modern agricultural activities and the like. Most of the late-Roman interments, however, are inhumations in comparatively deep pits (shaft graves) which would normally require a much deeper disturbance of the soil before they are discovered. They are, in fact, very often found during the construction of buildings which provides an additional disadvantage, and not only because the possibilities for archaeological investigations are often limited in such a situation: the aversion of contractors to reporting accidental finds is too well known! Convincing as the above explanation may be, the relative scarcity of late-Roman burial sites is nevertheless remarkable. It is not uncommon for late-Roman interments to lack grave goods,315 which could mean that substantial numbers may have disappeared without the slightest attention being paid to them, especially during construction work. But that nothing at all is found and, in the end, comes to the attention of some professional or amateur archaeologist, is not probable. After all, a cemetery will normally have at least some graves with complete vessels or other objects which are not likely to be thrown away as often happens to sherds from a settlement. In addition, the presence of inhumations is bound to attract attention (cf. site 386) and, as far as archaeological interpretation is concerned, attention to the fact that one might be dealing with a late-Roman grave. Especially when Roman cemeteries contain late-Roman graves as well, the possibility that they are not identified is small.

This last observation leads to what is most remarkable about the lack of late-Roman burials. As mentioned above (p. 303), virtually all late-Roman settlements are either continuously inhabited or reoccupied middle-Roman sites. If the same should apply to cemeteries, many more should have been known. Therefore, while the data clearly point to a prolonged use of the same sites for habitation (which is not the same as continuity of occupation!), they indicate equally clearly a discontinuation in the use of burial sites. This conclusion is also supported by the known late-Roman cemeteries. Only sites 292 and 410 were definitely used in both periods. The burials at site 413, and certainly those at sites 418 and 419 are doubtful (see below) and the (small) spatial overlap between sites 409 and 410 has been shown by the excavations to be completely accidental. The same could be true for the middle- and late-Roman graves at site 292, but there might also be real continuity in this case. It is difficult to interpret this change. It is entirely conceivable that it means nothing, as might be concluded from the best documented example, the Nijmegen cemeteries. The new cemeteries at sites 405 and 410 may be merely a result of people being practical: they were living at site 403 and buried their dead around its perimeter, the other sites being too far away (e.g. 398) or by then already long fallen into disuse. On the other hand, Nijmegen may be an exception because the other settlements did not move and yet their cemeteries were no longer used. Admittedly the entire archaeological data base leaves much to be desired, but one (292) more certain case of continued use out of 78 is too little to be a coincidence.316 It is concluded, therefore, that deliberate socially or ideologically motivated choices must have played a part, and it could be significant that the possible instances of continuity (292, 413, 418, 419) reach only to the mid-4th century.316

To move from these general considerations to particular burial sites, only 6 of these are definitely late-Roman. Site 386 is an inhumation burial covered with seven tegulae and is probably but not definitely late-Roman. The material of the cemeteries at sites 413, 418, and 419,


316 See further chapter 12, 444.
which were looted rather than excavated, includes a minimal amount of late pottery, whose origin, however, is not beyond all doubt.\textsuperscript{317}

The two largest cemeteries are those around Mariënburg (405) and Hugo de Grootstraat (410; also known as cemetery ‘OO’ or ‘Margriet’), substantial parts of which were excavated after the last war.\textsuperscript{318} The former is estimated to have consisted of approximately 1500 interments (c. 750 excavated), the latter presumably held around 800 burials (c. 350 excavated). Both presumably started in about AD 300, but the Mariënburg cemetery survived into the Merovingian Period while interments at the smaller graveyard must have stopped in about or shortly after AD 400.

The very careful excavation of the Margriet cemetery and new methods such as the biochemical analysis of bone, which permits a differentiation of age and sex even when most of the skeleton has decayed,\textsuperscript{319} will hopefully provide a detailed insight into the social and biological composition of the local population. It has already been established that site 410 contains a group of Germanic and, in all probability, military burials.

The population indicated by the two cemeteries has been estimated at c. 460 and over 200, but these figures may need further specification. The provisional reports indicate that, at least at the largest cemetery, the majority of the burials might belong to the period before AD 350. Nevertheless, the number of late 4th- and early-5th-century graves from both cemeteries is not inconsiderable, which means that the remaining population at site 403 was not too severely reduced or, to be more exact, that the number of people living at site 403 after c. AD 350 was not too severely reduced. After all, we know that the population’s ethnic identity may have undergone considerable change (see chapter 12).

The two graves around Wijchen (292, 321) are accidental discoveries and can contribute little to our understanding of the population. They contained quite a few grave goods, a characteristic that also seems to be especially common in early-4th-century graves in Nijmegen. No further investigations followed the original discovery.

317 See Stuart 1963, 25, 48, 109, and 111 for the pottery. The findspots of the pottery are usually considered to be reliable (e.g. Brunsting 1937, 6), but there was no control whatsoever over the find-circumstances and errors are not impossible.

318 Provisional reports in Wynia 1979, 66-8 (site 405) and Bloemers 1983, 193-8 (site 410). Complete publication is currently being prepared by P.A.M. Zoetbrood.

The two cemeteries north of the Rhine (12, 16) are different from the others because the late-Roman phases at both sites only constitute the limited beginnings of what were to become large Merovingian cemeteries. The oldest burials in both cases date to the late-4th and early-5th centuries. In Wageningen (16) these are only a few, grouped around a Neolithic barrow.\textsuperscript{320} Interestingly enough, some interments are still cremation burials. These were also present (but exceedingly rare) in Nijmegen, which may illustrate a difference in burial practices known to exist between the Roman empire and the Germania Libera, where cremation was still quite common in the Late-Roman Period.

One or a few late-Roman cremation burials are also attested to in Rhenen (12), where the majority of the 34 graves from the first phase is a clear group of inhumation burials.\textsuperscript{321} In fact, one might as well consider these a separate cemetery at a short distance from the main Merovingian graveyard starting in VB.

Although a coherent publication of the Rhenen cemetery is still lacking, various important bits and pieces can be found scattered in the recent literature. The most important contribution for our purpose is Böhme’s analysis.\textsuperscript{322} In his opinion, the cemetery, with 62% of the male graves containing weapons, is clearly military... und hat sicherlich zu einem spätrömischen Militärposten nördlich des Rheins gehört... Böhme’s conclusion is not, of course, as absolutely certain as presented: the relation between Germanic warrior graves and late-Roman military strongholds is not an entirely unambiguous one and such a site near Rhenen remains to be found. On the other hand, while perhaps stated too confidently, the interpretation arose from a thorough comparative study and is surely more than a mere possibility. By itself, the evidence indicates that a late-4th-century military post in Rhenen is not improbable although the character of such a facility at that location remains an open question.

An additional argument is provided by the geological situation. A fortified watchtower at the site of present-day Rhenen or on the Grebbeberg would be situated on the high Pleistocene ice-pushed ridge. That would not...
only provide an easily defensible position but also an excellent look-out post and opportunities for signalling. Such a station would also be conceivable in the context of what is known about the eastern river area and against the general background of the late-4th-century situation in Germania II. A rejection of Böhme’s conclusion for the sole reason that Rhenen is situated on the Rhine is quite untenable.

8.3.4 Isolated Finds
Most of the isolated finds from the Late-Roman Period are stray sherds, some of which have been mentioned in previous paragraphs. One stray find, the bronze hairpin recently discovered at site 199 along the river IJssel (see chapter 7), should be mentioned here because it is not indicated on Appendix 4.

Only one category of late-Roman isolated finds, the hoards, deserve special attention. Not more than three of these could be indicated as such on Appendix 4, but there are several other finds which might also be considered hoards. In some cases that seems to be an acceptable interpretation, such as the relatively valuable gold coins (stray finds), but there are also finds reported in the literature which are rather dubious and a few which have to be rejected. To sum up, the following finds are concerned:

A The area north of the Rhine
1 Site 9, Rhenen (Achterberg-Friesesteeg). Hoard of two faceted gold torcs of the so-called ‘type Velp’ and a fragment of a third torc of different form, decorated with five glass paste stones. See fig. 145. The finds, discovered in 1938, were published by Roes (1947). The total gold weight amounts to 212 grams. The hoard has been dated to IVB, preferably in IVd, and may have been buried in about AD 400.

2 Renkum–De Beken (probably not far from site 21). One gold coin of Gratianus (ER III, 164), found before 1826. No other data are known. There is a possibility that there is yet another hoard from the area Wage-ningen–Renkum, consisting of a few gold coins (latest specimen: Magnentius), but there is no really tangible evidence in this case.

3 Site 30, Velp–Hervormde Kerk. Large hoard, consisting of six faceted gold torcs which led to the designation ‘type Velp’, a fragment of a torc of plain round gold wire and a fragment of a similar but decorated torc, as well as three finger-rings. The finds were discovered in 1851 and, fortunately, published immediately afterwards by Janssen. The total gold weight is 530.4 grams. The dating of this hoard conforms to that of Rhenen and it can be assumed to have been buried in about AD 400.

4 Site 32, Velp–Grote en kleine Durk. The extraordinary collection of finds from this site as described by Pleyte (see catalogue) might be considered a hoard. In view of the artefacts involved and the absence of data on the find circumstances, such an interpretation is not very likely to be correct.

5 Emmerich–Elterberg (c. 209/341). One gold coin of Honorius. No other data are known.

B The area south of the Rhine
6 Ressen–Bemmel. The aureus of Constantinus I reported from this site (site 150) was not included in the catalogue because it was almost certainly not found there and quite possibly not even in the river area.

7 Site 244, Beuningen–De Heuve. One solidus of Arcadius, stray find.

8 Site 316, Wijchen–Wijchens Meer. The small golden fibula from this site (see p. 313 and fig. 89), weight 5.5 grams, could be interpreted as part of a (washed-down) hoard: it was found in soil from the former branch of the Meuse. Obviously, other interpretations are also possible. It is datable in the late 4th or the first half of the 5th century.

323 See below, chapter 12. Note that, in contrast to what has been said earlier about middle-Roman frontier-forts (chapter 3, p. 57–8), the location of a late-Roman heavily fortified installation north of the river is unusual but not unthinkble. In addition, and perhaps more likely, the soldiers involved and thus their ‘station’ may have been very irregular indeed.

324 See Ypey 1975, 57, who otherwise agrees with Böhme on the basic data provided by the finds. His only argument, cf. his notes 6–8, is based on the no longer tenable hypothetical construction of a late-Roman ‘Waal-limes’ by Bogaers 1967 and 1968.
9 Site 399, Nijmegen--Fort Krayenhof. Probably a hoard, consisting of one damaged gold bracelet decorated in the style of the torcs of the Velp type, found in or before 1901 near the former fort Krayenhof, and thus in the area of Ulpia Noviomagus. The weight is 18.25 grams and the dating and estimated period of hiding are identical to the hoards of Rhenen and Velp--Hervormde Kerk.

10 Site 403, Nijmegen--Valkhof. One gold coin of Valentinianus, found in 1786.

11 Megen (c. 167/425). One gold coin of Arcadius. No other data are known.

12 Site 499, Cuijk. One gold coin of Valentinianus. No other data are known.

13 Site 467, Kessel--Niers. Hoard, consisting of three solidi of Constantinus I, Constantius II, and Magnentius, as well as a golden ring with a stone of rock-crystal. It was found in 1935 and is probably a washed--down hoard.

In addition to the above-mentioned finds there are two other gold coins from Nijmegen and, most notably, the first hoard of Velp (site 28, Velp--Het Laar), found in 1715. There are several viewpoints from which these finds should be discussed together with those mentioned so far. Nevertheless, there is also a difference because the hoard from site 28 was buried in the mid-5th century and thus de facto in a post--rather than a late-Roman context. It has been indicated on Appendix 5 for that reason, at the same time symbolising the absence of abrupt changes between 'late-' and 'post-'Roman, as already outlined in chapter 2.4. These are, after all, no more than analytical historical concepts which may obscure issues rather than clarify them. However, it is difficult to believe that the owner of the wealth stored in the gold artefacts hidden at site 28 felt compelled to do so for reasons which were very different from those of the owners of the other hoards a few decades earlier.

From the available evidence it is quite clear that the eastern river area has its fair share of the enormous amounts of gold that were hidden in the late-4th and early-5th centuries both inside and beyond the imperial frontier. A treasure such as the second hoard from Velp (site 30) is not surpassed by many other hoards and in itself is an indication that its owner must have been a high-placed an powerful individual. It is entirely conceivable that the torcs such as those from Rhenen and Velp were status symbols of very high-ranking individuals. It is generally accepted that they were manufactured from melted down solidi, but it is rather improbable that they should be 'nichts anderes als eine Art der Aufbewahrung einer grösseren Menge Gold bei Völkern, wo das Münzwesen kaum oder nicht von praktischer Bedeutung war'. The fact that the torcs in some of the hoards show hardly any traces of wear does not justify such a conclusion. It only means that they were not regularly worn trinkets, which is hardly surprising for such valuable objects, and it is not incompatible with an interpretation as status symbols. Such symbols are often reserved for display on very special occasions. Furthermore, it is difficult to understand why the solidi were melted down in the first place, if the purpose was only to preserve the gold. In addition, the practical use of gold coinage inside the empire was very limited as well, and yet torcs--or equivalent objects--are practically absent there. With some exceptions, such as the

330 Braat 1954, 3.
332 De Boone 1954, 220, sub B.
333 Boersma 1963, 37 (with partially wrong references).
334 BJ 142, 1937, 196, 202; 143/4, 1938--9, 292.
336 Waterbolk/Glasbergen 1955, 100.
337 See e.g. Reece 1977, who suggests that it was important only in dealings by and with the state. Cf. also Frere 1978, 415--6.
one bracelet from Nijmegen (which is not even definitely a hoard), hoards from inside the empire appear to have consisted entirely or almost entirely of coins and normally do not contain the equivalent in solidi of even one torc (some 12 to 19 coins of c. 4.5 grams).338

This is quite possibly due to the fact that the gold expended by the state in political payments outside its frontiers could not be retrieved as was most of the precious coin used to pay the imperial military and civil organization which returned into the hands of the state (cf. Reece’s model of gold circulation inside the empire).

Perhaps the above reasoning can also be employed to use the distribution of large gold hoards to establish a divide between the area where the Roman state was still able to retrieve its gold successfully and the area where that was not possible. Because that divide evidently coincides with the Rhine it might be concluded that the river also marks the line inside which gold was paid out in retrievable form such as salaries and beyond which it was used for political payments to people outside the cycle of gold circulation and thus outside direct imperial control.

Such a reasoning is not without its risks, but, as is indicated by the hoards from Rhenen and Velp and also other evidence discussed previously in this chapter, the line was rather sharply defined around AD 400, at least from a supra-regional point of view. A hoard such as that from Menzelen shows that the Rhine as a divide should not be overaccentuated at a regional level even though the same hoard may also be interpreted as a chronological divide which illustrates (rather than marks) the end of the cycle of gold circulation. In any case, while incorporating a rather dangerous element of reasoning ex nihilo the distribution and size of hoards do support a picture of fairly effective Roman control in the eastern river area up to the Rhine until the early-5th century.

8.6 THE MEROVINGIAN PERIOD

The entire post-Roman period to be discussed here, labelled ‘Merovingian’ for convenience, was defined in chapter 2 as lasting from c. Vb to VIIIa. It thus spans almost exactly three centuries, approximately twice as long as the preceding late-Roman phase. This long duration is undoubtedly the major reason why the total number of known settlements is larger, although it is still much smaller than during any of the phases before the late-3rd century, including the Late-Iron Age. This observation is in accordance with the picture presented by the numerous available pollen diagrams, which show a maximum of tree pollen and a minimum of culture indicators in the 5th and 6th centuries.

The total number of known settlements between Vb and VIIIa is 104, to which can be added the sites in Oosterhout (545) and Escharen (548) mentioned in chapter 7. Of this total number, 10 sites are only probably inhabited settlements and 6 are mere possibilities. There is, of course, no evidence that all these sites were inhabited during the entire period under discussion. The archaeological material does not, however, allow too much chronological differentiation because the most frequent finds, sherds of the Merovingian coarse ware, notably the steep-walled cooking-pots discussed in paragraph 6.2.7 (fig. 48), are hardly suitable for that purpose.

Only the settlements which were definitely inhabited during the 5th century can be reliably distinguished from the others because of the presence of some shorter-lived pottery forms, such as the ‘type 3’ cooking-pots discussed in chapter 6.2.5 (p. 168-170). There are 25 such sites, including the doubtful settlement in Huissen but, more important, without even one Nijmegen settlement. In Nijmegen, reliable finds indicating habitation throughout the 5th century are still lacking, although continued use of the Mariënburg cemetery (site 405) has been assumed.

The figure of 25 settlements is, of course, only a minimum number because there are other sites where late-Roman as well as Merovingian habitation has been demonstrated (such as Nijmegen!) and which are at least likely to have been inhabited during the 5th century. In other cases, such as Ede–Veldhuizen (site 1), the final desertion of the settlement may well have taken place only towards AD 500. On the other hand, not all of the 106 Merovingian settlements are likely to turn out to have been inhabited during the 5th century, and thus an increase in the number of settlements throughout the Merovingian Period is a logical assumption.

It is, in fact, more than just an assumption because the 400 solidi (BJ 151, 250–1 and op. cit.) may be less relevant because it was buried in the mid-5th century, at least after AD 425.

338 See Bloemers 1983, 198–200. A notable exception is the hoard of Menzelen near Xanten (Kaiser-Raß/Klüßendorf 1984) which contained at least 207 solidi and was buried very shortly after AD 412/13. The other famous Xanten hoard of c.
evidence from the pollen diagrams mentioned above very clearly leads to the same conclusion. The values for culture indicators show an increase before the end of the Merovingian Period (such as some Hordeum, the appearance of Secale, and the spreading of Plantago and Rumex\(^{339}\)). A third source of information in this respect are the cemeteries (see below). Certainly on sites such as Rhenen (12), Wageningen (16), or Lent (159), there is an increase in the number of burials from the 5th or the 6th into the 7th centuries which again points to a general process of population growth in the river area as a whole. Finally, the distribution of the sites compared to the preceding period should also be mentioned (see Appendices 4 and 5). A concentration of habitation in the eastern part of the eastern river area such as in the Late-Roman Period is no longer observable, which might be interpreted as evidence for the resettling of virtually deserted areas in the west.

On the basis of the available evidence it is clearly a very risky undertaking to provide estimates for the total number of settlements in the area during the three centuries after the end of the Roman occupation. The archaeological and pollen-analytical data indicate that the maximum number cannot have been anywhere near that of the five centuries before the late-3rd century. If the estimated numbers of 250–300 for the Late-Iron Age and 350–500 for the Middle-Roman Period are indeed in the correct order of magnitude, then the maximum number of inhabited sites at the very end of the Merovingian Period, in the early-8th century, may have reached 200. Given the number of settlements known to date, the probability of a substantial number of undiscovered sites on the Pleistocene soils, and the probability that the dependence on relatively scarce wheel-turned pottery caused the Merovingian occupation on a number of known sites to remain unnoticed, a figure of 200 does indeed seem to be a reasonable estimate. Comparable arguments for the 5th century lead to an estimate of some 50 settlements, although the relatively very limited means for dating a site in the 5th century may also allow a higher estimate: 50–100? On the other hand, the near absence of culture indicators in the pollen diagrams can hardly be reconciled with higher figures even though the return of the forest undoubtedly prevented the wide dispersion of pollen, and most if not all of the settlements may have been very small indeed. Unfortunately, little is known about this last subject or, for that matter, about most aspects of Merovingian settlements.

### 8.6.1 Settlements

Although there have been a number of excavations at settlements which were also inhabited during the three centuries after the Roman Period, there are only a few sites where at least some coherent features from that time were also included in the investigated part. The excavations at sites 21, 105, 109, 126, 143, 150, 403, 461, 473, and 479 are of no use in the present discussion. That leaves only the investigations at sites 75, 246, 444, 466, 500, 536, 548, and perhaps those at site 1.

The latter site (Ede-Veldhuizen) may, as mentioned above, have existed until well into or the end of the 5th century. Although the site does not appear on Appendix 5 because of the conclusions on its chronology in the preliminary reports, the datable wheel-turned pottery certainly allows a different conclusion. In addition, the settlement of Wijster, comparable in many ways, is nowadays also considered to have been inhabited throughout the 5th and even into the 6th centuries.\(^{340}\) The latest phase of Ede-Veldhuizen, as a nucleated village with a considerable degree of internal structuring,\(^{341}\) may thus represent one form of 5th-century habitation. Unfortunately, a final publication and plans of the successive phases are still lacking, which prevents a more detailed discussion of the structure and size of the final phase.

The small excavation at De Hoge Hof in Zetten\(^{342}\) produced finds which are datable, and a series of features which could be datable, between the Roman v. ‘Carolingian and later’ traces. The excavation remained too small to yield interpretable plans although, as was also mentioned for the late-Roman occupation here, a reanalysis of the original documentation may provide useful information. The excavations in Millingen (site 444) and Angerlo (site 536) are similar to Zetten in that the excavated area is too small.\(^{343}\) For both sites, Merovingian habitation has been demonstrated and a few features were identified, but the core of the occupation from that period was either not excavated or only very partially, and thus uninterpretable, touched upon.

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339 See e.g. Teunissen/Teunissen van Oorschot 1980, 263 and especially the overview in Teunissen 1982, 27, fig. 11. See also fig. 137.
340 Waterbolk 1984, 22.
341 See above, p. 283–4 and 313–4.
342 Braat 1937.
The excavations in Cuijk (site 500), where a continuous habitation in a probably continuously important settlement can be assumed, only yielded a few features between the late-Roman fort and that of the (early-?) Middle Ages which was destroyed in AD 1132. The summary publications only mention one or probably two sunken huts, one of which was presumably used for weaving and datable to the 7th century. The presence of other possibly intermediate but undatable features is not recorded. If these were indeed absent, this may well have been due to the extensive later disturbance which could have erased all but the deepest features. Of course, the continued use of late-Roman buildings is, in Cuijk, also a phenomenon to be reckoned with.

Although not entirely relevant in the present context, the late-Merovingian or early-Carolingian temporary occupation of site 466 (Asperden) should also be mentioned here. It is not a settlement in the sense of the concept as employed here, but a single activity site (and thus essentially an isolated find) where people remained for a short time to rob (at least) the iron from the late-Roman burgus and to rework it on the spot. The latter has left traces of activities which are usually difficult to identify but which must already have been regular occurrences during the Late-Roman Period.

The only two sites where, with respect to the early-medieval occupation, more successful excavations were carried out in our area are Beuningen (site 246) and Escharen (site 548). Although both excavations have only very recently been completed and the analysis of the results has yet to begin, it is clear on both sites that the Merovingian settlement is only a small part of the total settled area, which is datable to later periods. The settlements as such probably consisted of only one or two contemporaneous houses. What these houses looked like, and the way in which they were organized into a settlement are, obviously, questions which cannot be treated here.

Unfortunately, evidence on these aspects from outside the eastern river area is also limited, especially as far as accessible published information is concerned. Only for the northern Netherlands has the work of the BAI re-

345 Hinz/Hömberg 1968, 190–1. The site was not indicated on Appendix 5 because it is most likely to have been early-Carolingian.
sulted in a more or less coherent picture with excavations such as those of Wijster, Odoorn, Eursinge, and Peeloo. A report is also available for the settlements of Kootwijk (see fig. 80) and Hoog Buurlo on the Veluwe. Apart from a few other published excavations such as those in Rijnsburg and Grubbenvorst, there are only two large excavations for which partial and preliminary reports are available: Den Burg on the island of Texel and Dommelen in Northern Brabant.

Leaving aside the details of the intricacies of the typological and functional development of houses, it appears that there is gradual and uninterrupted evolution from the Roman Period onwards. For the northern provinces that has already been conclusively demonstrated, and for the Veluwe the same phenomenon can be recognized. Although most Merovingian houses appear to be fairly small, large structures with a clearly recognizable byre have been found in the north and in the settlements of Rijnsburg, Ede-Veldhoven, and Kootwijk-5, but are so far lacking in the southern Netherlands.

In general, the houses form settlement units together with accompanying structures such as barns, a well, and, as a very characteristic element, sunken huts. Sometimes these units are clearly marked yards, differentiated from their surroundings by ditches or palisades (fig. 90). Occasionally, burials are also associated with the settlement units (Dommelen, Escharen). Most of the excavated settlements appear to be similar to those at sites 246 and 548 in that they were composed of only one or a few of such units, indicating a rather dispersed form of habitation. The large nucleated villages such as Ede-Veldhoven and Wijster (see fig. 80) disappeared during the 5th century, to return again in basically the same form after the Merovingian Period. For the eastern river area, virtually all sites may well have been more or less similar, small settlements.

On the other hand, not all settlements were necessarily the same. The poverty of available data indicates that direct archaeological clues for differentiation are virtually absent. There are, however, still a few sites which may have been of special importance and perhaps also larger, although the evidence is circumstantial and indirect. The first of these is, of course, Nijmegen, the Noita of the Anonymus Ravennas (see fig. 140), which was presumably never outside Frankish control. It is assumed to have been the base from which Dagobert I conquered the central river area in VII b, and which was later used by Charlemagne as one of his residences. Leupen postulates the oldest parish church of Nijmegen on site 403 (Valkhof) as early as c. 600 AD. Archaeological evidence for the post-Roman habitation in Nijmegen is, however, meagre. Occupation of the Valkhof is probable but, apart from the 7th century cemetery (site 404) there, without direct evidence. The cemetery may have belonged to the church. Other traces of Merovingian occupation were discovered around the Korte Brouwersstraat (site 401) and in the 1983 and later excavations at the Waalkade. Some habitation in this low-lying area along the Waal can thus be considered proven, but its character and extent are as yet unknown. An elongated settlement along the bank of the Waal, perhaps even a fairly large one connected to the Valkhof, may, however, easily escape attention: it may have been largely eroded by the river. Nevertheless, its existence as a relatively large site needs to be documented by more finds; even material in secondary position is limited so far.

The historical information is thus more indicative of a centre (though not necessarily in an administrative sense) of some importance in Nijmegen than the archaeological data, which only indicate habitation. Indirect archaeological evidence for the status of Nijmegen is also provided by the Niomago coins (see fig. 92) in the hoard from Escharen (site 514), which were probably minted in Nijmegen in the late-6th century. The activity of a monetarius presupposes economic activities of some importance and thus the presence of a mint in Nijmegen testifies to its central function as well as to the economic importance of the city.
revival in the region around it; the central importance of the river area as a whole can be deduced by various means. 359 Apart from Nijmegen, the secondary centre and military stronghold in Cuijk from the Roman Period may have remained important in the Merovingian Period. Continued use of the Roman road to Nijmegen and its strategic position at the Meuse-crossing, continuity in the name Ceulum–Cuijk, and the presence of the later castle (destroyed again in the 12th century) are all indications for such an interpretation. 360 Another settlement which may have remained some sort of centre is Wijchen (site 315), with evidence for the 5th to 8th-century (and later) occupation over the entire area of the Roman vicus. Corroborating historical data do, however, seem to be lacking. In addition, the Wijchense Maasje started sitting up in the 5th century which should have negatively influenced the development of the site. To consider site 315 as a relatively large settlement with some central functions is thus rather doubtful for the period at hand. The same is to some extent true for site 123 in Driel. The 5th to 8th-century (and later) finds are plentiful and some are even extraordinary, but the site as far as it is known today was not large. The first source naming Driel (Dryele in Betua) dates only from 1262, but the name itself may be older: if it should be a -lo-name, a dating in at least the end of Merovingian Period is necessary. 361 On the other hand, the Rhine crossing at the Drielsche Veer was a vital link in the very important early-Medieval north-south route from the Veluwe to Nijmegen and the location of the Hunneschans or Duno, a ring-fort from before AD 1000, on the other side of the Rhine, can hardly be a coincidence. 362 In addition to the above-mentioned sites, there is enough archaeological evidence at sites 23 and 126 in Meinerswijk to assume a direct relationship to the Meginhardswisch (AD 814) or vicus Meginhardi which was looted by Vikings in AD 847. It seems logical to assume that the evidence for continuous occupation here is evidence for the development from the late-Roman stronghold Castra Herculis into the early-Medieval emporium. The available archaeological information is discussed in chapter 9. There are a number of other places known from historical sources which may have been important settlements, but for which corroborating archaeological evidence is usually not available. In contrast to place-names in classical sources, those mentioned in early-Medieval sources do not almost automatically imply that the sites were of at least regional significance. Furthermore, in most cases what is known about these settlements seems to refer to the 7th and 8th centuries or later. It might be more relevant in the context of the late-Merovingian and Carolingian (or even later) developments than in the present context, which is primarily concerned with what remained after the Roman Period and not so much with what was built again on and from those remains. Nevertheless, there are a number of sites which should at least be mentioned here. North of the Rhine, these are Rhenen, Wageningen, Arnhem, and Elten. The evidence for Rhenen (AD 855: Hrenhem) as an important place is summarized by Heidinga, who mentions, for example, an exceptionally large ring-fort at the Grebbeberg. 363 In chapter 8.5 (p. 310 and 317), the possible presence of a small late-Roman stronghold in Rhenen was already referred to and with the additional evidence of the Merovingian cemeteries here (below, chapter 8.6.2) one might consider Rhenen an important site during the 5th-8th centuries. The possible importance of Wageningen (possibly AD 838: villa Uuaganuuega), 364 opposite the presumed Roman fort in Randwijk and on the north-south route crossing the Rhine at the old Lexkesveer, is indicated by its geographical position. The large Merovingian cemetery (site 18) on the only part of this route where the actual road can be identified with certainty (the Diedenweg) could provide archaeological support for a post-Roman settlement of some size. 365

359 E.g. Heidinga 1984, part III, chapter 2, who considers it as a nuclear region or kerngeest (see esp. p. 201, fig. III, 2:2) and Ewig 1980, 45, who called attention to the presence of mints in Nijmegen and Dorestad on the one hand and Köln and Maastricht on the other, but also to their absence along the Rhine and Meuse in the area between these regions.
360 Cf. note 344.
361 Cf. the comparable arguments for (Maas)Driel in Blok 1980, 35. Another toponymical derivation is, however, possible.
The historical evidence for Arnhem (AD 893: *Arneheym, Arneym*) was recently discussed by Verkerk. Although there is some evidence for a Merovingian cemetery (site 25) there and the situation across the Rhine from Meinerswijk is significant, there are no definite data to support an interpretation as an important settlement before the 8th century.

An administrative centre during the 9th and 10th centuries was situated on the Elterberg, once the seat of Wichman, Count of Hamaland, who also owned the *praedium Arnhem* and a *praedium and curtis* in Renkum (perhaps the above-mentioned Hunneschans or Duno). There is, however, no firm archaeological evidence for any habitation before the late-9th century. Definitely Merovingian finds are lacking and the Roman material was probably transported to the site from elsewhere.

South of the Rhine, Elst (AD 726: *villa Eliste*) is, of course, also a settlement to be considered here. Although there is evidence for Merovingian habitation at site 105, there are no indications for a continuous occupation and not even for a continuity between the Roman temple and the first church which could 'not possibly' date to the 8th century. The fact that *villa Eliste* belonged to the domains of Everhard, confiscated before AD 711 because of his disloyalty to Childebert III, does not necessarily imply that it was of special importance. On the other hand Elst, which must have had important ruins — if not actual stone buildings — surviving from the Roman Period and thus have been a desirable property, is also located in the geographical centre of the Betuwe and very favourably situated in relation to overland routes. In any case, if the current assumptions on the later developments are correct, a church was built on the site of the Roman temple only after AD 726 when Elst was given to Bishop Willibrord in Utrecht.

It is possible that close to Nijmegen the village of Beek (AD 814: *villa Bechi*) was of special importance. The finds at site 420 show that it may have been an industrial centre (pottery production). In addition, it is known that in AD 826 Louis the Pious and his son Lotharius traded a large estate in northern Italy to a count Boso for his relatively modest possessions (eight *mansi* and a chapel) in Beek. As Leupen observed in his study on Beek, this exchange implies that Boso's property must have been of considerable value to them. This may, however, as Leupen suggests, not have been the intrinsic value of the villa itself and there is no definite connection between the archeological and historical data which are at least 100-150 years apart.

Further to the east, the definitely continuously occupied Qualburg, and perhaps also Rindern, may also have been of some significance. Rindern is, for example, mentioned as the place near which the Viking chief Godfrid was slain by Count Everhard of Hamaland. Finally, the town of Kleve seems to have become important only in later times.

### 8.6.2 Cemeteries

Although Merovingian graves are usually as deep and as difficult to detect as late-Roman burials, the total number discovered so far is surprisingly high. The maximum number of known cemeteries in the area covered by Appendix 5 is 32 and, while still deficient in many ways, the information about them is better than that on settlements from the same period. After all, excavations of cemeteries were (and are) more readily started and complete pots or other objects are not as easily discarded as a few sherds.

There are, of course, great differences in the level of knowledge about the various sites. The most important excavated cemeteries are those in Rhenen (site 12), Wageningen (site 16), and Lent (site 159). Burials were also excavated in Huissen (site 134), Nijmegen (site 404 and 405), Donsbrüggen (site 454), and Escharen (site 549), while the grave goods of several graves (parts of cemeteries) were salvaged from Beuningen (site 247), Hermen (site 287), Wyler (site 435), Deursen (site 484), and Wijchen (site 519). In addition, there are older reports on finds from cemeteries where the available information leaves much to be desired, such as those on the Grebbeberg (not in the catalogue) near Heelsum (see chapter 5 under site 21 and note 13), in Millingen (site 441), and near Klein-Linden (site 497).

The remaining sites so far all represent either definitely

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366 Verkerk 1983; see also Heidinga 1984.
372 Leupen 1977, 387.
374 Gorissen 1952.
or presumably only one grave. In many cases they are only the findspots of one undamaged vessel, usually a biconical pot. With some exceptions it can safely be deduced that these pots must have originated from accidentally disturbed graves, for which sometimes additional proof exists.

The largest cemeteries, those in Rhenen and Wageningen with c. 1100 and c. 800 (originally probably also over 1000) burials, were used from the Late-Roman Period onwards. They are, in fact, a separate category of cemetery because of their size as well as their chronology. It is not inconceivable that such cemeteries were central burial grounds for a population from several villages instead of belonging to one large settlement. There is as yet no proof for such an assumption, but even if there were that would not necessarily invalidate what was said above about the possible significance of Rhenen and Wageningen in the Early-Middle Ages.

Neither of the cemeteries has yet been fully published, which hampers a discussion of their interpretation beyond the two distinguishing features already mentioned. A sensible framework for such a discussion has recently been provided by Steuer, together with an overview and critique of older interpretations, but it cannot be applied here. For the moment, we must be content with the observation that the two cemeteries represent a much larger burial community than all others. It is also noteworthy that, while the Rhenen cemetery generally speaking is the richer of the two, exceptionally wealthy interments have not been recorded at either site. This does not mean that rich burials are lacking, for example, the male burial 775 from Rhenen recently published by Ypey, with such grave goods as an umbo, snaffle bit, bucket, the sheath of a (disappeared?) sax, part of a (disappeared?) bronze bowl and two sets of gold-covered silver fittings with central discs in the shape of D-bracteates. With a few other burials this interment surely belongs to the relatively rare Qualitätsgruppe C as defined by Christlein, which represents people in a relatively high social position. It is, however, not comparable to the really rich burials of group D, which are absent in the eastern river area. There is only one (remote) possibility or such a grave, namely, the presumably female burial from Beuningen (site 247), which is in fact probably also a ‘type C’ grave and was uncovered by a contractor under somewhat obscure circumstances. It must have contained, among others, a splendid necklace (fig. 91) composed of 13 amethyst, 1 amber, and 10 glass beads with no less than 13 gold pendants and a gold tremissis from Dorestad, monetarius Madelinus (total gold weight 10.2 grams). Comparatively speaking, the value of the objects is certainly not excessive, but the

376 See the comparable arguments forwarded in the JROB 1981, 76 (on Wageningen) and by Heidinga 1984, 178–80.
377 For the recent excavations in Wageningen see JROB 1981, 76. For Rhenen, see Ypey 1973a, Böhme 1974, Ypey 1978 and 1983.
379 This may not be such a clear difference after the new excavations in Wageningen have been published. In addition, part of the central area of this cemetery was destroyed.

Fig. 91 Reconstruction of the necklace of Beuningen (site 247), without the glass beads. Scale 1:2.
grave goods from Beuningen are quite possibly not complete.\textsuperscript{384} From a chronological point of view, the only cemetery comparable to those of Rhenen and Wageningen is site 405 in Nijmegen which was also in use in the Late-Roman Period. Compared to its former size, this cemetery, as far as it is known to date, seems to have been rather small in Merovingian times,\textsuperscript{385} a situation which is exactly the reverse for the other two. There are several other sites which were also used for burials during the Middle-Roman Period, namely, nos. 134, 217, 241, 454, and 474, but there are no cases where continued use can be demonstrated. The size of the chronological gap, which may amount to four centuries, is too large to allow any hasty speculation on this point.

As has already been mentioned, the evidence from most Merovingian cemeteries, as far as they can be evaluated, points to a start in the 6th century while the majority of the burials dates to the 7th century. It is probable that the majority will prove to have been of medium size, comparable to the graveyard of Lent, which contained some 140 interments and may be ascribed to a community of only a few households.\textsuperscript{386} It belongs to Steuer’s type 2 and at least the smaller of its two groups of graves contained burials with a snaffle bit belonging to Christlein’s group C and thus the interments of people of relatively high rank.

The presence of small cemeteries from a single household can also be expected, as well as the isolated and very small graveyards of high-ranking individuals sometimes referred to as Adelsgräberfelder.\textsuperscript{387} Site 247 may be a case in point. Whether or not isolated graves occur is rather uncertain. With the exception of a few Fürstengräber, genuine isolated graves are apparently unknown.\textsuperscript{388} There is no evidence that any of the single graves recorded in the eastern river area so far was indeed isolated and in view of the lack of evidence from elsewhere there are no reasons to assume they were present.

From the available evidence on Merovingian cemeteries it is not yet possible to present far-reaching conclusions. That they come in different sizes is obvious, and so is the fact that there is a substantial amount of discontinuity in the use of burial sites from the previous period. In fact, it might be significant that there are eight cases (25\%) where Roman cemeteries were used, even though real continuity exists for only three of them.

It is also worth noting that the number of wealthy people identifiable by ‘group C’ grave goods seems to be quite considerable. There is a group of those in Lent and the number in Rhenen will undoubtedly prove to be rather large. They are even present in the part of the ‘poor’ cemetery from Wageningen that has been published.\textsuperscript{389} Together with other data, such as the burial from Beuningen, the probable activities of a monetarius in Nijmegen, and the presence of hoards and stray gold coins (to be discussed in the next paragraph), they testify to an economic revival and the relative importance of the eastern river area. Evidence for very high-ranking officials and nobility is, however, absent after the mid-5th century. Only the owners of the two Velp hoards and perhaps that of the one from Rhenen may be classified as such.

8.6.3 Isolated Finds

Most of the isolated finds indicated on Appendix 5 are stray sherds and a few other objects. Only the hoards and the stray gold coins, some of which might be hoards as well, merit a separate discussion. There are, in fact, quite a few of those in addition to the two known hoards of which only one is Merovingian. The following finds are concerned:

1. Site 28, Velp–Het Laar. This hoard, which was discovered in 1715, has already been mentioned in paragraph 8.5.4 because it can also be considered ‘late-Roman’. The hoard is only partially known because most of the finds, a large but unknown\textsuperscript{390} amount of gold coins, rings or torcs, and five 5th-century gold medallions originally attached to a chain, have been dispersed. Only three of the medallions, with portraits of Honorius and
Galla Placida, have survived. Of the coins, specimens are mentioned from Constantinus I and his sons, Valens, Gratianus, and Johannes, as well as a Valentinianus. The hoard can thus be dated to after AD 425 and was presumably buried around the mid-5th century.

2 Site 97. Homoeo--De Hoge Woerd. Tremissis from Metz, monetarius Garoaldus, AD 630–640 (660).

3 Site 171. Flieren-dorp. Tremissis Anastasius, probably Visigothic imitation, AD 491–518.

4 Site 403(?). Nijmegen–Valkhof(?). In his discussion of the coins from Nijmegen collections, Daniëls mentions two aurei of Valentinianus III (AD 425–455). The findspot ‘Valkhof’, or indeed ‘Nijmegen’, is not absolutely certain.

5 Site 468(?). Dieden. Tremissis Justinus II (AD 565–578). Gallic imitation. The exact findspot is unknown.

6 Demen (c. 171/425). Tremissis Justinus I (AD 518–527). The exact findspot is unknown.

7 Beers (c. 186/415). Medallion, cast of aureus of Antoninus Pius, dated to c. AD 630. Found in 1802 between Cuijk and Beers.

8 Site 445. Millingen-kerk. Gold coin Justinianus (AD 527–565), found in 1847 close to the church of Millingen.

9 Site 514. Escharen-Graafschche Raam. Hoard of 66 solidi and tremisses, found in 1897. Total gold weight c. 108 grams. The hoard must have been buried around AD 600, the latest coins having been minted during the reign of the emperor Maurice (AD 582–602). The hoard contained two solidi and five tremisses of Magnia vico, minted in Niomago (fig. 92). Although not entirely certain, this Niomago can probably be identified as Noviomagus--Nijmegen, and contribute to the evaluation of the importance of sites 401/403 in post Roman times.

10 Escharen. At a very short distance from the large hoard, a tremissis of Justinianus (AD 527–565) found in 1983, and a small hoard of nine, presumably early-8th-century, sceattas. In addition to the above-mentioned finds, two other stray coins should be mentioned here. They are both gold coins of Maioranus (AD 457–461) and were found in Lienden in the Betuwe, 2 km west of the area covered by Appendix 5 (c. square 164/439) and ‘s Heerenberg in the Montferland, only 4 km east of our area (c. square 214/432).

Together with the tremissis of Anastasius from site 171, these coins more or less bridge the gap between the perhaps still ‘late-Roman’ gold finds from sites 28 and 403 and those of the 6th to early-8th centuries. In fact, the available evidence indicates that gold coinage continued to be imported into the river area after the Roman period. The quantities are less than during late-Roman times, but that is primarily due to the extremely rich hoards which were buried in a very short period from the end of the 4th to the early-5th centuries, culminating in the first hoard from Velp (site 28).

391 Daniëls 1950, 3. The aurei of Valentinianus III mentioned in ER III, 97 and by De Boone 1954, 224, sub B, are probably the same.

392 Boersma 1963, 55, sub 53c.

393 Hermans 1865, 27; Boersma 1963, 55, sub 53a.

394 Boersma 1963, 30, sub 8a.

395 Spann 1967, 15.

396 Lafaurie 1960.

397 The other possibility is Noviomagus-Neumagen (Blok 1979, 25–7), but Nijmegen is generally accepted (also e.g. Ewig 1980, 45).

398 JROB 1983, 151 (tremissis) and Verwers 1983, 42–3 (sceattas).

399 De Boone 1954, 140.
storing wealth in gold remained possible and, if the evidence from the Escharen hoard is interpreted correctly, gold was already minted locally in the 6th century. The list of finds is perhaps large enough to conclude that a system whereby gold was retrieved by a central authority, such as may have been the case in the late-Roman period, did not exist or function effectively. That is consistent with, for example, the information provided by the Historia Francorum of Gregorius, Bishop of Tours, on the large royal hoards which were continually depleted by costly gifts which subsequently must have spread widely, a substantial number eventually ending up in burials. The original royal hoards seem to have been acquired from outside sources by such means as conquest, confiscation, or ransom rather than by taxation, and most of the gold moved from the top downwards or between local elites, in a horizontal direction, and not upwards again. In any case, the number of stray Merovingian gold coins is larger than those of the Late-Roman and considerably larger than those of the Middle-Roman Period. Of course, this phenomenon can also be attributed to the insecurity of the area and the unstable political and economic environment from the late-3rd century onwards. In that case, the argumentation implies, in fact, an interpretation of the stray gold coins as hoards but that is not exactly a foolhardy assumption. The relative value of gold coins was enormous, as was so pointedly illustrated by Zadoks, who concluded that the late-Roman hoard of Hapert (Province of North Brabant), which consisted of no less than 2598 small bronze coins, must not have been worth much more than half a solidus! Which does, of course, not mean that there were no taxes and other ‘internal’ means of income. See Weidemann 1982, 327–38.

A process leading to the edict of Theodoric (AD 483–526) prohibiting excessively valuable grave goods and even sanctioning the robbing of graves in some cases (cited by Steuer 1982, 500).

The mechanisms involved in the circulation of gold are, of course, more complex than suggested here and urgently need further study, cf. Van Regteren Altena/Theuws 1984.

Zadoks-Josephus Jitta 1955, 108. One solidus was sufficient to buy provisions for a whole year (Steuer 1982, 451).

9 THE ROMAN FORT AT ARNHEM-MEINERSWIJK

9.1 INTRODUCTION

The site of the fort in the polder Meinerswijk, across the Rhine from Arnhem, was discovered in the course of an inspection of previously ignored ancient settlement soils recorded by a soil survey of the Betuwe. A small excavation, intended to provide information on the suspected nature and on the chronology of this new site, was carried out in September 1979. The other circumstances leading to this enterprise have been outlined in chapter 8 and in the preliminary reports. The geological situation, the interpretation of which proved to be of much wider relevance and first led to new insights in the Roman Period fluvial system in the northeastern part of the river area, was discussed in chapter 3.

The size of the site as indicated by the ancient settlement soil is give in fig. 18. From profiles AB and CD, it appears that the easternmost extension of the soil is virtually identical to that of the original settlement, while only a small part of the formerly settled area in the south was eroded by a late-Medieval or even later channel coming from the east. Precise information on the western boundary is lacking. Part of the area has been dug out, presumably for the clay, but that is likely to have occurred even after it had been eroded by the river. Data on the possibly still surviving part of the site to the north, the entire area behind the road north of the excavation trench (fig. 93), are completely lacking. It is covered by brick factories and inaccessible for any observations (see fig. 11). At least the finds from site 23 (fig. 93) give some idea about the possible maximum extension of the original site which may well have suffered greatly from erosion by the Rhine which, in Roman times, presumably followed a course north of site 23. The excavation trench, although near the centre of the area which still survives, is thus probably located in the southern part of the Roman Period settlement, or rather settlements, considering, as mentioned in the previous

1 Zegers/Zandbergen 1958. See also chapter 3, pp. 71–2.
2 Willems 1980a and b; chapter 8, pp. 255–6.
3 Chapter 3, pp. 54–5 and fig. 18.
4 This is the so-called groene rivier (green river), still used as an additional outlet for the Rhine during times of very high water-levels (Kuper a.o. 1978, 3–4).
chapter, that it is likely to have consisted of a military fortification with associated civilian habitation.

The trench itself, due to limitations of time and money as well as the objective of the excavation, measured only 5 x 40 m, later extended by an even smaller trench of some 2 x 11 m to the south. Due to the complicated and very deep stratification of the site, only part of this area was excavated. The northern half of the trench, with the foundation trenches of stone walls, was left intact after removal of two levels, and only the southern part was investigated to deeper levels although, unfortunately, not to virgin soil. Due to unfavourable weather conditions but especially the proximity to the river, normal pumping became ineffective at 2.5 m below the surface and futile at a depth of 2.8 m when a sterile level was still not reached. From the geological profile CD in fig. 18, (chapter 3, p. 54), the northern part of which coincides with the west profile of the excavation trench, it could be deduced that virgin soil may only be found at 3.1 m to as deep as 3.5 m below the present-day surface. It cannot, however, be taken for granted that the first occupation of the site coincides with the top of the sand and gravel of the pre-Roman channel deposits. At least the lowest parts of the sandy clay on top of these deposits may also predate the first human occupation, although the pollen analysis indicates that at the very least an inhabited site cannot have been very far away. 5

9.2 THE EXCAVATION

An overview of the excavated features is presented in fig. 94. 6 It shows only one complete plan, at the second excavation level, some 40-50 cm below the surface. Because the circumstances did not allow complete excavation, it was decided to leave the robber trenches intact and to concentrate further efforts on the southern part of the trench with a dark-black fill in which some features, notably a possible ditch, were nevertheless visible.

The robber trenches clearly indicate a large stone building with a wall some 6 m to the south of it. As shown by the material in the robber trenches, the walls must have been built of tuff. The width of the trenches is 1.50-1.40 m for the outer wall and 1 m for the inner wall. As shown by a section through the latter (fig. 94, profile AB), this probably corresponds fairly closely to the original width of the walls. There is no evidence to date the removal of the stone, but it is at least clear that it was done effectively with a minimum of effort, and only to obtain the tuff. The actual foundation was left intact. It consisted of a 40-cm-thick bed of large blocks of greenish sandstone with relatively little quartz, which might be described as grauwacke. In their turn, these were founded on (probably six) rows of closely spaced, rammed in oak posts, a common method of construction on an unstable or damp subsoil and also used for the nearby temples of Elst (site 105). 7

There are no indications about the size or purpose of the building. Even though the robber trenches reach virtually to the present-day surface, they were already clearly visible at the first excavation level at 15 cm below Specimens of wood from this and other features were submitted to Dr W.A. Casparie (BAI Groningen). All constructional timber proved to be oak.

5 See Teunissen a.o. 1985, 22.
6 On a separate map: Appendix 6.
7 Bogaers 1955, 47 (see also Haalebos 1977, 37, note 59).
the surface, it proved to be almost impossible to trace them. Several attempts by an electrical resistivity survey during and after the excavation produced no results. Air-photography in 1979 and 1981 also remained without success, although one attempt by the National Aerospace Laboratory in 1979, with an infrared line-scanner (fig. 95), showed a probably more or less rectangular or square structure some 60 m southwest of the excavation trench. Borings at that spot have shown that the features do not represent walls, but they might be ditches of some sort. Other borings, around the excavation trench, showed that the southern wall of the building extended for at least 10 m to the east.

Some 6 m south of the building, another robber trench is indicated on fig. 94. It reaches only half-way into the excavated plane and in the preliminary publication it has been assumed that there must have been some sort of opening here. However, at the first excavation level the robber trench went straight across, as indicated by the interrupted line. In view of the efficiency of the methods used to remove the stone walls, it is not impossible but rather unlikely that a trench would have been dug where there was no stone. Unless this is true after all, there should thus have been a continuous wall but the lack of a foundation indicates that it may originally have had an opening which was later closed. If there was such an opening, it may correspond with an interruption in one of the ditches, if that feature is indeed a ditch.

It is also possible that the wall is not contemporaneous with the building because there is a slight difference in orientation between the two. Because there is no direct stratigraphical relation between both features, a discussion of the possible significance of this will be returned to after the presentation of the stratigraphical data.

These data were obtained in the southern part of the excavation trench, which showed a very complex picture of natural deposits and layers due to the artificial raising of the soil as well as features due to both natural and human agents. The stratigraphy could not be properly interpreted from the various plans during the excavation, but only after the entire profiles became available. Especially because of the limited width of the excavation trench, this did not interfere with the attribution of finds to relevant stratigraphical units in later analysis, with the exception of some material from the lower levels: from c. 2 m below the surface layers of brown, overlying greyish-blue, clay were encountered and finds kept apart. Only after studying the profiles in the field did it become clear that the border between the two was crossed by archaeologically relevant levels and was merely a natural phenomenon, representing reduction of the clay (from brown to greyish-blue).

In the following interpretation of the stratigraphy, the attempt has been made to obtain a more or less general archaeological periodization for the site. The excavated area is of course much too small to guarantee that this

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8 Two attempts during and shortly after the excavation by Ir R.S. Sporry, of the Adviesbureau Arnhem BV (Heidemij Group), and one in 1980 with equipment provided by Dr I. Scollar (Rheinisches Landesmuseum Bonn) failed, presumably because the subsoil in Meinerswijk is too wet.
periodization will long outlive more extensive excavations in the future, but this is all the more reason not to attempt a very detailed periodization which might just be accidental and relevant only to this part of the site.

9.2.1 Period i
As mentioned earlier, it is not certain that the phase of the occupation history of the site designated here as Period i is indeed the first. Earlier features and, more significantly, earlier finds are, however, lacking. The trajectory in profile CD belonging to Period i is indicated in fig. 96. It consists of an up to 1-m-thick deposit of greyish-blue and brown sandy clay which is only very slightly polluted (fig. 96, H). It does contain, however, a quite considerable number of artefacts throughout the fill. At the moment, it is not quite certain whether the deposition of this clay was due to human or to natural agents, or to both, although the possibility of natural deposition is certainly the least probable one. In view of the period of time involved and the thickness of the deposit, completely natural sedimentation can almost be ruled out. The original top of this deposit has been preserved in the southern part of profile CD, where it is covered by c.-cm thick layers of gravel reaching to 8.90 m NAP, with a maximum width of 4.5 m (fig. 96, G). There can be little doubt that this zone of gravel, which has the same orientation as most of the other (later) features in the excavation trench, represents (the remains of) a metalled road.

Immediately north of this road the profile shows two depressions, reaching below the deepest excavation level (fig. 96, B and E). As already indicated in the preliminary report, it is possible that these represent the upper part of two parallel ditches. If this is true, the ditches should belong to Period 2, although it cannot be excluded that they in fact still belong to Period i. The original surface of the deposit in which they were dug has almost completely been eroded by a later gully (fig. 96, D), which is an additional argument for the ditches because at times of high water gullies follow the easiest path along depressions such as ditches. The southern slope of the gully shows bands of washed-in gravel, originating from an eroded part of the road (fig. 96, F). The northern slope of the northern depression is more interesting, because it is covered by an up to 20-cm-thick layer of humic brown sand (fig. 96, A). The same layer is also present on the southern slope of this depression although more vaguely and perhaps in an eroded and redeposited state (fig. 96, C). The pollen analysis of this same layer in profile CG (see Teunissen a.o. 1985, figs. 3 and 4) has shown that, as well as the fill of the gully directly above, contains an unusual and very high amount of Calluna pollen. This indicates that the surface of the slope and perhaps of the entire inhabited area north of the road has been deliberately covered with sand and heather, presumably harvested on the Veluwe north of the Rhine. There are no definite archaeological indications that sods were involved. Their transport to the site would have required an enormous labour investment but that is in itself not at all impossible. The sandy nature of the layer may well be an indication for sods, even though they could not be seen in the profile. In addition, the pollen diagram shows rather important deviations, limited to the trajectory through this layer, notably the minimum values for Compositae, Cyperaceae, Dryopteris t, and allochtone pollen in addition to the high value for Calluna, which at least leave open the possibility of sods.

Undoubtedly, this cover of the surface was applied to gain a surer and drier footing, perhaps also to prevent too much erosion causing a rapid silting up of the ditches (if there were any). Comparable measures, such as covering the slope of ditches with sods, are known for example from Nijmegen. Covering the surface of low-lying and wet ground with a carpet of harvested plants is also known from other sites, such as the bracken covers found in Vindolanda. The pollen analysis has, by the way, also provided evidence for the use of bracken (Pteridium aquilinum) from the deepest (unexcavated) level up to the beginning of the levels ascribed to Period 3. It is quite likely, therefore, that bracken was harvested on the Veluwe as well, and for a longer time. The 'heather' layer, whether belonging to this period or the next (it did not contain any finds), was in any case due to artificial raising of the soil. The genesis of the underlying deposit of greyish-blue and brown sandy clay is more difficult to interpret. It contains a chronologically rather homogeneous group of pottery throughout the

9 See also Teunissen a.o. 1985.
10 Birley 1977, 122 and 130–1. Perhaps also relevant in this context is the evidence for the use of a specially imported (because it grows only in a salt-rich environment) variety of rush (Juncus gerardii) in the fort at Woerden (Bogaers/Haalebos 1983, 309).
entire stratum which at least proves that it does not predate the occupation and that its formation took place in a relatively short period of time. As mentioned earlier, this makes a completely natural deposition process rather unlikely. On the other hand, artificial deposition would have left some traces and the pollen diagram shows no indications of disturbances. For the moment, the most reasonable interpretation seems to be that the process of deposition was natural in principle but greatly accelerated by human influence: clay from the immediate surroundings (and thus occasionally artefacts) being added.

The finds
With the exception of one find from the gravel of the road, all material originates from the thick deposit of sandy clay indicated in fig. 96. These are finds collected after the correct interpretation of the profile and the recognition of the sharp border between the greyish-blue and the brown clay as a natural phenomenon as mentioned above. The result of its earlier misinterpretation as an archaeologically relevant border is that there are two groups of finds collected before then, which cannot be attributed with certainty to one archaeological stratum. For the present discussion only one of these (no. MW 1-0-23) is relevant. It may contain some material from levels ascribed to Period 1, although the vast majority can only belong to Period 2 and a small fraction might be from Period 3. It will be discussed in the next paragraph. Relevant material from the definitely Period 1 deposits concerns the following finds:

12 The other group (no. MW 1-0-24) is only very small and will be omitted from the discussion because it contains no relevant finds.

Fig. 96 Meinerswijk, profile CD, Period I. Relevant deposits are shaded. A-H, see the text.

Terra sigillata
1 Fragments of an almost complete plate Haltern 2 (fig. 97, 1) with missing foot-stand with a central stamp VITILIS (see chapter 6.2.1, p. 157 and fig. 33, no. 51).
2 Wall-fragment of a plate Haltern 2, probably from the same plate as no. 1.
3 Bottom fragment of a dish, presumably Haltern 2 (fig. 97, 2), with a central stamp CLA (see chapter 6.2.1, p. 153 and fig. 33, no. 10).
4 Wall-fragment of a cup, possibly Haltern 8.
5 Wall-fragment of a dish Haltern 2.

Gallo-Belgic ware
1 Two orange-to-brown coloured base-fragments, presumably of butt-beakers Haltern 84 (fig. 36, nos. 2–3).
2 Two wall-fragments, at least one of a chocolate-brown butt-beaker Haltern 85 (fig. 36, no. 4) decorated with wavy lines.
3 Two wall-fragments of cork-urns Haltern 58/91.
4 Base-fragment of a terra nigra cup with foot-stand, probably form Haltern 78–80 (fig. 97, 3).

Smooth ware
1 Neck and rim of two flagons Haltern 47, the neck-fragment with a 4-ribbed handle (fig. 97, 4–5). Another 4-ribbed handle (fig. 97, 7) might be from the same flagon as the rim-fragment.
2 Rim of a flagon Hofheim 50 (fig. 97, 6).
3 Neck of a two-handled flagon with neck-rings and 3-ribbed handles, Hofheim 58 (fig. 97, 8).
4 Fragments of five two-handled flagons Haltern 52, four of which have 3-ribbed handles which is an early
Fig. 97 Meinerswijk, finds from Period 1 deposits, 1–13 pottery, 14 glass.

characteristic (figs. 97, 9-10). The only find from the road-gravel, a 3-ribbed handle, belongs to this group.

5 Neck of large two-handled flagon with a flat rim and very slight neck-ring (fig. 97, 11). Typologically, the specimen may be compared to Oberaden 52 (Stuart 133), but the jugs with a neck-ring (but a very different rim) Oberaden 53/Haltern 53 may also be related. The shape of the neck does not, however, suggest a very early date.

6 Pointed amphora-base and wall-fragments of oil-amphorae Dressel 20.

7 Two wall-fragments of flagons with graffiti on the shoulder (fig. 98). One is an unreadable fragment. The other is probably a military inscription which could be read as > IHCv, and interpreted as: (centurio) LEG (ionis) v [Alaudae]. The legio V was stationed in nearby Xan-

14 See also the discussion in Schönberger/Simon 1976, 95–6 (types 37–38).
15 I am grateful to Dr J. van der Werff (Amersfoort) for his help in the determination of the amphorae.
16 Reading by J.E. Bogaers (Nijmegen).

Fig. 98 Meinerswijk, graffiti on flagons from Period 1. Scale 1:2.
ten from AD 9-69/70. An alternative reading might be L III CY, L(egionis) III CY(renaiaca). Although perhaps not entirely impossible,\(^{17}\) the normal abbreviation of Cy-renaica seems to be CYR instead of CY and the interpretation of the first sign as an L is not very plausible. In contrast, the second sign may well have been an L and there are no real objections against a reading of the last as V instead of Y. Nevertheless, the first reading of the inscription cannot be taken as definite.

Coarse ware
1 Rim-fragments of two cooking-pots with everted rim (fig. 97, 12-13) Stuart 201.
2 Fragment of the neck of a jug, no rim, presumably Haltern 54/Hofheim 86A.

Glass
Rim-fragment of a pillar-moulded bowl of monochrome, bluish green glass, Isings 1957, type 3 (fig. 97, 14).

Metal
Bronze terminal for a strap with a rivet still in its place (fig. 104, 1).

Dating
The dating of Period 1 is primarily determined by the terra sigillata. The forms (service 2) and the stamps indicate that we are dealing with a late phase in the production of Arretine ware. On the other hand, not a single fragment of South-Gaulish ware is present. Although that may not be interpreted as hard evidence, in view of the relatively small total numbers of pottery, the most probable dating is surely the second decade AD. This is confirmed by the available smooth ware, which is dominated by Haltern forms 52 (with 3-ribbed handles) and 47. But there are also some flagon-necks which are more correctly identified as a Hofheim-type. The combination of these forms in a deposit which can hardly be later than c. AD 20 and is also very likely to post-date the occupation of the Haltern camp (ended in AD 9) is, in fact, exactly what was to be expected. It agrees well with Stuart's conclusions on the period between Haltern and Hofheim.\(^{18}\)

As far as the other pottery and glass is concerned, the wares and forms fit into the proposed dating. No fragments of fine wares, which could have contributed to the dating, are available.

9.2.2 Period 2
With the exception of the two possible ditches mentioned in the previous paragraph, there are no features which can be ascribed to Period 2. From the profiles (see figs. 94 and 99) it appears that the surface of the previous settlement at the site of the excavation has been largely eroded by a natural gully, following the course of the presumed ditches (fig. 99, A). Part of the gravel of the road (fig. 99, D), the top of which may also belong to Period 2, was washed into this gully (fig. 96, F), as well as a wooden conduit-pipe or drain (figs. 99, B and 101). The channel of the gully seems to have been filled in fairly quickly and probably by natural means: except for the washed-in gravel on the southern slope the fill is homogeneous.

The fill of the gully is sealed by a partly preserved and up to 20-cm thick strongly polluted dark layer, contain-

\(^{17}\) See RE XII, 1508, s.v. Legio [Ritterling] and especially Schillinger-Häfele 1977, 531-2 (no. 152).

\(^{18}\) Stuart 1977, 15-6.
ing much charcoal (fig. 99, C). As visible on profile EF (fig. 94), it gently slopes down to the south. It seems as if Period 2, at least at the site of the excavation, is characterised by two calamities: it starts with a flood and ends with a fire. The pollen analysis of the trajectory through the fill of the gully indicates more or less a return to the relative frequencies observed underneath the 'heather' layer.

The finds
All except one of the relevant finds originate from the fill of the gully. Unfortunately, as explained in the previous paragraph, the finds probably contain a few sherds from the underlying Period 1 deposit and also from the gully-fill above, ascribed to Period 3.

Terra sigillata
1 Large fragment of a bowl Drag 29 (see chapter 6.2.1, p. 143 and fig. 26, no. 11), Neronian or early-Vespasian and presumably a product of Passenus. This is the only sherd from the layer with charcoal overlying the gully-fill.
2 Rim and base fragment of two early South-Gaulish cups Drag 24/25 (fig. 100, 1–2).
3 Base- and wall-fragment of two South-Gaulish cups Drag 27, the base with an unreadable stamp, and a base fragment of a dish.

Gallo-Belgic ware
1 Wall-fragments of a butt-beaker decorated with apparently only one band of applied ribs and manufactured in a well-fired smooth white ware (fig. 100, 3). The beaker is possibly a further development from the ribbed beaker (Grätenbecher), Oberaden type 103.
2 Base/wall-fragments of a globular beaker with barbotine spikes (Stachelbecher) in terra nigra, Hofheim type 106.
3 Wall-fragment of an ovoid beaker decorated in barbotine (Perlurne), Hofheim type 118.

Smooth ware
1 Neck of a flagon Hofheim 50 (fig. 90, 4).
2 Neck of a small flagon Hofheim 51 (fig. 100, 5).
3 Neck of a flagon which seems to be of the type Stuart 110B (fig. 100, 6). Although this mid-2nd-century type does clearly differ from the other finds, it is not from find no MW 1-0-23, but from a secure stratigraphical posi-
tion. Even though the inside of the rim is only very slightly concave, it may be that we are dealing with a variant of Hofheim 53.19

4 Complete flagon Haltern 52/Hofheim 57 with two-ribbed handles (fig. 100, 7).
5 Wall- and base-fragments of oil-amphorae Dressel 20, neck-fragment of a wine-amphora Pélichet (1946) 47, and wall-fragment of a fish sauce-amphora Dressel 7-11/Pélichet 46.

Coarse ware
1 Complete beaker Hofheim 85 A/C (Stuart 204) (fig. 100, 8) and one rim-fragment of an identical specimen.
2 One complete and one fragmentary cooking-pot with everted rim Stuart 201 (fig. 100, 9–10).

Native ware
Rim-fragment of a cooking-pot with inturned, faceted rim (fig. 100, 11).

Building material
1 Two fragments of tuff.
2 One fragment of limestone.
All three fragments are from find no. MW 1-0-23, which means they originate from the level of reduced, greyish-blue clay. Because not even the tiniest crumb of building material was observed in the layers ascribed to Period 2 in the profiles, the (fairly large) stone blocks probably belong to Period 3. Only the deepest part of the gully-fill from that phase (see fig. 102, B) consisted of the greyish-blue clay and that is, in fact, the most logical place for such fairly heavy objects to be deposited.

Organic material20
1 Parts of washed-down oak posts.
2 Part of a wooden conduit-pipe, consisting of a hollowed-out trunk covered by a lid (fig. 101). In contrast to all other wood (constructional timber), the pipe and the lid were both manufactured from alder which is not very durable but easy to work. Comparable pipes in oak are known from other sites.21

Dating
Apart from one or two exceptions, the datable material presents a fairly coherent picture. The finds from the gully-fill are no later than Vespasian and more probably pre-Flavian. The ‘charcoal’ layer above it is dated by the Neronian or early-Vespasian bowl Drag 29. The combination of these facts leads almost inescapably to the conclusion that we must be dealing with the ubiquitous ‘layer of AD 69’ and that Meinerswijk suffered the same fate as so many other forts along the Rhine which were destroyed during the Batavian revolt.

As far as the beginning of Period 2 is concerned, the situation is more difficult to evaluate. If any finds actually originating from the Period 1 deposit are among the finds, these cannot be identified with certainty. In principle, the available evidence allows the conclusion that there was continuous occupation over Periods 1 and 2. The flooding and the gully may in addition have been very local phenomena and of no consequence for the site as a whole. For the moment, however, this has to remain an assumption.

With the exception of two fragments of tuff and one of limestone, no building material except wood was found. The fragments belong to find no. MW 1-0-23 and thus very likely originate from the deposit above the gully-fill. The total absence of other stone, even in the charcoal layer, support this conclusion.

19 See esp. Ritterling 1912, 283, fig. 64, 1–7.
20 The zoological material from Arnhem-Meinerswijk has been examined by R. Lauwerier (BAI) and will be published separately.
21 See Jacobi 1934, 52–7 and fig. 14, 8.
9.2.3 Period 3

From the profiles (figs. 94 and 102) it is evident that the surface of the site was raised after the events which led to the formation of the charcoal layer (fig. 102, E). In the southern part of the trench this layer is covered by an up to 30-cm-thick deposit of greyish-brown clay (fig. 94, profile EF). In the northern part, the remaining depression of the gully was filled with the same material and covered with a 20-30-cm-thick layer of sods (fig. 102, A). As appears from the pollen diagram these sods were probably cut locally, in the immediate surroundings of the site. There are no obvious and sudden changes in the trajectory through the sods.

At exactly the same spot as the previous one(s), a new road was built, with a metalling of gravel with a maximum remaining thickness of 30 cm (fig. 102, D). North of and apparently parallel to this road, a V-shaped ditch was dug which originally must have been between 1.50 and 2 m wide and 1.25 to 1.50 m deep (fig. 102, C). The function of this ditch is unknown. It is perhaps more likely to have been a road ditch than part of the defence works, but the relation to the road and raised surface is lost because the ditch must have attracted water during a high-water period and once more a gully was formed (fig. 102, B) which eroded part of the previous gully-fill and the layer of sods above it, as well as the northern side of the road, most of the gravel landing in the ditch and completely filling the lower (50-60 cm) part of it. The lowest part of this gully silted up or, less likely, was filled in again fairly quickly after the flooding.

The finds

The finds from these various deposits belonging to Period 3 can thus be divided into two groups.
1. Finds from the sod layer overlying the gully-fill of Period 2 and from the clay deposit plus the road above the charcoal layer (fig. 102, A and D).
2. Finds from the - stratigraphically later - lower fill of the second gully and from the ditch (fig. 102, B and C). These two groups are indicated separately.

Terra sigillata

1.1 Wall-fragment of a cylindrical bowl Drag 30 (see chapter 6.2.1, p. 143 and fig. 26, no. 12), Flavian, and a base-fragment of such a bowl.
1.2 Rim-fragments of two dishes Drag 15/17 (fig. 103, 1a-b). South-Gaulish ware.
1.3 Rim-fragments of two dishes Drag 18. South-Gaulish ware.
1.4 Wall-fragment of a cup Drag 27. South-Gaulish ware.
2.1 Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 28, no. 47).
2.2 Base-fragment of an early South-Gaulish dish with a graffito X.
2.3 Two rim-fragments of cups Drag 33.
2.4 One rim- and one wall-fragment of two plates Drag 31.

Fine wares

1.1 Wall/base-fragment of a colour-coated cup decorated in barbotine, Hofheim 22 (fig. 103, 2).
1.2 Rim-fragment of a dish of orange-brown ‘fine Nijmegen’ ware, Holwerda 1944, type 37b (fig. 103, 3).
2.1 Large fragment of a wine-sieve of ‘fine Nijmegen’ ware (fig. 103, 4). A similar vessel in the same ware has been found in the legionary fort in Nijmegen. Other specimens in different wares are known from Arentsburg, Zwammerdam, and Nijmegen.22

22 See Bogaers/Haaebos 1976, 159 and Haalebos 1977, 238 and notes 12 and 13.
Gallo-Belgic ware
1.1 Fragments of one or two dishes of well-polished terra nigra with a metallic lustre, Hofheim 99.
1.2 Wall-fragment of an ovoid beaker decorated in barbotine (Perline), Hofheim 118.

Smooth ware
1.1 Rim of a flagon Stuart 106 (fig. 103, 5). A three-ribbed handled is probably from the same flagon.
1.2 Neck of a two-handled flagon Stuart 131.
1.3 Rim of a cylindrical jug with wall decorated with feather-rouletting, presumably the rare form Stuart 128 (fig. 103, 6).
1.4 Rims of mortaria with level bead-rim Stuart 149 and a wall-sided rim with narrow spout, Haltern 59/ Hofheim 79 (fig. 103, 7).
1.5 Fragment of a plate with a very low rim and a low, double foot-ring (fig. 103, 9). Traces of wear on the foot-rings indicate that it was probably not a lid.
1.6 Fragments of oil-amphorae Dressel 20 and of fish sauce-amphorae Dressel 7–11/Pélichet 46.
2.1 Rim of a mortarium with level bead-rim Stuart 149.
2.2 Pedestal of a tazza (incense cup).
2.3 Wall-fragment of a flagon with part of a graffito ]vs (fig. 103, 8).
2.4 Wall/rim-fragment of a wall-sided mortarium Haltern 59/Hofheim 79.
2.5 Wall-fragments of a wine-amphora Pélichet 47.

Coarse ware
1.1 Rim-fragments of three cooking-pots Stuart 201.
1.2 Rim-fragments of three cooking-pots Stuart 210.
1.3 Neck of a jug Stuart 214B.
2.1 Rim-fragments of two jars Stuart 213A.
2.2 Rim-fragment of a dish Hofheim 94A.

Building material
1.1 Fragments of tegulae and one fragment of opus signinum.
1.2 Fragment of tuff.
1.3 Fragment of slate.
1.4 Piece of burnt wattle-and-daub.
2.1 Two fragments of tegulae with cement.

Glass
1.1 Two fragments of blue and green glass of bottles Isings 1957, form 50.

Metal
1.1 Bronze coin, As of Nero, rev. Victoria, AD 64–68 cf. RIC 329 (fig. 104, 4).
1.2 Two bronze rings, diam. 23 and 35 mm (fig. 104, 2–3).
1.3 Iron L-shaped tumbler-lock lift key (fig. 104, 5).
1.5 Iron dagger (pugio) with point broken off, remaining total length 28 cm, length of the tang 10 cm (fig. 104, 7). The leaf-shaped blade has no prominent midrib and is decorated by four parallel grooves. Although different from the 1st-century daggers with narrow blades and a

23 The two blocks of tuff and one limestone fragment from find no. MW 1–0–23 should probably be added to this.
Fig. 104 Meinerswijk, metal objects from Periods 1 (no. 1) and 3 (nos. 2–8). Scale 1:2 (coin 1:1).

midrib, the form of the dagger is not necessarily indicative of a much later date.24

1.6 Leaf-shaped iron spearhead with missing point, remaining length 16.9 cm, with a circular-sectioned and split socket (fig. 104, 8).

Organic material

1.1 Part of a wooden post (oak).
1.2 Various oyster-shells.
1.3 Pointed end of an oak pole (fig. 105).

24 Cf. Herrmann 1969, 133. See also Abb. 3, 1–3.

Dating

The evidence from the previous phase implies that Period 3 can be dated from c. AD 70 onwards. Some material, such as the early wall-sided mortaria, the colour-coated cup Hofheim 22, and the base of an early South-Gaulish terra sigillata dish, is probably residual from earlier layers (opspit). Much of the terra sigillata (1.1–4) as well as the fine Nijmegen and Gallo-Belgic ware are evidence for a dating in the Flavian period.

The finds from the lower fill of the gully and the ditch contain, however, also material that is probably post-Flavian, notably some terra sigillata sherds (2.1 and 2.3–4). This indicates that the flooding which created the gully must have occurred in the first half of the 2nd century and probably well before AD 150. The lower gully-
fill is ascribed to Period 3 because major changes must have taken place only after it had been deposited and the youngest finds should date the end of that phase. The available building material and small particles of brick and some of stone in the various layers indicate that at least some structures must have been built in stone and that there were tile-covered roofs.

9.2.4 Period 4
Stratigraphically, the phase which constitutes the fourth period of occupation of the site can be conveniently described as all those layers and features which overlie or cut those of Period 3 and which are beneath the upper dark fill ascribed to Periods 5 and 6 (fig. 106). The attribution of finds to this phase as a whole does not cause any problems. The interpretation and sequence of the various features belonging to Period 4 is, however, rather problematical in some respects. It is clear that after the second flooding the surface of the site was raised again, and this time quite considerably, to almost 1 m above the surface of the sod layer and the road from Period 3 (fig. 106, A). A thin layer of stone and tile debris above the southern slope of the former gulley (fig. 106, B) and of some gravel above the sod

Fig. 105 Meinerswijk, pointed end of oak pole, Period 3. Scale 1:2.
Fig. 106 Meinerswijk, profile CD and plan at c. 9 m NAP, Period 4. Relevant deposits and features are shaded. A-E, see the text.
layer in the profile GC, as well as two or three different layers in the southern part of the trench (profile EF) are indications that this raising may not have been a single enterprise, and certainly not in the area beyond the southernmost ditch. A raising in phases is all the more probable because the profiles show two V-shaped ditches which must belong to Period 4: the southernmost V-shaped ditch (figs. 106, E and 94, profile EF) and a V-shaped ditch with flat bottom dug through the gravel of the former roads (fig. 106, D). Both ditches have definitely been dug from a level above those of Period 3 and definitely not from the dark upper fill but neither can be related to any specific level, despite intensive efforts during the excavation. It is quite well possible that this lack of a clear stratigraphy can be attributed to yet another flooding of the ditches, with erosion and redeposition of material. Some proof for this assumption is provided by the northern slope of the southernmost ditch (fig. 94, profile EF), which has partly disappeared. Fortunately, the southern slope is still preserved to a level high enough to conclude that it must have been dug after Period 3. This circumstance plus the fact that the fill contains no gravel to speak of indicate that the ditch cannot have been the southern road-ditch from Period 3, although it has approximately the same depth as the gravel-filled ditch north of the road. It is not clear which of the two ditches from Period 4 is the older, but the southern one reaches c. 50 cm deeper than the ditch cut through the road and is thus likely to be somewhat earlier. Both ditches have been preserved to approximately the same level at c. 9.50 m NAP. The ditch through the road is the only one which does not run straight but curves southward, where it is cut by a small north-south trench (fig. 106, C) at the same spot where both features are cut by one of the large ditches from period 5. This indicates at least that the two ditches can hardly have been contemporaneous. The precise nature of these features and, in part, also the sequence of events remain highly elusive. The finds from the various features can contribute little to solving this problem. On the whole, the stratigraphic units attributed to Period 4 differ from previous ones because they contain much more building material, such as fragments of tiles, tuff, limestone, and cement. They do not, however, provide any clear arguments for a chronologically differentiation of the various features. The finds Two groups of finds can be differentiated:

1. Finds from the (upper) fill of the gully and from the northern part of the excavation trench (the top of the layer into which the foundations of the stone walls and some refuse pits have been dug; fig. 106, A/B).
2. Finds from the southernmost ditch (fig. 106, E). The other two trenches or ditches (fig. 106, C, D) are without significant finds.

Terra sigillata
1.1 Rim-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 27, no. 45).
1.2 Wall-fragment of a bowl Drag 37 from the Argonne (see chapter 6.2.1, p. 146 and fig. 28, no. 59).
1.3 Base-fragment of a bowl Drag 37 from Trier (see chapter 6.2.1, p. 148 and fig. 30, no. 96).
1.4 Base-fragment of a bowl Drag 29 with a stamp [OF IVCVNDI, from La Graufesenque (see chapter 6.2.1, p. 155 and fig. 33, no. 21).
1.5 Rim-fragments of a dish Drag 18, a dish Drag 18/31, 3 plates Drag 31, and one plate Drag 32 (Lud Tp).
1.6 Base-fragment of a dish Drag 18/31 with a stamp SECVN[DI]N from Lezoux (see chapter 6.2.1, p. 156–7 and fig. 33, no. 42).
1.7 Rim-fragments of a cup Drag 27 and 3 cups Drag 33.
1.8 Base-fragment of a cup Hofheim 9 with a stamp [OF] MASO from La Graufesenque (see chapter 6.2.1, p. 156 and fig. 33, no. 28).
1.9 Rim-fragments of 3 bowls Drag 38, a bowl Drag 44 (form Lud Sn) and one mortarium Drag 45.
2.1 Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 28, no. 48).
2.2 Large part of a bowl Drag 37 from the Argonne (see chapter 6.2.1, p. 146 and fig. 28, no. 60) with a graffito X on the base.
2.3 Rim-fragment of a cylindrical bowl Drag 30.
2.4 Fragment of a cup Drag 33.

Fine Wares
1.1 Rim-fragment of a cup Stuart 1.
1.2 Rim-fragment of a cup Stuart 2.
1.3 Rim-fragments of plates Stuart 10.
1.4 Wall-fragment of fine Nijmegen ware.

Smooth ware
1.1 Neck of a flagon Stuart 110B (fig. 107, 1).
1.2 Rims of two-handled flagons Stuart 129B and 132B.
1.3 Rim of mortarium with level bead-rim Stuart 149.
1.4 Rims of 3 mortaria with vertical flange cf. Brunst-
Fig. 107 Meinerswijk, pottery from Period 4 deposits.

ing 1937, type 37. One of these (fig. 107, 2) with a graffi-

1.5 Wall-fragments of oil-amphora Dressel 20.
2.1 Neck of a two-handled flagon Stuart 129A (fig. 107, 3).
2.2 Wall-fragment of a wine-amphora Pélichet 47.

Coarse ware
1.1 Complete face-urn with frilled rim and three ornamental (unpierced) applied spouts (fig. 107, 4), cf. Niederbieber 80.
1.2 Rims of 3 plates Niederbieber 112B, one fragment (fig. 107, 5) with traces of cement attached.
1.3 Rims of one vessel each of the following forms: Stuart 201, 215, and 217; Niederbieber 89, 104, and 111; and the neck of a jug Niederbieber 100.
2.1 Rims of two cooking pots, Stuart 201 and Niederbieber 89.

Building material
1.1 Numerous fragments of tegulae, imbrices, and lateres; one fragment with the circular stamp with two ‘tails’ (fig. 108) EX GER INF: CIL XIII/6, p. 124–5, type 19 + 14 = Holwerda/Braat 1946, Pl. 29, 30.
1.2 Fragments of wall-painting (white and pink with red dots) and cement.
1.3 Fragment of opus signinum.
2.1 Fragments of slate, grauwacke, and quartz.
2.2 Fragments of cement.

Glass
1.1 Fragment of window-glass.
1.2 Base-fragment of a bottle with a pontil-mark.

Fig. 108 Meinerswijk, tile-stamp EX GER INF from Period 4 deposit. Scale 1:2.

Metal
1.1 Bronze coin, As of Domitian, AD 86, cf. RIC 335 (fig. 109, 1).
1.2 Leaf-shaped iron spearhead. Most of the socket is missing, remaining length 9.4 cm (fig. 109, 2).
1.3 Curved iron pick (fig. 109, 4) rear end of a pioneer axe as no. 1.4.
1.4 Large and well-preserved iron pioneer axe (dolabra) with broad blade and relatively small curved pick, length 36.3 cm, weight 1.228 kg (fig. 109, 5). Compare e.g. Ulbert 1970, no. 287.
1.5 Fragment of bronze binding from an oval wooden shield (fig. 109, 3). The binding has a U-shaped section and on one end still has part of the roughly semicircular lugs with a hole for a rivet surviving.

Organic material
1.1 Fragment of corroded hobnails with some leather; part of a sole.

Dating
From the lower (natural) fill of the gully, ascribed to Per-

183 (343)
raising of the site and the abandonment or at least relocation of the road must have started some time before AD 150. Apart from some clearly residual finds from earlier layers, the material covers the entire 2nd and the early-3rd centuries. The military tile-stamp of *exercitus Germanicus inferior*, produced at site 433 (De Holdeurn), indicates the Period 4 lasts at least till after AD 175. Late terra sigillata forms such as Drag 32 and especially Drag 45 point in the same direction, as does a fragment of the coarse-ware dish Niederbieber 112B which must have landed in the opus caementicum of some wall which was demolished again in time for the fragment to end up in a Period 4 layer. On the other hand, for Period 4 to have lasted far into the 3rd century the number of late coarse-ware pots Niederbieber 89 is rather low and the absence of sigillata from Rheinzabern may also be significant. The total quantities of datable material are, however, too small to draw any firm conclusions. The end of Period 4 should probably be dated to around AD 200.

9.2.5 Period 5

The main features of Period 5 are two or perhaps three large parallel ditches with a very dark and polluted fill and merging without noticeable transitions into the dark upper layer covering the entire southern part of the excavation trench (fig. 94). In its turn, this layer is indistinguishable from the upper layer of dark soil which covers the entire site (fig. 110, A). It is only 10 cm thick, occasionally at the most c. 20 cm, and cannot be identified as a ploughzone. This could be verified in the northern part of the excavated area where the foundation trenches (robber trenches) of the stone walls became clearly visible immediately after the sods were removed! It is not impossible, and for reasons to be discussed below and in the next paragraph even likely, that the surface at the highest point of the site, where the excavation trench is situated, has to some degree been levelled in the past. Definite proof is, however, lacking. This state of affairs implies that there is no definite relation between the stone walls and any of the features in the southern part of the trench. In theory, the walls could belong to the latest phase of Period 4 but, as suggested by profile AD (fig. 94), a relation to the features of Period 5 (or 6?) is more likely: in any case the founda-

tion trenches were dug into the layer that was raised during Period 4.

It is possible that there are actually two phases in the building activities. As mentioned at the beginning of this chapter, the foundation trench of the southernmost wall covers only the western part of the trench but the robber trench of the same feature at the first plane (immediately under the surface) runs straight through. In addition, the foundation trench shows a slight but nevertheless clear difference in orientation from the other foundations (and, for that matter, from virtually all other features from all periods) while the robber trench has the same direction. It is possible, as mentioned before, that during the removal of stone from the foundations a robber trench continued where no foundation existed. But it is more likely that there was indeed a foundation. This implies two things. First, the original surface of the site was somewhat higher than it is today. Second, that an opening was closed perhaps in a more or less provisional way.

These findings may correspond with the results of the excavation in the southern part of the trench. Apart from the two large ditches (fig. 110, B and E) there is a depression in profile EF (fig. 94) with a lower fill that has a somewhat lighter colour than the normal ‘Period 5’ fill. It has a flat bottom, c. 50 cm wide. This feature extends only c. 1 m into the excavation trench (fig. 110, C) where it is cut by a pit dug from the Period 5 level (fig. 110, D). Although the black layers in the profile did not permit a stratigraphical differentiation between this and other features, the less-polluted lower fill and the stratigraphically later pit show that it must be a relatively early phenomenon in Period 5. It is not impossible that this feature is some elongated refuse pit, but it may in fact be the end of a flat-bottomed ditch. In that case, the opening corresponds well with the opening in the southernmost wall. 25

The two large and probably contemporaneous ditches may thus have been dug after the interrupted ditch was filled in again, and perhaps at the same time as the wall was closed. If this southern wall is the wall around the fort, the distance between it and the northern side of the inner ditch is no more than 2.5 m, which would seem to be a very narrow space and, moreover, the wall and the ditch do not appear to run parallel. It is, therefore, also

25 Note that the small trench which seems to run through this opening (fig. 106, C) definitely does not belong to the same phase!
conceivable that the wall was demolished and replaced by a more northern wall, in which case the building in the northern part of the trench could be a gate or, less probably, a tower.

Whatever the true interpretation of the features and the real sequence of events may prove to be, it is obvious that the available data are too limited to allow a completely satisfactory interpretation. It is clear, however, that the two large ditches belong to a fort which was probably built entirely in stone. The entire dark upper fill of the excavation trench, including the ditches, contains large amounts of brick and various kinds of stones.

The origin of this dark layer can probably again be attributed to natural causes. In section, the ditches are more or less U- or bowl-shaped. Only the section through the southern ditch (and, perhaps, the interrupted ditch) in profile EF (fig. 94) still show a more or less V-shaped profile with a flat bottom. For the southern ditch it could be observed that the 50-cm-wide base of the ditch was in fact a sort of cleaning channel, lined with sods. Such a construction has also been observed in the ditches around the fort in Zwammerdam and elsewhere. The origin of the dark layer as well as the final shape of the ditches were probably caused by (repeated?) flooding, and the width of especially the northern (inner) ditch, some 6 m, may well far exceed its original width. If it is assumed that the surface during Period 5 is more or less the same as the present-day surface of the site, the northern ditch (fig. 110, B) was 1.70 m deep, and the southern (E) and the interrupted (C) ditch 2 m deep.

The Finds
In the discussion of the finds, five groups are differentiated. The foundation trenches are without significant finds, but there is some material from the various pits. These are included here, although there is, of course, no guarantee that they are indeed all features from Period 5. The following groups are concerned:
1. Finds from pits (fig. 110, D and on the first excavation plane, fig. 94).
2. Finds from the dark layer in the southern part of the excavation trench (fig. 94, profile EF).
3. Finds from the northern ditch (fig. 110, B).
4. Finds from the southern ditch (fig. 110, E).
5. Finds from the possible, interrupted, ditch (fig. 110, C).

26 Haalebos 1977, 34 and note 51.
Terra sigillata
2.1 Rim-fragment of a bowl Drag 37 from Blickweiler/Eschweilerhof (see chapter 6.2.1, p. 145 and fig. 27, no. 38).
2.2 Fragments of three dishes Drag 18/31.
3.1 Fragments of three bowls Drag 37 from Trier (chapter 6.2.1, p. 148 and fig. 30, nos. 95, 97, and 98).
3.2 Fragments of three plates Drag 31 and five fragments of mortaria Drag 45.
4.1 Fragments of bowls Drag 37 from Trier (see chapter 6.2.1, p. 148 and fig. 30, nos. 92 and 93) and Rheinzabern (see chapter 6.2.1, p. 151 and figs. 31, no. 125 and 32, nos. 126–129).
4.2 Fragments of two South-Gaulish cups: Drag 24/25 and Drag 27 with a graffito XXI[.]
4.3 Fragments of one cup Drag 27, one Drag 40, one Drag 46 (Lud Bb), and seven specimens of Drag 33; five plates Drag 31, one bowl Drag 36, one Drag 38, and one Niederbieber 11B; three mortaria Drag 45.
5.1 Fragment of a bowl Drag 37 from Trier (see chapter 6.2.1, p. 148 and fig. 30, no. 94).
5.2 Fragments of a plate Drag 31 and a cup Drag 33.

Fine wares
1.1 Rim of a colour-coated beaker Niederbieber 32c.
3.1 Fragments of three beakers Niederbieber 32c and one beaker Stuart 2.
4.1 Rim of a colour-coated plate Stuart 10.
4.2 Fragments of two beakers Niederbieber 32c and one Niederbieber 32d.

Gallo-Belgic ware
3.1 Rim of a terra nigra plate Hofheim 99.

Smooth ware
2.1 Part of a lid.
2.2 Rim of a mortarium with horizontal flange Stuart 149.
2.3 Fragments of oil-amphora(e) Dressel 20.
4.1 Fragments of a flagon with brown 'marbled' colour-coating.
4.2 Fragments of two mortaria, one with horizontal and one with vertical flange.
4.3 Fragments of oil-amphora(e) Dressel 20 and the handle of a wine-amphora Pélichet 47.
5.1 Fragments of a mortarium with horizontal flange.

Coarse ware
1.1 Rim of a cooking-pot Niederbieber 89.
2.1 Fragments of two bowls Stuart 210 and one cooking-pot Niederbieber 89.
3.1 Fragments of one bowl with inturned thickened rim Niederbieber 104, two plates Niederbieber 111, and nine cooking-pots Niederbieber 89.
4.1 Fragments of two bowls Stuart 210, one c. Gose 1950, no. 505, and one Niederbieber 104; one cooking-pot Stuart 201 and seven Niederbieber 89; three plates Niederbieber 112b and one Niederbieber 111.
4.2 Neck of a flagon Niederbieber 98.
4.3 Wall-fragment of a face-urn.
5.1 Rim of a cooking-pot Stuart 201.

Building material
Brick, cement, and stone fragments occur in all features from Period 5. The following items have special relevance:
A Brick27
1.1 From the refuse pit cutting the possible (interrupted) ditch: fragment of a tegula with one half of a round stamp (fig. 111, 1), diam. 5.1 cm, with traces of positive letter imprints: [ ] F, possibly VEX ER GER F.
2.1 Fragment of a tegula with a round stamp (fig. 111, 2), diam. 5.3 cm, with traces of positive letter imprints, presumably VEX ER GER (INF).
2.2 Stray find, probably from the topsoil: fragment of a tegula with a square stamp (fig. 111, 3) EXGERINF: CIL XIII/6, p. 124, type 32 + γ = Holwerda/Braat 1946, Pl. 28, 28. The stamp is known from many frontier fronts.28
2.3 Stray find, probably from the top soil: fragment of a tegula with an illegible round stamp with traces of positive letter imprints (fig. 111, 4), probably VEX ER GER (INF).
4.1 Fragment of a tegula with a rectangular stamp (fig. 111, 5) EXGERINF. Compare Holwerda/Braat 1946, Pl. 28, 7.
5.1 Fragment of a tegula with a rectangular stamp (fig. 111, 6) EXGERINF. Compare Haalebos 1977, Taf 28, 48 and Holwerda/Braat 1946, Pl. 28, 25.
5.2 Fragment of a tegula with a rectangular stamp (fig. 111, 7) EXGERINF: CIL XIII/6, p. 124–5, type 19 + γ. Compare Haalebos 1977, Taf. 27, 41.
5.3 Fragment of a tegula with part of an illegible, rectangular stamp (fig. 111, 8) with traces of negative letter imprints, presumably EXGERINF.

27 I thank J.E. Bogaers (Nijmegen) for his help in the reading of several tile-stamps.
28 For references, see Haalebos 1977, 184, nos. 60–61.
5.4 Fragment of a tegula with part of a curious rectangular stamp with partially negative, partially positive letter imprints (fig. 111,9) EX[GER. INF]: CILI[III,6, p. 125, type 25 + γ = Holwerda/Braat 1946, Pl. 30, 62.

5.5 Fragment of a tegula with a half-round satellite-stamp (fig. 111, 10) belonging to a broken-off round EX GERINF stamp: CILI[XIII,6, p. 125-5, type 19 + k2 = Holwerda/Braat 1946, Pl. 29, 33. Also known from Zwammerdam, Maurik, Neuss (Haalebos 1977, 184 and Taf. 29, 64), and Utrecht (Van Giffen a.o. 1934-38, 14, fig. 3, 83f).

5.6 Fragment of an imbrex with a rectangular stamp with negative letter imprints (fig. 111, 11) EX[GER]INF in unknown frame. Compare Holwerda/Braat 1946, Pl. 28, 4).

5.7 Fragment of an imbrex with a rectangular retrograde stamp (fig. 111, 12) LEG I M ANT: Holwerda/Braat 1946, Pl. 33, 2. The name Antoniniana is characteristic for the reigns of Caracalla and Elagabalus.29

B Stone

4.1 Tuff block with an inscription LEGIMPF: LEG(io) I M(inervia) P(ia) F(idelis). The front measures 57 x 14 cm and the block is 26 cm wide (fig. 111a). A large part of the rear side and a smaller part of the front are broken off, thus damaging the I of the inscription. The block is probably a building inscription from the stone fort of Period 5, although such inscriptions do not normally seem to have been cut in tuff. It is most likely that the inscription indicates building activities of a vexillatio of the legio I in Meinerswijk. In view of the dating of Period 5 and the absence of an A at the end of the inscription, these activities – and thus the inscription – are likely to predate AD 211.

Somewhat comparable to the inscription from Meinerswijk is another building inscription from the Netherlands (but from an unknown findspot), LEG: I: M: P: F: ET / LEG: XXX: V: V:, read as: LEG(io) I M(inervia) P(ia) F(idelis) ET LEG(io) XXX V(lpia) V(ictrix).30 The inscription is, however, put in a tabula ansata frame and cut in a block of limestone instead of tuff.

Glass

2.1 Three fragments of green glass from a square bottle Isings 50.

29 Stein 1932, 181.

30 Cf. CIL XIII, 8832 = ER II, 409.
2.2 Fragment of bluish-green glass from a bottle Isings 50 or 51.
4.1 Wall-fragment of blue glass from a bottle Isings 50.
4.2 Shoulder-fragment of bluish-green glass from a bottle Isings 51.

Metal
1.1 Fragment of a leaf-shaped iron spearhead. The point and most of the socket are missing (fig. 112, 1).
1.2 Three fragments of what appears to have been an approximately life-sized bronze statue, deliberately chopped into very small pieces, presumably for recycling the metal. The fragments were found in the large refuse pit in the stone building (fig. 94). At the first excavation level this pit showed clear traces of fire, as did the surface in the northwestern corner of the excavation trench (west of the building on the level drawn in fig. 94). It is possible, therefore, that the smelting-activities post-date or predate Period 5 and do not belong to this phase. If the traces of burning inside the building and outside are indeed contemporaneous, the bronze fragments should actually belong to the last phase of Period 4. Of the fragments themselves, one fragment is identifiable as a toe, probably the small toe of the right foot (fig. 112, 2). It is hollow cast à cire perdue. Compare, e.g., Boube-Piccot 1969, Pl. 89.
3.1 Fragment of an iron dagger with a straight-sided blade and without a prominent midrib (fig. 112, 3).

Organic material
3.1 Two fragments of leather soles studded with badly corroded hobnails.

Dating
Most of the pottery from Period 5 belongs typochrono-
logically to the Niederbieber horizon and dates either from the late-2nd century or approximately the first half of the 3rd. The stamps of the *exercitus Germanicus inferior* indicate considerable building activities after AD 175, although at least one specimen (fig. 108) arrived in Meinerswijk early enough to land in a Period 4 deposit. It is possible that the stamp from the *legio I Minervia Antoniniana* dates these activities more precisely to between AD 211 and 222. However, the possibility of renewed construction work during Period 5 as discussed above and the absence of the title *Antoniniana* on the building inscription from the southern ditch both suggest that the LEGIMANT stamp may also date this rebuilding or repair work.

In any case it is clear that Period 5 can be dated approximately to the first half of the 3rd century. Arguments for a more exact dating are lacking. Precisely datable late 3rd-century material, such as the latest terra sigillata products from Trier or Rheinzabern, is not available but its absence among the finds from the small excavation trench is hardly significant.

9.2.6 Period 6

As indicated on fig. 113, there is one feature which definitely post-dates Period 5. Directly above the southern slope of the inner (northern) ditch of that phase is a foundation trench filled with secondarily used blocks of stone - mostly tuff - only occasionally bonded by cement that may have been poured over the stones after their deposition in the trench. Most of the blocks cemented together in an orderly fashion appear to have been used while already in that state. They undoubtedly originally belonged to the fort of Period 5. The trench itself was more or less clearly visible only at the first excavation plane, directly under the grass cover: it could not be observed clearly in the profiles.

Although it appears to be a somewhat ramshackle foundation, the construction was not built without care. The lowest part consists of large and heavy blocks, such as a cornice (fig. 116), the smaller blocks piled on top of them. Immediately south of the foundation two sizeable posts - presumably part of a row - have been observed, with a diameter of 30 cm and reaching to 75 cm below the surface of the site (indicated in black).

There are no definite clues as to the interpretation of this foundation. It evidently indicates some sort of wall, perhaps supported by sturdy posts, with a maximum thickness of 1.40 m and built with the remains of previous stone buildings. It may have been built to protect the highest part of the site against the water but it could just
as well have been a defensive wall or have had some altogether different purpose. At the site of the excavation there are no levels preserved which correspond stratigraphically to this construction. As mentioned in the previous paragraph, it is likely that some soil was removed in the past, leaving only a thin upper layer of some 10 cm with material that could belong to Period 6. Confirmation for this assumption is also provided by some finds from the cores of borings by the RGD (Netherlands Geological Survey) at the eastern border of the site (a.o. the C14-boring Meinerswijk II: see chapter 3, fig. 18). The deepest part of the core contains Roman finds, but above those are some late-Roman or Merovingian sherds and in the highest part even later medieval 12th-century finds. It thus seems very likely that post-Period 5 layers are still preserved around the highest part of the site, as is also indicated by the geological profile AB on fig. 18, which indicates that from the 7th century onwards these even partly covered the fossil channel delimiting the eastern and southern perimeter of the site.

The Finds
The finds from Period 6 can be divided into three groups of which only the first can provide direct dating evidence:
1. Finds from the foundation trench.
2. Finds from the topsoil north of the foundation.
3. Finds from the topsoil south of the foundation.
Because a sizeable portion of the material is evidently residual from earlier layers, these have been marked with an *.

Terra sigillata
1.1* Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 27, no. 43).
2.1* Wall-fragment of a bowl Drag 37 from La Madeleine (see chapter 6.2.1, p. 145 and fig. 27, no. 46).
2.2 Wall-fragment of a bowl Chenet 320 from the Argonne (see chapter 6.2.1, p. 153 and fig. 32, no. 139) and a base-fragment, probably from a late-Roman bowl or plate.
2.3* Fragment of a plate Ludowici Sb with a stamp Q[v]IITVZ3 (see chapter 6.2.1, p. 156 and fig. 33, no. 40).

2.4* Fragments of three cups Drag 27, two Drag 33, and two dishes Drag 18.
3.1 Base of a mortarium Drag 45 (possibly Alzei 3).

Colour-coated wares
2.1* Rim-fragment of a beaker Niederbieber 30.
2.2 Three-ribbed handle of a flagon or jug in an almost red-brown slip ware or conceivably with a brown 'marbled' colour-coating.

Gallo-Belgic ware
2.1* Rim-fragments of a type I jar, a type 2 cooking-pot (see chapter 6.2.3) in a terra nigra-like ware (fig. 114, 1-2).
3.1* Rim-fragments of a cooking-pot with inturned rim ('cork-urn' form) in a terra nigra-like ware (fig. 114, 3) and fragments of one or two ovoid beakers decorated in barbotine (Perlurnen), Hofheim 118.

Smooth ware
2.2* Rims of a mortarium with vertical flange and of a large dolium.
2.3* Fragment of an oil-amphora Dressel 20.
3.1* Neck of a two-handled flagon Stuart 129B.
3.2* Three rims of mortaria with horizontal flange.

Coarse ware
1.1 Large fragment of a cooking-pot with crescent-shaped rim in very coarse brown-violet Mayen ware, Alzei 27 (fig. 114, 4). This find is of special importance because it was found inside the foundation and dates its construction.
2.1 Fragments (a.o. three rims: fig. 114, 5-7) of at least three cooking-pots Alzei 27 in Mayen ware.
2.2 Rim-fragment of a deep dish with thickened rim Alzei 29 (fig. 114, 8).
2.3* Rim-fragments of two cooking-pots, possibly to be identified as late examples of Niederbieber 89 (fig. 114, 9-10); four specimens Niederbieber 89 and four Stuart 201; two bowls Stuart 210.
2.4 Rim-fragments of three cooking-pots with thickened, cordonned rim (type 3: chapter 6.2.5) Trier-Umbaukeramik type 43 (fig. 114, 11-13).

31 The material was provided by J. de Jong, RGD Haarlem. These finds do, incidentally, confirm the C14-datings of 1500 ± 35 and 1350 ± 30 BP.

32 On the dubious chronological value of rims of this type, see chapter 6.2.5 and also Pferdehirt 1976, 118-20.
33 Hussong/Cüppers 1972.
2.5 Rim-fragments of three steep-walled Merovingian cooking-pots (fig. 114, 14–16) and a similar fragment decorated by wheel-rouletting (fig. 114, 17) which presumably dates to about the 8th century.

3.1 Fragment of a vessel of very coarse brown-violet Mayen ware.

3.2* Rim-fragment of a bowl Niederbieber 104.

3.3 Handle of an early stoneware jug, late 13th century.

3.4 Wall-fragment of glazed stoneware, 16th century.

Building material

1.1* Fragment of a tegula with part of a round stamp EXG[ERIN]F and a satellite-stamp in the form of a stylized hoof-imprint (fig. 115): CIL XIII/6, p. 124–5, type 21 + 2 = Holwerda/Braat 1946, Pl. 29, 29 and Haalebos 1977, Taf. 28, 62 and p. 184, 290. The stamp has been recorded at many sites along the limes of Lower Germany: Valkenburg, Roomburg, Zwammerdam, Vleuten–De Meern, Vechten, Maurik, Xanten, Gellep, and Neuss.

1.2 Numerous – and virtually all tuff – blocks of various shapes. A well preserved large block from the base of the foundation is illustrated in fig. 116. It is an elaborately moulded cornice, length 111 cm, maximum width 80 cm, and 24.5 cm thick. On the upper side a 12-cm-deep mortise for a dowel (dookgat) is preserved. Cornices with similar profiles (cyma recta moulding) are known from the temples in Elst (site 105). The cornice may have been used in the Period 5 fort. Apart from the cornice, and a few fragments of similar stones, some wedge-shaped stones should also be mentioned here. Originally, these have been part of an arch.

Dating

The only find directly suitable for dating purposes is the large fragment of a cooking-pot Alzei 27 from the foundation. Such pottery is characteristic for the Late-Roman Period and especially for the second half of the 4th and the early-5th centuries. It can thus be assumed that the activities of Period 6, the demolition of some stone walls or perhaps more likely the collection of building materials from structures already in ruins and the construction of a new wall from them, took place in that same period.

In view of the small excavated area and the probability that some soil was removed in the past, a considerable number of late-Roman finds especially from the area north of the foundation, testifies to occupation during the Late-Roman Period. It is not inconceivable that parts of some buildings were still standing and could be used after some repair work; this would explain the clear concentration of late-Roman material north of the foundation.

The presence of Merovingian and later finds at the site of the excavation and in borings is evidence for either continued, renewed or intermittent occupation of the


35 Bogaers 1955, Pl. 40. For an identical profile, see the corbel stone from Hesselbach, Baatz 1973, Taf. 21, 1b.
site until at least the 12th-13th centuries and possibly even later. If Period 6 is dated to IVB-Va, as suggested by the available finds, it is clear that future research will undoubtedly produce evidence for several additional early- and late-medieval phases.

Stray Finds
There are a few stray finds (fig. 117) from the excavation as well as the site as a whole which deserve to be mentioned. They are the following items:
1 Rim-fragment of what seems to be a moulded pipe clay bowl with a pierced wall.
2-3 Two rim-fragments of steep-walled Merovingian cooking-pots.
9.3 CONCLUSIONS

Periodization

The excavation and other research in Meinerswijk have shown that the site was inhabited from at least the first decade AD into the Early- and possibly even Later-Middle Ages. Because its presence is primarily demonstrated by the evidence from borings and only very marginally by the data from the excavation trench, the post-Roman occupation phase(s) cannot be discussed in detail. In contrast, the site of the trench proved to be exceptionally favourable for our purpose: to gain an insight in the nature and chronology of the Roman Period occupation. Although the excavated area is too small to take anything for granted, the stratigraphy and dating evidence are such that most if not all of the successive phases of the Roman habitation are represented. Only the earliest occupation, from the last decades BC, is missing which is all the more intriguing because the excavation did not reach virgin soil. On the other hand, there are no indications to conclude that such an early phase must have been present.

On the basis of the evidence discussed in the previous paragraph, the following phases can be proposed:

Period 1: 2nd decade AD
Period 2: pre-Flavian – AD 69
Period 3: AD 70–IIa
Period 4: IIa–IIId
Period 5: IIId–IIIic
Period 6: IVc–Va

The available data do not allow a more precise periodization. Although it contains a few chronologically fairly secure dates, there are also some important uncertainties.

Period 1 can only be dated in the 2nd decade AD. The evidence is, however, wholly inadequate to fix the transition from Period 1 to Period 2 in time. If there was such a transition it should be under Tiberius, but for all we know there may well be a hiatus of two to three decades and the start of Period 2 may be a reoccupation of the site under Caligula or Claudius. The change from Period 2 to Period 3 is well founded, and for the following phases the time of transition can at least be narrowed down to periods of some 25 years. The hiatus between Periods 5 and 6 is quite probable, but its length cannot be determined precisely.

The end of Period 5 cannot be much earlier than AD 250 but it could be considerably later, especially when the (related) problems of general scarcity and identification of late-3rd century material are taken into consideration. The beginning of Period 6 depends on the construction of only one structure that accidentally survived in the small excavated area and, although supported by the distribution of other finds, that structure is dated by only one sherd. Fortunately, the association between both is absolutely certain and the rim-sherd itself can be confidently dated to the late-4th century, but in itself that is not a very precise dating.

Although not necessarily relevant to the present discussion it is worth while to note that the periodization of Meinerswijk might be broadly similar to that established for the successive forts of Valkenburg: Meinerswijk 2 \(\approx\) Valkenburg 1–3 (AD 39/40–69), Meinerswijk 3 \(\approx\) Valkenburg 4 (c AD 70–100), Meinerswijk 4 \(\approx\) Valkenburg 5 (c AD 100–174), Meinerswijk 5 \(\approx\) Valkenburg 6 (c AD 178–240), and Meinerswijk 6 \(\approx\) Valkenburg 7 (c. IVbc).

Interpretation and historical context

Finds and features from the excavation leave little doubt that we are dealing with a military establishment. Although its layout and even extent in all of the successive phases are still unknown, the available data, in combination with such information as the geological context and the location in relation to rivers and surrounding sites, as well as the general historical background, do allow a number of conclusions.

As mentioned in the preliminary report, the dating of the first occupation phase almost inevitably leads to the
assumption that Meinerswijk must have been one of the camps used by Germanicus when he assembled his troops in the insula Batavorum in AD 16.\textsuperscript{39} If the interpretation of the graffito (fig. 98, top) is correct, the camp in Meinerswijk may have been used by legionary soldiers of the legio V Alaudae which was stationed in Xanten at that time. The geological situation around the site as discussed in chapter 3 would have made it very suitable as an assembly point for ships, no fewer than a thousand of which are reported to have been used in the campaign.\textsuperscript{40}

The presence of an early camp in Meinerswijk has also been used (chapter 3) as an additional piece of circumstantial evidence to argue for the IJssel as a navigable stream in the Roman Period and which may well be identified with the Fossa Drusiana (cf. fig. 19). The evidently important strategic position of Meinerswijk at the point where the Rhine turns westwards would be even more significant if the identification is indeed correct. By itself this important position, confirmed by the later use of the site as a frontier fort, suggests that continued occupation after Germanicus’ campaigns is perhaps more likely than immediate abandonment and reoccupation sometime around AD 40.

However this may be, it is clear that Meinerswijk must have been incorporated as some sort of frontier fort in the limes of Germania Inferior from the construction of the frontier under Claudius until its end in the late-3rd century. The details of the site’s development and occupants during that time will need to be examined by future excavations. There are only a few rather general observations to be made on the basis of the material now available.

For Periods 1 or 2 there is clear evidence that the natural resources in the area north of the Rhine, the Veluwe, were exploited. At least there is clear evidence for the harvesting of heather and bracken or the cutting of heather sods that can only have come from that area. The first restricted but unmistakable evidence for stone buildings occurs only in the Flavian Period 3 which is, however, quite early compared to other frontier forts and may well be a further indication for the importance of the site. If the gladius ‘from the Rhine near Oosterbeek’ indeed originates from Meinerswijk, as was proposed above,\textsuperscript{41} it could provide an indication for the presence of a legionary detachment in Meinerswijk. But this remains a mere possibility.

Evidence for a complete reconstruction in stone is only available for the 3rd century Period 5 and, as indicated by an inscription and a tile-stamp, the work was probably undertaken by soldiers of the 1st legion from Bonn. Building material from this fort was reused in the late-4th century for the construction of a wall (Period 6). Pottery finds, especially to the north of this structure, provide clear evidence of occupation into the 5th century. Strictly speaking, the small excavation has not produced definite evidence that late-Roman occupation was of a military nature. But although regrettably little is known as yet about the structure and extent of the late-Roman occupation it is clear that the alternative, a non-military occupation involving some substantial work and labour-investment in a former frontier fort on the Rhine in the late-4th century, is hardly if at all plausible.

A military occupation, however, would fit well into the picture of late-Roman activities along the Rhine as presented by Ammianus Marcellinus and, more important, is supported by a steadily increasing amount of archaeological evidence.\textsuperscript{42} On the basis of the Tabula Peutingeriana it should be concluded that Castra Herculis, the first of the seven ‘towns’ which were refortified by Julian in AD 359,\textsuperscript{43} is located in the area covered by the present study. More specifically, it should be located on the Rhine downstream from Quadriburgium which can presumably be identified as Qualburg (site 460) and its position can be even more precisely calculated if the system of roads and places as presented in fig. 21 is more or less correct:\textsuperscript{44} some 17.5 km north of Noviomagus–Nimege gen\textsuperscript{46} and some 29 km east of Carvo-Kesteren (?).

These considerations at least suggest that the identification of Meinerswijk with Castra Herculis is quite probable. Castra Herculis should be located at least approximately at the point where the Rhine turns westwards but

\textsuperscript{39} Stein 1932, 92.
\textsuperscript{40} Tacitus, Ann. II, 6.
\textsuperscript{41} See p. 301 and note 264.
\textsuperscript{42} See also chapter 12.
\textsuperscript{43} Ammianus Marcellinus XVIII 2, 3–6.
\textsuperscript{44} See also the remarks in paragraphs 3.4.1 and 3.4.3 on the limitations of the Tabula as an independent source.
\textsuperscript{45} Note that fig. 21, due to an unfortunate printing error, is not correct for the distance between Noviomagus and Castra Herculis. As is evident from fig. 20, this should have been VIII (leugae).
\textsuperscript{46} See also chapter 8, p. 250.
its location at another site cannot yet be ruled out completely. From the known sites only nos. 123 in Driel and 135 in Huissen could provide an — archaeologically less feasible 47 — alternative.

There are no plausible arguments to reason Castra Herculis away from the Rhine, for example, by translating Rhenus as Waal instead of Rhine. 48 Although perhaps possible in theory, such a move is not only contrary to the consistent information of various classical sources if taken simply at face value but also unlikely in view of the archaeological evidence for late-Roman activities along the Rhine. Moreover, Ammianus in his enumeration does not include Noviomagus. As already noted by Haalebos, 49 if Castra Herculis were located on the Waal to the west of Nijmegen, that is very odd indeed! The solution to Haalebos’ problem is, of course, very simple: whatever did or did not occur in Noviomagus was of no concern to Ammianus in the quoted paragraph because it was not related to his subject matter there, the enumeration of accomplishments of Julian along the Rhenus = Rhine.

What happened to Meinerswijk–Castra Herculis after it was finally abandoned as as Roman military stronghold is largely unknown. 50 It is certain only that the site was inhabited and even expanded in the Early-Middle Ages. It presumably belonged to the royal fisc, and Verkerk, in his historical reconstruction of Arnhem, 51 assumes that Meinerswijk may be the fort mentioned in the charter of 9 June, 726, in which Carl Martel donates the villa Eliste (Elst), situated in the locus (area) Marithaime ubi castrum fuit, where a fort has been. As mentioned in chapter 8.6.1, Eliste or Heliste and other property had been given to a certain Everhard by Childebert and was confiscated again for treason. It is uncertain but apparently generally accepted by historians that this Everhard belonged to the family of the Meginhardi. 52 As already noted by Heidinga, the early-8th-century territory of Everhard apparently encompassed the vicus of Meginhard, which makes a family relationship plausible. This place, which is known as Meginhardiswich in AD 814 and vicus Meginhardi in AD 847, 53 was probably an emporium raided by Vikings in the latter year. It is undoubtedly the same as Meinerswijk.

The post-Roman layers and the late- or post-Merovingian expansion of the site (cf. fig. 18) thus make it very likely that it is, or that it is part of, the early-medieval emporium. Although the how and why of the 8th-century and later developments are beyond the scope of the present study, it is perhaps useful to note two factors which may have been important in that respect. First, the medieval site may have grown up around Castra Herculis especially because it was still a stronghold in late-Roman times and it may have been rather more than a mass of ruins or partially eroded by the river. Second, this early-medieval development would not have occurred if the position of Meinerswijk had not still been favourable in relation to land and water (trade) routes. The later development of Arnhem at the other site of the Rhine, as pictured by Verkerk, 54 was the result of a number of factors, but from a macro- or supra-regional perspective the location of Arnhem and Meinerswijk are not fundamentally different.

48 Bogaers 1968, 157. See also chapter 12.
49 Haalebos 1976, 205.
50 The site is presumably the same as the Coadulfaveris (see fig. 140) mentioned by the Anonymus Ravennas (IV, 24). That Coadulfaveris = Castra Herculis is now generally accepted, see Bogaers 1968, 152 and note 18, Ewig 1980, 11. The Ravenna geographer probably wrote his Cosmographia in VIIIa (Cf. chapter 2, 27; Bogaers 1968, 152, note 15), but VIIc is also maintained by some (e.g., Ewig 1980, 9). See also chapter 12, note 91.
51 Verkerk 1983, 6.
53 Sloet 1872–6 (OBGZ), nos. 27 and 39 (Ann. Xantenses). Meginhardiswich is, by the way, the oldest –wik-name in the Netherlands and Westphalia (Schütte 1976, 197).
54 Verkerk 1983.
10  THE RIVER AREA: THE NATIVE BACKGROUND

10.1 INTRODUCTION

As indicated in its title, this study is concerned with Romans and Batavians or, in more general terms, with allochthones and autochthones and the interaction between them in a specific area and during a specific stretch of time. The present chapter is intended to set the stage for this: to examine the conditions in the area before the point when the process of interaction started. In chapter 2, this point was defined as the year 12 BC, in which large numbers of Roman troops arrived in the river area. As already mentioned there, one may question the adequacy of this choice on the grounds that more indirect Roman influence had already caused significant changes.

This is a matter related to the question of who precisely were the autochthones and the content of the term Batavians. We cannot ignore the explicit historical evidence for a new group of people settling in the river area, but it is by no means coherent and is certainly difficult to interpret. To reach a conclusion about this problem with the help of the archaeological data is important. But it is only part of the larger problem of how to interpret the available knowledge to offer some reasonable propositions about social and economic developments during the last centuries BC and conditions in the region on the eve of the events starting with the arrival of Drusus’ troops.

In order to do this, a number of steps have to be taken. It is necessary first to examine the historical information and notably from an anthropological perspective. This can be done with the help of similar studies on various aspects of Celtic and Germanic societies, and on their interface. These provide material from which a model of conditions in the river area can be deduced. At the same time such studies, as far as they are also based on archaeological data, can be used to gain insight into factual conditions at a supra-regional level. As such, they are also helpful for a theoretical approach, when one attempts to formulate some ideas about the general processes which led to the events of 12 BC and later. But this is a subject which merits a separate discussion. In this chapter the first step must be to interpret the (supra-regional) situation up to and including the period of the Gallic wars, and the second to evaluate as far as possible the resulting developments in the next phase, between 51 and 12 BC, with direct reference to the river area itself.

10.2 THE SOUTHERN LOW COUNTRIES UNTIL THE END OF THE GALIC WAR

10.2.1 Historical Information

In recent archaeological literature a wealth of evidence has been collected showing increasing socio-cultural complexity during the last centuries BC in areas on the northern periphery of the Roman republic, notably in central and eastern Gaul. One aspect of this phenomenon is that a flow of information must have existed, the consequences of which are especially visible in its northern direction but which were, of course, reciprocal. Information about Celtic societies thus reached the Mediterranean where some of it was recorded and of these records scraps and pieces have even survived into modern times in various ways. The first complete report which survives, however, is Caesar’s Commentarii Belli Gallici. Apart from the deficiencies and merits of all surviving early sources, which were and are the subject of much scholarly discussion, one thing is very clear: they virtually lack information which is directly pertinent to the Dutch river area. It is possible to reconstruct in a general way the location of various tribes in its surroundings (fig. 118), but that is about all. Everything else has to be pieced together more or less indirectly. This is, in one way, a curious state of affairs. After all the river area or, in other words, the Rhine-Meuse delta, is by its nature situated at potentially excellent channels of communication with central and eastern Gaul. It may partially be

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1 The relevant literature is considerable, but see esp. the works of Crumley (1974) and Nash (1976, 1978a, 1978b, 1984).
2 E.g., Thompson 1965, Krüger (ed.) 1976, Jankuhn 1976, Mildenberger 1977, and Steuer 1982, to name only a few of the more important and recent works.
4 See below, chapter 11.2.
5 Cf. note 1 above.
6 Of central importance (cf. Norden 1922, 65 f. and 170) are the largely lost books of Posidonius. See ER I, 27 and esp. Tierney 1960.
explained by the fragmentary nature of the surviving sources. But Caesar's reports are not fragmentary, and the only reference to the insula Batavorum (IV, 10) is generally considered to be a later interpolation. Even when it is assumed that most of the delta was inhabited by the Menapii, which is a possibility but not a very serious one, the information provided is rather vague and imprecise.

The conclusion from this lack of information must be that the area was apparently not very interesting from Caesar's point of view. As he admits himself it was very difficult to control the Menapian territory, and he implicitly acknowledges two failures in the third (III, 29) and fourth (IV, 38) year of the war. Despite the evidently great difficulties caused by the character of the landscape the tribe was finally brought under some measure of control in the sixth year (VI, 6) when it had become absolutely vital for tactical and political reasons. It was thus difficult but not impossible to handle the Menapii, and one can be quite sure that had their land really been as important at the time as one might assume because it encompassed the mouths of three major rivers (Scheldt, Meuse, and Waal) it would have been duly conquered. In view of their resistance, the inhabitants would most likely have suffered the same fate as some of their neighbours, such as the Nervii, and certainly the Aduatuci and Eburones who were virtually annihilated.

It is unlikely that the Menapii also inhabited the eastern part of the delta, which must have been relatively more accessible than the western area, but we can safely assume that where control of the western region was only important for specific reasons and not so much for itself, the same is true for the eastern river area. It could have belonged to the territory of the Eburones, but then it does not figure prominently, or at all, in the descriptions of the campaigns in this territory. The one clear reference to the river area is the place where the German invasion-force of the Tencteri and Usipetes was finally crushed, at the junction of Meuse and Rhine (IV, 15). This Rhine can only have been the Waal, and the easternmost point could conceivably have been at Rossum/Lith (see figs. 123, 130, or 135) but if the above assumptions are correct it was further to the west because the invasion had been in the territory of the Menapii, not that of the Eburones.

However this may be, it is clear that Caesar had no great interest in the river area as such and the maximum and incidental extent of his activities is almost certainly the Waal-branch of the Rhine. This shows that the region was at least not of real strategical value and an important link in any supra-regional exchange and communication network in his day. By implication, we may conclude that in the Late-Iron Age, as seen from central and east-

7 Cf. chapter 2, 23, note 16 (with a printing error: read book IV, not VI), but see p. 370 below.
9 Namely, the possibility that they might otherwise provide a refuge and new operational base for Ambiorix, leader of a Belgic revolt, who had caused serious trouble (VI, 5).
11 Van Es 1981, 27 and note 34.
12 The only possible instance (VI, 31, 3) is a reference to islands where Ambiorix' men hid themselves. Although it is unlikely to pertain to the Overbetuwe or the Land van Maas en Waal (see Van Es 1981, note 32), this is not inconceivable either (cf. chapter 7.2.1).
ern Gaul, it was on the periphery if not indeed at the dead-end of such a network. This argumentation *ex nihilo* is, of course, not without its problems but it is supported by other inferences from the classical sources. We can take it for granted that the social organization of the population was very much like that of the Menapii or Eburones, who belonged to a group collectively described as Belgae. Some of these, the Eburones, Conduri, Segni, Paemani, and Caerosi, are repeatedly denoted as Germani Cisrhenani, and we can confidently assume the same applies to the population of the river area. The literary evidence for the North Belgic tribes in general has recently been discussed by Roymans. He reaches the conclusion that 'In Caesar's time the North Belgic tribes exhibit a number of traits typical of rather primitive, highly segmented tribal societies that are made up of a large number of smaller chiefdoms. Particularly indicative of this is the absence of social stratification, a central political authority, an integrated economy, and a hierarchical settlement structure having urban centres'.

As noted by Roymans, there is thus a marked contrast between these people and the much more complex societies further to the south. They are also different from the tribes in the Belgic 'core' area north of the Seine and in the Somme valley, of whom they can be considered to constitute a kind of periphery at least in the last century BC. In practice, the North Belgic tribes are very much like what can be inferred about Germanic society in general. Although it is not necessary to discuss here in full all arguments provided in the literature mentioned, it is useful to summarize some important aspects which should be characteristic of society in the river area during the Late-Iron Age.

First, only limited social stratification is to be expected. In general, tribes and tribal segments (*pagi*) had chief-tains, together making up a tribal council (*senatus*) but normally without a central authority or even coercive power superseding that of any one *pagus* should it wish to act differently. Characteristically, central leadership was only temporarily granted in times of war, although in some cases there were peace-time chiefs as well. This may well have been connected with a kind of tribal aristocracy formed by a leading clan or lineage, but there are numerous indications that these cannot have been people at the apex of a truly stratified society and that there was, on the contrary, a strongly egalitarian ideology. Second, the level of economic development is limited, as well as interregional trade. There are virtually no indications of more or less urbanized regional centres among the Belgae. From the context of Caesar's report one might deduce these for the Nervii (*BG II*, 28) and the Aduatuci (*BG II*, 29) but normally, if *oppida* are mentioned at all, such as in the last paragraph, one can also interpret them as temporary refuges. A much more com-

13 Even the Nervii, whom Caesar calls Belgae, are reported to have been Germans by Strabo (*Geographica IV*, 194) and later also Tacitus (*Germ.* 28), but the interpretation of these comments is uncertain, see *ER I*, 102 v. *Hachmann a.o.* 1962, 45–6.
14 Strictly speaking, those living north of the Waal could well have been considered Germani *transrhenani* by Caesar and at least for the area between the mouth of the Waal and the Oude Rijn there is even evidence for that because the area (see fig. 118) was probably inhabited by Frisians (Van Es 1981, 26).
16 Note that here 'tribe' is used as a modern anthropological concept cf. the terminology of Service (1962, chapter 4) and Sahlins (1968), comparable to what Fried (1967, chapter 4) has termed 'rank societies'. There are thus tribes, notably those in central and eastern Gaul, which are not at all tribes in a 'narrow' anthropological sense (see Roymans 1983, note 3) but stratified societies (Fried 1967, chapter 5). The ambiguity of the tribe concept (cf. Fried 1967, 154 f.) is unfortunate but apparently unavoidable.
17 Cf. the analysis of Hachmann 1976 (esp. fig. 1), followed by most authors on the subject and recently, most explicitly, by Nash (1984, esp. 99–105).
19 The dissident *pagi* of the Morini (Caesar, *BG IV*, 22) are an example, as well as the segment of the Chatti known as Batavians (Tacitus, *Hist. IV*, 12 and *Germ. 29*).
20 Cf. e.g. Fried 1967, 181–2 and Sahlins 1968, 45, and Caesar's comments in *BG II*, 23 (Nervii) and VI, 23 (Germans in general).
21 The first is termed a *dux*, the latter (a.o.) *rex*, and there is a clear difference between the two, as formulated by Tacitus, *Germ. 7: reges ex nobilitate, duces ex virtute sumunt*. This refers to Germans at least a century later (cf. Thompson 1965, 32–41), but it may have been the practice in Caesar's time as well. The Nervii chose a *dux*, but the Eburones had two *reges*. Although the latter office seems to have been held for life and was reserved for members of a royal lineage or clan (*stirps regia*), it implied no coercive power (see esp. Thompson, *op. cit.*, 36–7 and Caesar, *BG V*, 27).
22 Expressed in its purest form in *BG VI*, 22.
mon response to an outside threat was to scatter widely. This is repeatedly reported for the Menapii and also for the Eburones (BG IV, 34): "Erat, ut supra demonstravimus manus certa nulla, non oppidum, non praesidium, quod se armis defendeter, sed in annis partis dispersa multitudo." Clearly, this implies that there were no central places to defend at all costs or, rather, considered necessary to defend. Urbanized oppida serving as central administrative places were vital institutions in central and southern Gaul, representing considerable investment. Not surprisingly, indications for central accumulation of wealth and for tribute flows are also lacking as well as those for forms of redistribution managed by and in support of an elite hierarchy. By contrast, there is evidence (for Germanic societies) of redistributional levelling mechanisms counteracting wealth concentration, such as the annual reallocation of arable mentioned in BG VI, 22. In accordance with this, interregional trade, which could have provided an elite with prestige goods, was also lacking and merchants did not often visit the Belgae (BG I, 1). From the various descriptions of difficulties encountered in the movement of Caesar's troops it can be concluded that a network of proper roads was lacking, again in contrast with more southern areas.

In conclusion, it can be said that in contrast to theoretical possibilities, the position of the river area was apparently not advantageous enough, or simply too far away, for the inhabitants to have reached a level of socio-cultural development which would have made them, or their territory, figure more prominently in the historical record. An additional illustration of this state of affairs is provided by the clear evidence that it did not figure at all in the contacts with societies in southeastern Britain, which went by way of the Belgic 'core' tribes on the coast of northwestern France. This conclusion is strongly supported by what the sources reveal about the weakly differentiated and integrated social and economic organization of groups in or directly adjacent to the Rhine-Meuse delta, and differences with what is reported about Germanic society are only slight. It is also supported by another source of information: the general picture provided by archaeological correlates of social and economic complexity at the supra-regional level.

### 10.2.2 Archaeological Evidence

The archaeological information available for approximately the southern part of the area between Scheldt and Ems as described in chapter 1 (see fig. 4) has, fortunately, been the subject of some research already, but an encompassing, detailed investigation of this information is still badly needed. A great deal of material has not yet been studied or not in a coherent way. What is available today is, however, enough to be fruitfully used as an independent source of information about the same subjects as in the previous paragraph, especially when viewed from the even larger perspective of northwestern Gaul.

One of the most prominent phenomena marking the transition, in Fried's terminology, from rank to stratified society or, in other words, the process of secondary state formation in Gaul, are the large 'urban' oppida. Apart from being heavily defended refuges, they were the optimally located centres for the administration of the different socio-political units and the seats of the ruling elite, which also made them centres for regional and long-distance trade and obviously themselves foci for the accumulation of luxury goods. The development of stratified society, in spatial terms from south to north and chronologically from the 2nd into the 1st century BC, is increasingly better understood. From the distribution of such oppida as presented in the numerous publications (see fig. 119a) it is very clear that they occur

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23 'As above mentioned, there was nowhere any definite body of troops, any stronghold, any garrison to defend itself in arms, but the population scattered in all directions' (transl. H.J. Edwards, Loeb Classical Library no. 72). See also BG III, 28 where the Morini and Menapii are reported to respond with tactics quite different from the rest of the Gauls' to evade a major conflict, scatter, and resort to guerilla warfare.


25 On aspects of redistribution, see e.g. Earle 1977. For more southern groups, there is ample evidence for this, cf. Roymans 1983, 47.

26 Cf. the references in Roymans 1983, note 23.


28 It is currently being undertaken by Roymans.

only up to the southern and southeastern limits of the area between Scheldt and Ems as outlined in fig. 4.  

The oppida were only the apex of a diverse range of settlement types, including large sites which are also lacking outside their area of distribution. There are, however, in northern France, Belgium, and the Rhineland, numerous other fortified sites which were constructed or reoccupied in the Late-La Tène Period. These extend from the Pas de Calais and along the upper course of the Meuse up to the Ruhr valley (fig. 119b), and thus reach into a zone of approximately 100 km north of the northernmost oppida. This has been interpreted as an archaeological manifestation of a prolonged unstable situation, such as could be caused by frequent raiding, for example, for captives to be exchanged as slaves with Roman traders, but raids by Germanic tribes and, possibly, partially internal developments could be mentioned as well. At the same time it can be seen as a first step, even if forced and temporary, towards nucleation which could have led to a development similar to that in central Gaul. There are still problems with dating many of the sites involved, but it is fairly certain that among the North Belgic tribes up to and — not to forget! — during Caesar’s campaigns, a kind of crisis situation produced nucleation. There is little evidence for a more permanent character of this nucleation, let alone for true central places. But then it has to be remembered that the period involved is probably in the order of only half a century. However this may be, fig. 119b clearly demonstrates that although fortified sites could hardly have been established in the Holocene river delta itself they could have been constructed on the high Pleistocene ridges surrounding it, and, moreover, there is still an empty zone of approximately 100 km to the south of the delta where such sites are also lacking. One might suggest that the area was simply too far away for raids from the south. When a threat from the north is considered, it was perhaps not so much itself threatened but rather the southernmost part of the region where the danger came from.

But in either case it cannot have been of major importance on strategic or political grounds, otherwise there would have been attempts to control it and conceivably some archaeologically visible response to such attempts. Therefore, we cannot but conclude that the population in the area concerned must have had a rather low level of socio-political organization. This is the reason for the poorly developed, and for the lack of developments in, settlement pattern which was (or became) different in adjacent southern regions.

As was to be expected, this conclusion is supported by the evidence from burials, although that is not exactly abundant. The southern Netherlands and northern Belgium seem to belong to one group which is characterised by increasing simplification in burial rites throughout the Iron Age, culminating in the archaeologically near invisibility of burials in the last centuries BC (cf. paragraph 8.2.2). Apart from what they might mean, wealthy burials are lacking from this region. This situation is different in a zone from the middle-Rhine valley along the river Mosel over southern Belgium and in France north of the Seine (and from there extending into southeastern Britain). There are quite a few late-Iron Age burials known from this zone and, notably from the last century BC, also a number of wealthy burials. There are various unifying characteristics in this burial tradition which should not be seen as a direct derivative from southern traditions. The particular manifestation of burial customs south of our area is not entirely – in fact just barely – pre-Caesarian and a direct interpretation in terms of social personae recognized by those customs is not advisable because we know how much change there was in the area during the 1st century BC. The rich burials as such can perhaps best be seen as a form of conspicuous consumption of commodities, motivated by a desire for political legitimation. This is especially relevant in a society where rules are changing and unclear, with resulting competition over social positions. It could also provide a reason for the observed

30 E.g. maps in Collis 1975, Collis, in Cunliffe/Rowley 1976, fig. 4, Nash 1976, fig. 1, Hachmann/Kossack/Kuhn 1962, Karte 2, and many more.  
33 For a simple and elegant model of changes in a settlement pattern in a crisis, and various possible consequences, see Collis 1982, 74–6.  
35 Caesar (BG VI, 19) explicitly mentions the splendour of Gaulish funerals. See also Duval 1984.  
36 Cf. Haselgrove 1984, 22–3. A similar line of reasoning is adopted by Pearson (1984, 78–80) to understand the increasingly rich Danish burials during the same period, although developments there are seen primarily as the result of internal processes.
distribution, assuming the causes are to be sought in trouble coming from the south. At the same time, we may conclude that a similar situation apparently did not exist in the southern Netherlands. Burial customs there remain the same and convey no sense of either the recognition of a deceased's status or of social legitimation and competition. Instead, as far as the surviving archaeological record is concerned, the burial evidence indicates sound and stable egalitarianism.

The general lack of wealthy burials does, by the way, not imply that accumulation of wealth by some more powerful individuals was entirely absent. There are a number of precious objects known, as well as hoards,37 but they are relatively scarce. The same is true for Celtic coins, which are almost exclusively of gold and very rare until the end of Caesar's campaigns. Such coinage is generally considered to have functioned primarily as special purpose money in the prestige sphere.38 There are, however, arguments to assume that in the southern part of the Low Countries, where coins are so scarce and only the Nervii and Eburones are known respectively supposed to have minted their own and only at a relatively late date,39 the Celtic glass bracelets may have fulfilled the same function. But even when this interpretation is accurate there is still a difference from the area of circulation of gold coins and if the distribution of coins and (certain types of) glass bracelets should prove to be complementary as is — probably correctly — suggested by the distribution map (fig. 120), that difference is even strengthened.40

As far as primary agricultural production is concerned it is not probable that the southern Low Countries differed much from central Gaul. One might assume that surplus production was needed on a far smaller scale, but this is not necessarily true. Not so much because there was a large surplus-consuming elite here but because in central Gaul the size of the surplus-consuming population not directly involved in agricultural activities and living in the larger centres, relative to that of the total population, may not have been all that large either. Nevertheless, pollen analyses in the river area and the Rhineland have clearly shown a marked increase in culture-indicators and decrease of forest throughout the Late-Iron Age.41 In principle, it would be more logical to attribute this to increased technical capabilities rather than an archaic technology as is sometimes suggested.42 There are not, however, any signs that increased pro-

38 See Scheers 1977 and esp. Roymans/Van der Sanden 1980. The latter publication also offers an overview (p. 181–3) of recent interpretations regarding the function of Celtic coinage.
39 Scheers 1977, 88–97. The first (gold) coins were minted during the Gallic wars. There is no evidence for coinage of the Menapii, Aduatuci, and the other Germani Cisrhenani apart from the perhaps somewhat uncertain (Scheers, op. cit., 441–2) staters of the Eburones.
40 Willems 1983a, 110–2. On the limited distribution along the Rhine in Germany (only up to the Lippe valley) of forms Haevernick 3b and 7a–b, see Reichmann 1979b, 128–9 and 273–4. The distribution in Belgium is still largely unknown.
41 Teunissen 1982; see also paragraph 10.4.2 below and fig. 137.
42 E.g., Roymans 1983, 52. An increase in arable and forest clearance connected with agricultural innovation has been argued for Britain (Jones 1982).
duction and population caused changes in the economy, which must have remained fairly autarkic. Exchange is visible in the distribution of glass bracelets and of products such as querns of basaltic lava (tephrite) from the Eifel and salt from the coast,43 bronze or bronze objects, and perhaps iron. With the exception of the few extraordinary objects mentioned, such as the silver disc from Helden (fig. 121),44 there are no indications of long-distance trade.

Pottery production was entirely a local affair and at a rather low level of technical sophistication. Wheel-turned pottery was not manufactured but, on the other hand, there is good evidence for the use of technically more advanced updraught kilns at least from the Middle-Iron Age onwards.45 This could indicate semi-specialist production above the household-level but surely not sudden changes caused by influences from the south. In any case the pottery-production shows continuity throughout the Iron Age. It can be typologically differentiated from other groups,46 and thus testifies to a relatively autonomous development.

10.2.3 Conclusion
Although the overview of the evidence as presented above is brief and incomplete, it is sufficient for a general evaluation of social, economic, and political conditions in the river area relative to those elsewhere. The Rhine-Meuse delta as a whole emerges not so much as a collection of islands occupied by fierce barbarians living on fish and birds’ eggs (BG IV, 10) but as a fairly homogeneous and relatively isolated area with a thriving population, socially and economically well adapted to its surroundings. We cannot be sure of the tribal groups constituting this population, but it is certain that the delta was occupied by, or the border between, several of these. The western part was occupied by Menapii in the south and Frisii in the north. The eastern part was perhaps inhabited entirely by (a group associated with?) the Eburones, but it may have been divided between Germani (Tencteri, Usipetes?) in the north and Eburones in the south.

These divisions do not, however, seem to be very important. Undoubtedly the different opportunities offered by the landscape will have caused economic diversity between west and east in general but also — as will be discussed below (10.4.2) — between settlements in one area. But the overall level of economic and social organization must have been — as far as we can tell — the same throughout the delta. Both the historical and the archaeological record allow the conclusion that this level is comparable to what is known about Germanic tribes in general and quite different from that in Belgic Gaul north of the river Seine and around the Somme valley, let alone from that in central Gaul. The archaeological evidence might even be taken to suggest that there was a difference, at least in the last century BC, between the northernmost areas of the Menapii and Eburones and the other ‘North Belgic’ tribes. This evidence (fortifications, dispersion of coins, and the like) is, however, more likely to be the result of increasing instability in these regions caused by interference from the south, from which the Menapii and Eburones were largely spared until they

44 The disc was found in the peat area of De Peel (see fig. 135, SE corner) and is quite probably a votive offering. See also Allen 1971.
45 Dutch sites are those in Serooskerke, Ressen-Bemmel (site 165), and Horst-Meterik (Willems 1984, 372–4).
46 Cf. e.g. Mariën 1971, 215, who proposes an identification of the Groupe de la Campine with the Eburones.
were directly affected by Caesar's campaigns. There are no good reasons to assume that there were already deeply rooted differences before the Gallic wars and - although chronological control leaves much to be desired - there was perhaps not more than a few decades or half a century for this process.

This last assumption seems to be contradicted by evidence for the emergence of a well-defined elite group in the social hierarchy in southern Belgium at an earlier date, in late-Hallstatt - early-La Tène times. This could mean that much older differences existed, but such a conclusion implies the assumption of a more or less continuous development. There is, however, an alternative for such an interpretation, which is most strongly suggested by the evidence from the southern Netherlands and northern Belgium. There, it is evident from a number of rich late-Hallstatt (Oss, Wijchen, Baarlo, Overasselt, Mecerlo, Hegelsom) or early-La Tène (Nijmegen, Eigenbilzen) burials that in the 7th-5th centuries BC an elite manifested itself for which there is no evidence later on. This development is paralleled elsewhere, and indicates a more or less general cyclical process, the effects of which vary in different areas and are likely to have been determined by varying internal dynamics: for the southern Netherlands, the indications are that there was a reversal to an egalitarian society, elsewhere it may have been less marked.

Such a reversal does not of course need to have been as strong in reality as is suggested by the evidence and it needs to be explained as well. It is not very useful in the present context to speculate on this subject, but one may at least infer that the emerging elite was clearly not able, for whatever reason, to continue to manifest itself by prestige goods in any way. Either external input or internal demand or both were failing, and that should have had its consequences. But the important point is that the archaeological evidence for the southern Netherlands and northern Belgium clearly indicates a low level of social complexity during the Middle- and much of the Late-Iron Age. The North Belgic tribes in general can be seen as relatively autarkic and segmented tribal groups which only in the last century BC, and then primarily the southern groups known as the Morini, Nervii, Aduatuci, and the southern Germani cisrhenani, became subject to interference from the south.

This caused a number of changes, such as temporary nucleation in fortified sites and, in most areas for the first time in centuries, sure signs of an elite group manifesting itself. This can be seen as a first step towards a stratified society and it might well have resulted in a development similar to that in central Gaul one or two centuries before. As it is, the process was strongly affected by the direct interference from the core area itself: the Gallic wars. In a way, this may have accelerated internal processes of change, due to the greater need for organization of socio-political and economic affairs. The introduction of coinage by some tribal groups could have been one such effect. But one should not forget that the North Belgic tribes, even when Caesar's remarks on their bravery and bellicose nature are partly ascribed to the same difference. For example, their bravery and bellicose nature are partly ascribed to self-justification and propaganda, did have social institutions to cope with an external threat, such as the installation of a war leader.

The external threat in question, however, proved to be of a magnitude that could not be coped with. In this respect, there is an interesting difference between the northernmost and the other Belgic tribes. In the north, the main response was to scatter widely and to hide out. Therefore, there was at the same time no firmly organized resistance to evoke attacks nor the possibility of easily gaining control by an overwhelming force. Unless deemed absolutely necessary for other reasons, such tactics cannot be answered by an army except by a very costly and long-drawn effort with no guarantee of success, as generals are still finding out today. In contrast to this, the better organized resistance in the south, where armed men were rallying instead of scattering and the population was concentrated at defended sites, left only two alternatives: submission or utter destruction.

Although dispersal and guerilla tactics are in themselves no proof, the different fate of people in the river delta and those to the south can, in this way, be properly understood as the result of a difference in social organization. Admittedly, there are other factors, but their importance is related to the same difference. For example, as mentioned earlier, one can be sure that if the delta area had been important, for example as a major connection with Britain which it was to become only much later, it would have been conquered despite all difficulties. But then, if the area had been so important, its popu-

48 E.g., Mariën 1971, 219, the general characteristics indi-
ulation would undoubtedly have had a more developed kind of social organization. Also, the delta area is eminently suitable for dispersal tactics but it is, in principle, equally well possible to concentrate people and defences there, just as in other areas one can disperse and effectively hide out in woods and valleys (for example, the Sugambri).

The latter tactic was, in fact, adopted at one point by the Eburones (cf. note 23). But as a tribe they also had an elite group with two reges at the top who were capable of organizing resistance. Ambiorix continued to resist even after initial defeat, much to the detriment of the tribe which was virtually annihilated, to a large extent physically and entirely as a socio-cultural entity. One may wonder whether this included people in the river area, which depends on how far north one is prepared to postulate Eburonian territory and how thorough the genocide in all its reaches. But in any case, all North Belgic tribes suffered heavily from the war, also those who are known to have survived it as socio-cultural entities: the Morini, Nervii, and even the Menapii whose territory was, after all, repeatedly laid waste. Although the archaeological record has very little to say about this, we can assume from the historical data that the entire complex of interwoven societal systems was disrupted, the population reduced, and the subsistence base damaged. The consequences of all this, the events until the start of Drusus’ campaigns, and the resulting situation in the river area, need to be examined next.

10.3 FROM CAESAR TO DRUSUS

10.3.1 Historical Inferences

The period between 51 and 12 BC is notable for the absence of historical data pertinent to the situation in the southern Low Countries or, for that matter, to the entire former area of the North Belgic tribes. All we have are inferences based on the general framework of historical events and the information about the situation in 12 BC and later. The most important of these is, of course, the penetration in the area of tribes or tribal segments from across the Rhine. These are groups known as the Frisiavones, Cananefates, Batavi, Texuandri, Tungri, Baetastii, Ubii, and Sunuci. Their migration is not difficult to explain historically, in view of the previous destruction and depopulation of the area, although the reasons for leaving their former territories may have varied. Depending on the detail of later information their new locations can be reconstructed more or less precisely (fig. 122).

The degree of Roman interference with the area must have been restricted, although perhaps less so than could be concluded from sheer lack of information. After all, the civil war and the major enterprises of Octavian/Augustus, such as the war in Spain, attracted most attention. There is much to say for the assumption that the new groups settling in the area was the result of a conscious policy and not an entirely spontaneous process. We even known this for certain in the case of the Ubii,50 and somewhat later the same thing is reported for the Sugambri.51 With the probable exception of the western certain, but must have been very close. The latter were perhaps a segment of the former (cf. Rüger 1968, 9; see also Vittinghoff 1976, 74 and note 5).

50 Tacitus, Germ. 28 and Cassius Dio LVIII 49, 2.
51 Tacitus, Ann. II, 26 and XII, 39, Suetonius, Aug. 23 and Tib. 9. The relation between the Sugambri and Cugerni is un-
Tribes, the Frisiavones, supposed to be a segment of the Frisii and later (?) separated from these by the Cananeates, the new groups all came from the region east of the Rhine which is known to have had close contacts with the Belgic area. In addition, several are known to have been old allies of Rome. This is true for the Batavians whose relation to Rome was described by Tacitus (Germ. 29) as an _antiqua societas_, the Cananeates, who presumably held the same position, and the Ubii of whom Caesar had already reported that they _amicitiam fecerant_ and later _in deditonem venerunt_. From the fact that they were settled on the west bank of the Rhine by Tiberius (cf. note 51) it follows that by then it was also true for (the Rome-friendly fraction of ?) the Sugambri known as Cugerni.

In contrast, military action was undertaken by Agrippa in 38 or 37 BC and various offensives are known against German tribes during the next two decades, including one (the _clades Lolliana_) that ended in defeat. Groups such as the Suebi, Tencteri, Usipetes, and quite possibly a part of the Sugambri, were prevented from settling west of the Rhine. The location of Cananeates, Batavi, Cugerni, and Ubii along the Rhine can hardly be a coincidence for they also effectively shielded off possible spontaneous migrants living _intorsus_, not along the coast or the Rhine, such as the Frisiavones by the Cananeates.

The nature of these tribes in the interior is poorly understood but they appear to be for the most part rather makeshift compositions of older and new population groups, perhaps comparable to the anthropological concept of the composite tribe, the result of a breakdown and depopulation after contact with western civilization. Possible examples are the Frisiavones (former Menapian territory, Sturii?, Marsaci?), _Texuandi_ (former Eburonian and Menapian territory; explicitly used by Plinius as a collective name), and the Tungri (former territory of the Aduatuci, Eburones, Condruis, Segni, Paemani, and Caeroci). There is every reason to believe, however, that we are not so much dealing with an entirely spontaneous regrouping into composite tribes but rather with a somewhat engineered process. There are several clues for this, such as the enforced internal peace, placing new rulers at the head of tribes, the actions across the Rhine, and the exclusion of some particular tribes mentioned above, or the very early proof, in 36 BC, for German troops in the Roman army. All this, of course, does not mean that the Roman authorities were in perfect control in the entire period under discussion, nor that there may not have been – on the native side of the shield – a good deal of disintegration and disorientation, and not very much acculturation, as Roymans indicates. But on the other hand, there is no indication that one was ‘at a loss in knowing what to do with this peripheral Belgic area’ and neither ‘did Roman power not manifest itself effectively and in a positive way’.

Initially, there must at least have been a need for Octavian to secure the northern part of Gaul, and we can be confident that Augustus, perhaps even when still Octa-

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52 Cf. Van Es 1981, 27 and note 35.
54 Caesar, _BG_ IV, 16 and VI, 9. See also Rüger 1968, 5–9.
55 See the overviews in Rüger 1968, 8 and Von Petrikovits 1978, 53. As also noted by Rüger (1984, 10) a simple punitive expedition such as that by Marcus Vinicius against transshenish tribes for the murder of Roman traders in 25 BC (reported by Cassius Dio LIII 24,2) was most likely a typical case for a whole series of similar events. This particular incident could, by the way, have survived in the records especially because this same person, a good friend of Augustus, became so important later, in 2 BC, as supreme commander of the Rhine legions.
57 Plinius, _Nat. Hist._ IV, 106: _intorsus_ ... _Nervii_ ... _Tungri_ ...
60 Plinius, _Nat. Hist._ IV, 106: ... _Texuandi pluribus nominibus_ ...
62 E.g. Strabo, _Geographica_ IV 3, 5, C. 194. For a general account of Roman measures, see also Drinkwater 1983, 20–4, 93 f., 120 f.
63 E.g. Commius for the Atrebates ( _BG_ IV, 21) and later ( _BG_ VII, 76) also the Morini. A later example is Italicus, made chief of the Cheruci (Tacitus, _Ann._ XI, 16).
64 Appianus, _De Belis Civilibus_ V, 117: Germani with Octavian on Sicily. As Bellen (1981, 39) convincingly argues, these are likely to have been raised by Agrippa in the foregoing years and quite probably especially among the Ubii and Batavi.
65 Roymans 1983, 57.
vian, planned various expansionist designs. That these were purposively and effectively prepared is demonstrated by the arguments provided and, above all, by the fact that the former North Belgic territories along the Rhine were a readily available operations base for the large-scale conquests when needed. Nevertheless, most of what happened may well have been achieved by fairly indirect means, direct interaction remaining relatively limited. This is not only suggested by the historical record, which shows that the important events and thus the most direct Roman interference in local affairs were taking place elsewhere, but also by the archaeological information.

10.3.2 Archaeological Evidence
When regarded from the same viewpoint as the historical data, from a supra-regional and Roman perspective, the archaeological evidence in our area shows the same features. That is to say, on the whole such evidence is very deficient and indications for much direct interaction with the Roman administration or with traders are very limited. However, even in view of all the changes which must have happened in the period under discussion, one should not forget that it lasted only a few decades and the potential of the surviving archaeological record to register such changes, especially when followed by so many others immediately afterwards, is necessarily limited. Nevertheless, the nature of the changes such as large-scale depopulation and immigration of whole groups is such that the available archaeological evidence seems to be conspicuous by its limitation.

To be sure, it is not absent. For the territory of present-day Belgium, a number of data indicating changes, for example, in pottery forms, have been summarized by Mariën. Other changes are the disappearance of gold coins, most of which seem to have been minted in the North Belgic area only during the Gallic Wars anyway, and the issuing of some silver and especially copper and potin coins. This may be related to the use of natives for military service (the silver), but also be seen as a means to facilitate exchange in the new situation where traditional networks were disrupted and old neighbours perhaps replaced by new. A more or less contemporary and quite probably causally related phenomenon is the (gradual?) disappearance of the Celtic glass bracelets towards the end of the 1st century BC. In some areas, notably the (Eburonian) region between Rhine and Meuse west of Neuss, there are perhaps also indications for the abandonment of settlements in the mid-1st century BC, an interpretation which is supported by the results of ecological analysis showing a marked decrease in agricultural activities there during the last century BC. This is, however, not necessarily a general phenomenon and the ample evidence for the river area at least shows no such decrease.

Direct (military) Roman presence in the area, and then exclusively in the south, can only be demonstrated from the beginning of the second decade BC, in such sites as Liberchies, Tongeren, and Neuss. But even evidence for indirect contact, in the form of Roman imports, is virtually lacking. Admittedly, one may expect that most of the conceivable trade products left no archaeologically retrievable traces but it is remarkable that one of the best indications for trade contacts, early Roman amphorae, are almost absent. From the distribution of such finds as Dressel 1 wine amphorae it is abundantly clear that they almost entirely avoid the North Belgic area, the few known northern specimens being related to later Augustan camps. In fact, the distribution can only be taken as evidence for the relative poverty of the entire area but also for the fact that the new occupants were as weakly integrated into long-distance trade networks as were their predecessors.

66 Cf. e.g. Wells 1972, chapter 1, Mann 1979, 178-9, or Drinkwater 1983, chapter 1. A similar reasoning for Roman control and preparations along the Rhine in Rüger 1984, 10-11. For a penetrating analysis of Roman measures for control and infrastructural improvements in Gaul, see also Wightman 1977. For an opinion contrary to most writers on this subject, see Timpe 1975.
67 Mariën 1971, 228-34.
68 Roymans/Van der Sanden 1980; Roymans 1983.
69 Peddemors 1975.
70 Rüger 1984, 14-5. It is, however, unclear on which evidence such a detailed conclusion is in fact or, for that matter, can be based. Very detailed C14-dated pollen diagrams would seem to be the only possibility. At least in general the diagrams for the German loess area are rather more comparable to those in the Dutch river area (lecture Dr A.J. Kalis, Reuvensdagen conference November 14, 1980, in Maastricht; see also Kalis 1983).
72 For an overview of earlier Roman activities further into Gaul, see Wightman 1977 and Drinkwater 1983.
73 Cf. Gechter 1979, see esp. fig. 53.
74 See Haselgrove 1984, fig. 3 and esp. Fitzpatrick 1985.
This is, in one way, not very surprising. As mentioned earlier, and repeatedly noted by most writers on the subject, there was a strong similarity between the tribes on both sides of the Rhine, more differences being found between north and south than between west and east. This state of affairs may indeed go a long way into accounting for the relative lack of indications for change. As Rüger put it: Die Einwanderung rechtsrheinischer Gruppen hat die spätlatènezeitliche linksrheinische Tradition Ostgalliens ebensowenig geändert wie die faktische Zugehörigkeit zum römischen Reich seit etwa 50 v. Chr. 75

Such a statement is undoubtedly too strong in view of the difficulties inherent in providing archaeological evidence for such a short period and also with regard to what has nevertheless been found, let alone what may be discovered by concentrated research efforts in the future. But in many respects it is probably still correct. The burial evidence, for example, does not seem to change in this period. Wealthy graves are only found in the same zone as before, immediately south of the North Belgic area. Also important is the fact that the relative scarcity of archaeological indications for changes in population groups is not really in conflict with historical evidence, because a sizeable part of the original population did survive the direct and indirect effects of the Gallic Wars. The northernmost groups successfully applied tactics of evasion and others, such as the Morini and Nervii, suffered heavily but survived nevertheless. That leaves only the central part of the Eburonian territory between Rhine and Meuse, and partly between Meuse and Scheldt, in which to look for the most profound changes, and at least in the Rhineland there now is or seems to be some positive evidence for this (cf. note 70). In the delta, however, where the Batavians are supposed to have settled, the archaeological data indicate another situation, which can now be evaluated.

10.4 SITUATION IN THE RIVER AREA

From the preceding paragraphs, and also from what has been discussed in earlier chapters, it can be concluded that the Dutch river area was outside the mainstream of events during the entire Late-Iron Age. In the present historic situation socio-economic and political conditions were such that its geographical position was of little consequence. By the last century BC, it lay in fact on the extreme periphery of a Belgic core area in northwestern France and was even more peripheral in relation to the central and east Gaulish civitates.

As far as the eastern river area is concerned, we cannot be certain about the socio-political identity of its population. As noted above, in Caesar’s time the Waal may have been a border between the northernmost group belonging to the Eburones and Germani transrhenanii (Tencteri and Usipetes?). This is in accordance with the fact that we know (cf. chapter 3) that the Waal was the most important branch of the Rhine from a hydrographical point of view, but that does not mean much except as an explanation for Caesar’s mistaken identification of the Waal as the Rhine. Archaeological support for a Waal border can perhaps be found in that some settlements to its south appear to be slightly different in the sense that they are somewhat ‘wealthier’ (see 8.2.1). However, if this difference means anything it is more likely to be relevant as an indication for qualitative differences at the micro-regional level. This is all the more so because there are no other differences between the areas north and south of the Waal, in particular not in the pottery. This is typologically quite coherent for the eastern river area as a whole.

Unfortunately, because of a lack of available material it is not so easy to determine if, as a group, it is distinct from the pottery in adjacent regions. Virtually nothing is published about late-Iron Age pottery in the area directly north of the Rhine and the difference in native wares north and south of the river during the Roman Period may not necessarily have been present in pre-Roman times. In general, however, it seems that the Rhine was a major divide and the published information about pottery south of the Meuse indicates that it is at least very similar to river area forms. 76 In contrast, the pottery assemblage from the coastal provinces in the west is different again. 77 The limited range of other artefacts, including the glass bracelets, and also the still limited data on regular house-plans 78 as well as, perhaps, the irregular ‘post-hole swarms’, 79 point to the same conclusion as indicated by the pottery: similarities are in the first place to

75 Rüger 1968, 14.
76 For a concise overview, see Van den Broeke 1980, esp. 53-4.
77 Little has been published as yet, but an inventory is currently in preparation, see Van Heeringen/Van Trierum 1981 (with further references).
78 Van Es 1982.
79 Above, paragraph 8.2.1.
be found in the south but there are, at least at the moment, no arguments to prove either that the zone directly north of the Rhine was very different or that it should be included in the same group.

It is evident that as yet – if indeed ever – archaeological data do not allow a firm statement as to which tribal unit the population of the river area belonged. But the lack of internal differences and also the specific character of the landscape make it more than likely that the region as such was inhabited by one, socially and economically homogeneous, group which, from a supra-regional perspective, was more affiliated with groups to the south than with those to the north. There is nothing against an interpretation of this group as one of the largely autonomous segments of the Eburones, but positive evidence is lacking.

It is not impossible that this river area group was known as the Batavians. As Sprey has convincingly argued, etymological analysis indicates that bat/bet and aue are the constituent elements of the name, which would thus mean: (people of) the good/fertile waterland/river island. He suggests that we are dealing with a latinised German name and that the population was called after the region, not the other way around. This interpretation may, in fact, imply that the interpolation in Caesar’s report referring to the Batavian island (in this sense thus a pleonasm!) may not really be corrupt and much later. It could be an example of the marginal notes from Caesar himself which are supposed to have existed by some authors and inserted into the text after his death. But neither such an assumption nor the implication that the population in the river area was Germanic-speaking during and before Caesar’s time are grounds on which further conclusions can be based.

The migration of a segment of the Chatti as recorded by Tacitus leaves little doubt that this, if not indirectly caused by, was at least made possible by the effect of the Gallic Wars (settling in uninhabited lands). Whether these people became known after an existing Germanic name for the region or after the name they themselves gave it is not really important. Much more relevant is the matter how such a migration and its effect in the river area can be properly understood, and notably in relation to the archaeological evidence.

An important basic consideration in this respect is the degree of continuity or discontinuity visible in the archaeological record, and that is precisely the point where troubles start. In principle it is not very difficult to argue for continuity. As far as settlements are concerned, the evidence in chapters 4, 5, and 8 clearly indicates continuity from the Iron Age into the Roman Period for the region as a whole, from a chronological and also a settlement-typology point of view. Burial evidence is not available, and pollen diagrams show gradual change only in the Roman Period. Finally, the native pottery shows some development from the Late-Iron Age into the Roman Period, but that change is restricted and difficult to detect. Whatever its interpretation may be, it is certainly not a major change in native pot-making traditions.

Unfortunately, it is all too easy to demonstrate that the evidence for continuity is not as firm as it may seem to be. Excavations are few and far apart, and the degree of chronological control in the period before the use of wheel-turned pottery is very minimal indeed. It is perhaps not very probable but nevertheless conceivable that whole scores of settlements were in fact not continuously inhabited and even that on most of these sites the habitation was temporarily interrupted around the mid-1st century BC.

Fortified settlements, which frequently demonstrate such interruptions, are lacking in the region (cf. fig. 119). The pollen diagrams can also be interpreted in another way, because it could be argued that even a total lack of cultivation of the land lasting only a few decades is probably not visible or very difficult to demonstrate in the diagrams. The lack of burials does not allow any firm conclusions in any direction and even the seemingly best evidence for continuity, that of the pottery, is not entirely unequivocal. There is no major change, but that could also mean that the pottery of immigrants was not very different from that of the original population.

On the whole, archaeological data are thus not precise enough to reach definite conclusions, but it is at least not different from that of the original population. The matter undecided for the river area.
very likely that the river area was largely depopulated and reoccupied by a different group. Even the crude measures discussed above would have given somewhat more evidence in the case of such radical change, but there is more than that. Our historical sources reveal that a group of Chatti moved to the river area. They lived formerly in Northern Hessen, according to the almost universal scholarly opinion, and the historical link between the two regions has recently found archaeological support in the form of Celtic coinage. The so-called silver triquetrum coins of the Lith group, characteristic for the river area, are interpreted as a sequel to the emission of gold triquetrum staters which are only found in Northern Hessen and the Rhineland.85 Accepting this at the same time confirms both the historical sources and the conclusion that the region was not largely depopulated, because people from Hessen who almost entirely replaced the river area population would surely have left archaeologically detectable changes. It also indicates what the most likely interpretation of the events should be.

From what has been said in the previous parts of this chapter it appears that Caesar’s troops were hardly if at all active in the Rhine-Meuse delta. Undoubtedly the tactics of evasion and dispersal instead of confrontation and combat could not prevent destruction and disruption insofar as the troops did operate, but even there the population as such largely survived. Thus, if the people in the river area were actually affected by the war, it may not have done them too much damage in the long run and the area continued to be occupied very much or entirely as before. This agrees with the limited archaeological data we have and indicates that the arrival of a new group in the area can only have concerned a relatively small number of people.

It is not known how large the fraction of the Chatti, which Tacitus calls Batavi, actually was. But in his description of the migration, Tacitus says that ‘they occupied the edge of the Gallic bank, which was uninhabited (vacua cultoribus) and likewise an island close by, which is washed by the ocean in front but by the Rhine on its rear and sides’.86 When taken literally, this could imply that the Betuwe, or perhaps the entire river area, was not vacua cultoribus, in contrast to at least the area south of the Meuse. The former is probably true, as discussed above, but there are not yet enough data available for the Pleistocene sand area of Brabant to say anything about archaeological changes there. On the other hand, the most direct archaeological clues for Batavians are distributed in an area which more or less agrees with Tacitus’ description (see fig. 123). The silver triquetrum coins have mainly been found along the Meuse and at the point where Meuse and Waal are very close together, and then there is the altar from Ruimel dedicated by Flavus, summus magistratus of the Civitas Batavorum in IA.87

If this evidence is relevant, we may conclude that the area where the main body of immigrants settled is in the centre of the river area on both sides of the Meuse and the lower course of its small tributaries in Brabant, on a small stretch of the Pleistocene sand area. This would be in agreement with such descriptions as extrema Gallicae orae and non multum ex ripa. If the size of the new group was significant enough, future research in this area should provide more archaeological support for this hypothesis.

For the moment, however, all we have are artefacts related to an elite group of which Flavus presumably was a descendant and who issued the silver coins.88 This group did, however, settle in a strategically important area as far as control of the entire river area is concerned and was apparently able to gain that control rather quickly.89 It may well have had some sort of central place in the Rossum/Lith area on the northern bank of the Meuse and close to the Waal.90 This at least presupposes a substantial number of men because, even though the new group was backed by Roman force as suggested earlier (10.3.1) they needed – and, if the interpretation offered above is correct, were intended – to perform the task of securing the river area themselves.

As mentioned in chapter 2, it is possible that the immigrants were primarily some sort of irregular auxiliary aristocracy. A tribal emission (by a council or a king) is also possible.

85 Roymans/Van der Sanden 1980.
86 Tacitus, Hist. IV, 12 (translation C.H. Moore); also in Tacitus, Germ. 29.
87 Cf. chapter 2, 24 and chapter 11.3.1. See fig. 127. For a similar opinion on the implication of the findspot of the altar, see Rüger 1968, 33–4, contra: Bogaers 1972a, 328 and note 75.
88 This does not necessarily imply a private emission by the
The distribution of: 1 silver triquetrum coins of the Lith group, 2 findspot of the altar from Ruimel, 3 later Celtic bronze coinage (copper triquetrum coins of the Bochum group, GERMANVS INDVTILLI L, and AVAVCIA coins). The distribution of 1 and 2 may define the earliest area under Batavian control (4), while the distribution of later coins (compare fig. 128!) is determined by the Augustan military campaigns. Coin distributions after Roymans/Van der Sanden 1980. For a legend of the geology, see p. 13, fig. 3 or p. 421, fig. 135.
force and the site near Rossum/Lith was a camp, for which the silver coins could be an indication. This interpretation is also attractive in that it might explain the seeming absence of foreign pottery: if women were the potters and if we are dealing with a largely male group of immigrants they would have been provided fairly quickly with local wares. But the assumption of a largely male group is not very plausible, because the sources clearly imply that a whole segment of the tribe of the Chatti was involved, and we may assume that this encompassed at least a number of lineages but more probably a segment in the sense of pagus, one of the fairly autonomous units constituting a tribe. An assessment of events and social and economic conditions in the river area at the end of the Iron Age, on the basis of what is known from various disciplines, is clearly a matter of much interpretation and little certainty. Nevertheless, enough is known to arrive at a model which should be accurate in major outline and encompass at least a number of elements from which hypotheses testable by future research could be derived. This model – or speculative reconstruction – can be summarized as follows.

10.4.1 Social and Political Organization
Before the Gallic wars the fertile river area, Bat-auha, was either inhabited by a homogeneous group of people composed of one or a few tribal segments and probably affiliated with the larger tribal whole known as the Eburoines or by a quite distinct group. In either case, the social organization was predominantly egalitarian with little difference between individuals, no permanent leadership, and economically a considerable degree of regional autarky and adaptation to local conditions with relatively little outside trade. The settlement system may have been diversified but it was not hierarchically organized and thus there were no central places. The Gallic Wars did not substantially disrupt this situation both because sociopolitical conditions in the region or its position from a supra-regional perspective did not trigger much military action and because as far as that action did take place the response of evasion and dispersal prevented irreparable damage. Although relatively undisturbed, society in the river area must of course have been affected by the wars. Existing relations with the south, whether economic (trade) or more encompassing than that (affiliation with the Eburoines) were surely disrupted and considerable destruction and perhaps depopulation took place south of, if not in the south of, the river area. Famine and fugitives are also factors which may have had an effect in the river area although their true impact is impossible to estimate. During the following decades spontaneous and induced migrations as well as autonomous and more or less engineered regrouping took place in northern Gaul. But already at a very early stage, Roman concern for and thus more direct influencing of developments was strongest along the Rhine and, in so far as they were not already in the vicinity, the most dependent allies were manoeuvred into those areas. Conflict among the Chatti, causing tribal fission, led to the migration of the segment of the tribe that for some reason was allied with Rome. These people moved to a distant and for an autonomous move rather unlikely new location in the centre of the Dutch river area, perhaps to those locations where the original population had suffered most from the war and its aftermath. But the opposite, settling as close to a remaining centre of population, if there was any, is also conceivable. Nothing can be said about a variety of possible differences, for example, in language, between the new and old populations but, the former being a Germanic tribe, we can be quite sure that from a general perspective of social organization there was much similarity. A well-established tribal aristocracy (stirps regia) may have been typical for the immigrants, and the minting of silver coins is presumably associated with them. In any case we know with certainty from the historical information that the Batavi were once part of the Chatti and that the new group became dominant in the new social formation. This also follows from the inferred Roman support, perhaps a more effective military organization, and from the fact that their apparent primary area of settlement was strategically so well situated. It is rather futile to speculate on how, or how far, the old and new populations became integrated into one new tribal entity because we know so little about such basic relevant facts as the period of time or relative numbers of people involved. Moreover, there is no need even to assume some quantities of imported native wares as is demonstrated, for example, by the Frisian pottery associated with Frisian auxiliaries on Hadrian’s Wall (Jobey 1979).
sume that such integration should necessarily have taken place before the beginning of the Roman Period. If we are indeed dealing with a segmentary tribal system there is no point in presupposing an immediate coherence maintained from above by sociopolitical institutions, but to expect only a very weak integration at the tribal level. Several groups may initially have fruitfully coexisted under the domination of the new immigrants with only a kind of nominal common identity. At the same time, however, the entire system was subject to change by increasingly effective and direct interference from outside, thus providing opportunities for an elite to manipulate and develop the characteristics of a ruling class. But there is no way of knowing how far this process actually went because in the first half of the 1st century AD the imposition – not necessarily entirely forceful – of a Roman civitas model obscures indigenous developments. The position held by Flavus, for example, that of summus magistratus, does not necessarily imply that he was the highest ranking individual at the apex of a well-defined social hierarchy. The function as such may have been more the result of external than internal developments and, although surely reserved for members of the tribal elite, does not presuppose a strongly developed pyramidal social structure.

10.4.2 Economic Organization

As mentioned before, economic life in the river area was fairly autarkic. A few commodities, such as salt and thephrite (basaltic lava) querns, part of the metal artefacts, and other things such as perhaps crude glass, are known to have been imported. These are surely only the archaeologically best traceable items in a wider range of commodities including perishable materials, so that regional autarky should not be taken to imply a closed economic system. There was external trade with surrounding regions and the distribution of late-La Tène glass bracelets (in particular of Haevernick type 3b) may eventually even provide us with an instrument to measure the degree and direction of such trade. Despite all this, the overall picture is consistent with that of the social organization in that there are no centres for tribute flows where an elite group could be postulated. Luxury imports or indications that specialists were supported by such an elite are also lacking. Perhaps the closest to being specialists are the presumed glass bracelet manufacturers in the region between Waal and Meuse, but there is no need to link these to a centralized or hierarchically organized economic system. What craft production there was must have worked on a small scale at the micro-regional or settlement level and thus with few or no full-time specialists. The latter can, however, only be assumed because there are whole areas of economic activity of which as yet little or nothing is known, such as crafts dealing in perishable materials or in the ‘immaterial’ (religious specialists).

From the indications – barely deserving to be called evidence – that we have, it seems that this situation was only slightly altered in the final decades of the Iron Age. There are no indications that the group of Chatti which settled in the river area drastically changed economic life. But they did come to dominate the region and perhaps even established some sort of central place. This could have been associated with some tribute flow and accumulation of wealth. A connection with the minting of silver coins and with at least one other form of specialization, namely, in military affairs, is compatible with that. Whatever forms and degrees of contact with the Roman state existed at that time, their impact can hardly have led to fundamental changes. In contrast to Caesar’s actions in the northernmost Belgic area, everything we know about subsequent Roman policy along the Rhine is aimed at stabilization, not further disruption. This may perhaps also be deduced from the fact that the possibly fairly intricate system of agricultural production in the river area as developed during the Iron Age seems to have survived undisturbed well into the Roman Period. The nature of this system has already been touched upon in chapter 8, in the discussion of native settlements. Unfortunately, at the moment an analysis of the organization of agricultural production has still to proceed along indirect lines. Notably studies on faunal material and plant remains from river area sites are not yet available. On the other hand, we do have, apart from the excavation data, a detailed insight into the geological conditions which provides a general framework of agricultural possibilities and impossibilities as outlined above (chapter 7.2.2). In addition, there are a consider-
able number of pollen diagrams which yield information about the natural vegetation and human influence on it for the region in general. All through the Iron Age, these diagrams\(^96\) indicate for the Holocene river deposits an increasing spread of non-tree pollen, including *Cerealia* and, most notably towards the end of the period, a strong or even very strong increase of *Compositae* and *Cyperaceae*, as well as high values for *Plantago*. The evidence thus shows rapid and large-scale deforestation, formerly wooded areas (normally the riverine alder-woods: *Alno-Padion*) being transformed into arable but especially into grazing land. On the Pleistocene deposits and notably on the ice-pushed ridges, the oak- and birch-woods (*Quercetae robori-petraeae*) appear to have been only very slightly disturbed during this period.\(^97\) For the coversands area, there are more indications for deforestation in the Iron Age but on a far smaller scale than in the river area proper, with limited clearances for arable and some increase of heath.\(^98\)

In general, we may conclude from this that the high-lying and rather unfertile Pleistocene deposits were relatively sparsely settled and the Holocene river deposits, on the contrary, rather densely populated, with the Pleistocene coversands somewhere in between. The archaeological distribution map of Late-Iron Age settlements (fig. 60) is in complete accordance with this conclusion.\(^99\) The largely uninhabited oak- and birch-woods were probably marginally exploited for fuel and timber, demonstrably for hunting, and perhaps for other purposes.

In contrast, a more complex system developed in the other two regions. For the Pleistocene coversand areas, the palynological data and the archaeological information about settlements and house-types, especially the characteristic two-aisled ‘Haps’ houses, are compatible with mixed farming. If it is assumed that all cattle were stabled in winter, which is not necessarily true, the mod-

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\(^{96}\) Information based on published (e.g. Teunissen 1975, 1980b, 1982, 1984) and several unpublished diagrams kindly provided by Dr Teunissen.

\(^{97}\) Teunissen/Teunissen-Van Oorschot 1980.


\(^{99}\) Note that a number of sites south of the Meuse is not indicated on Appendix I, cf. chapter 8, note 1.

\(^{100}\) See on these subjects IJzereef 1981, 177.

\(^{101}\) See note 98.
stock-breeding is the more likely interpretation. This conclusion is now also supported by the archaeozoological analysis of river area sites by R. Lauwerier. It has shown that the one ‘post-hole swarm’ site included, that of Heteren-Het Lage Land (no. 93), is the only one different from other settlement sites in that the percentage of bones from young cattle is much higher than elsewhere. This would indeed seem to indicate a strong emphasis on stock-breeding, although in view of the lack of stables of a rather extensive kind.

We may assume that such specialization was made possible by the environment, notably the presence of the flood-basins which provided suitable grazing land in the dry season. But the environment is not all that counts. Socio-economic conditions in the region and beyond must also have been such that they allowed a fairly complex trade network because a specialized economy necessarily implies at least to some extent a surplus of one group of commodities and a shortage of another. Moreover, this network must have been reliable over a longer period otherwise the in itself somewhat risky strategy of specialization would never have rooted so deeply and have existed for such a long time from the Middle-Iron Age until the mid-2nd century AD.

On the other hand, the network as such may have been fairly complex but it was probably not very extensive as is shown by the lack of much imports from far afield. This agrees well with the absence of indications for more developed forms of social organization and we can assume, therefore, that the necessary trade was a function of the fairly egalitarian tribal society and not managed by a central authority.

In this context, it is perhaps useful to mention briefly in some respects similar sites from the northern Netherlands, namely, the late-Iron Age walled enclosures discussed by Waterbolk. They were in use between c. 350 BC and c. AD 100, which is roughly comparable to the known period of our post-hole swarms and they lack ‘proper’ houses but still have normal domestic refuse and a number of granaries and structures interpreted as barns. The interpretation is that the enclosures served to stock cattle and harvest products, and perhaps had an additional religious function. The interesting point is that these sites in any case diverged from the norm and had a special purpose, and that this also resulted in habitation without normal houses. In this sense, they are a parallel phenomenon, but such an observation does not imply that the enclosures, which are relatively rare, are comparable to the river area post-hole swarms in all respects. Their fort-like appearance and relatively empty internal surface suggests indeed a different function, which could be that of central storage or stocking place. To interpret these sites as territorial centres controlled by a fairly powerful elite, and to make a functional comparison with hillforts is, however, perhaps somewhat exaggerated. They may indicate some first developments towards such sites, but even their fort-like appearance is in principle perfectly compatible with a function for communal purposes in a still strongly egalitarian society.

To return to the eastern river area, the conclusion must be that when Drusus’ troops arrived there in 12 BC, they found the islands between the rivers rather densely populated, with an economy primarily geared towards extensive stock-breeding, surrounded by largely uninhhabited wooded hills and an only partially exploited Pleistocene sand landscape with scattered agricultural settlements. The population of this area still formed a fairly egalitarian tribal society known as the Batavians after the area they inhabited and had fairly recently come to be dominated by a group of Chattian immigrants. These may have had a centre (or military camp) at a strategic spot in the middle of the delta to which they were manoeuvered, if not actually sent, by Roman authorities with whom they were allied, probably even then under a treaty stipulating freedom from taxes in return for military assistance.

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104 See chapter 1, note 8. I am grateful to R. Lauwerier for discussing with me his provisional results and for his permission to incorporate the information here.

105 Waterbolk 1977. The best examples are Zeijen I,2, Vries 2, Zeijen I,3, Zeijen II,2, and Vries 3. Whether the ‘enclosures’ from Rhee should be included is somewhat doubtful (cf. op. cit., 169).

106 Waterbolk, op. cit. 162; the author prefers, however, a narrower dating of c. 200 BC – AD 50.

107 Which is becoming increasingly clear as more contemporary sites become available: compare Waterbolk 1982.


II ANNEXATION, GROWTH, AND BREAKDOWN. THE RIVER AREA FROM 12 BC–AD 270

II.1 INTRODUCTION

With the arrival of Drusus' troops in the river area a period of nearly three centuries started in which the inhabitants were incorporated into a much larger social and economic organization than before, that of the Roman empire. In this chapter, some of the processes leading to and determining this and following events will be discussed, as well as their effect in the region and the specific consequences as revealed by the archaeologically and historically documented developments. As outlined in chapter 2, these developments should be divided by the analytically highly relevant caesura constituted by the establishment of a formal frontier along the Rhine in the mid-1st century AD. This event had more than a direct political and military significance. As a result, the entire Rhine-Meuse delta became a frontier region which strongly influenced the nature and intensity of social and economic processes in the following two centuries.

Some of the theoretical concepts involved are discussed in paragraph 11.2, together with a discussion of their relevance to Roman expansion. Their relevance to the Dutch river area is examined in the following paragraphs, but it can also be briefly epitomised by three historically known Batavian leaders. The first is Chariovalda, only mentioned in passing by Tacitus and virtually unknown as a person. He exemplifies the chieftain of a client tribe which, as a separate society with its own norms and values, could exist in profitable symbiosis with the Roman state. The second is Julius Civilis, a figure representing the period of stress when tribal society was transformed to fit the changed needs of the empire and also the acceleration of this process caused by native reaction.

The third is Postumus, who typifies the militarized Roman population of the frontier zone which resulted from the developments started in the 1st century. At a time when the peripheral frontier zone, although first to be affected by trouble from beyond the border, had developed into a socio-economic centre, his actions illustrate the magnitude of this change. His Gallic empire, centred on the former periphery, is an attempt to build a new state. And the difference between people trying to maintain a state-level of social organization and those staging a native revolt against their incorporation by the Roman state is considerable.

But the developments of the second half of the 3rd century on the whole took a course leading to disintegration, and the military action and movement of population groups into the frontier zone - itself at least partly caused by previous influence (transfer of information) from or through that zone - provide a second major caesura for our region. It was perhaps, but not necessarily, a more gradual change, but neither history nor archaeology allow us to monitor developments very closely in this period. The different situation in the 4th century and its further consequences will be examined in chapter 12.

II.2 IMPERIALISM AND COLONIALISM

II.2.1 Theoretical Considerations

In recent years, a number of important theoretical studies in history, sociology, and economics have given a new impetus to the use of diffusionist models in archaeology. The explanatory potential of such models had been somewhat discredited as a result of the culture concept in which they were used, although they remained a basic assumption in much work and notably in Roman archaeology were Romanization is a concept of central importance. The interest in the results of work in other social sciences, which itself springs from the need to gain a better understanding of our western colonial past and its present-day consequences, has already provided many new stimuli to the study of late-Iron Age and Roman Period Europe.
This does not mean that there is some sort of generally accepted theory on imperialism, colonialism, and related phenomena. On the contrary, there are many different opinions and interpretations and sometimes there is not even agreement on the precise meaning of theoretical concepts. Therefore, it is useful to examine some of these concepts and thereby to formulate some ideas about the more general processes behind the specific developments within the chronological and spatial coordinates delimiting the present study.

Imperialism and colonialism are dominance/dependency relations between one society and another. The concepts are sometimes used interchangeably, which to some degree they are, but imperialism is normally considered the more general term for the situation in which one society or state controls another and in fact the cause of colonialism, although they are, at the same time, different forms of domination which may coexist. The difference lies mainly in the degree or rather character of this control which, in the case of colonialism, is inevitably direct and involves the movement of people.

There is much controversy about the causes of imperialism. In Marxist theory it is seen as the consequence of capitalism while others also view it as a kind of inevitable development resulting from many incidental circumstances or, conversely, as a deliberate choice. Important as it may be, such a discussion is not directly relevant here. In all cases, the prerequisite for domination is an inequality in the resources of the social systems involved, including their own internal structure. This difference may be expressed in forms of coercion. It is perhaps useful to look for general laws in domination processes, but it is more likely that the specific historical context and the societies— or even certain individuals—involved will have determined the precise causes in each case. All do, however, have at least one other common denominator in addition to coercion, and that is that at least at the point where imperialism turns into colonialism, there must be an interest at stake. The nature of this interest, and whether it simply arose or was deliberately pursued, is more or less irrelevant: it may be economical, political, ideological, all such motives together, or something else. From an analytical (‘etic’) point of view it may even be completely unintelligible, it just has to be important enough. It is easy to see why this should be so, for colonising has its price: it means the expenditure of energy.

This becomes especially clear when we look at the relation between imperialism and colonialism. Imperialism, when not defined as an umbrella concept but the antecedent stage of, or the alternative for, colonialism, also implies coercion. But it is a form of domination which consumes much smaller amounts of energy. It does not involve moving significant numbers of people but it is primarily based on the transfer of information. The distinction between ‘power’ and ‘force’ made by Luttwak will serve to clarify this point.

In his analysis, force is essentially a physical phenomenon which is consumed in application and wanes over distance: ‘...military force is indeed governed by constraints on accumulation, use, transmission, and dispersion akin to the physical laws that condition mechanical force’. Power, on the other hand, works very differently: ‘It works not by causing effects directly, but by eliciting responses— if all goes well, the desired responses’. When you use force to obtain obedience, you use energy, but when you are obeyed because of your power, it is the object of your power who is the actor and therefore the one who uses energy. The power itself is not a physical phenomenon, so it is not consumed by this action. It remains the same, just as—in principle—it does not diminish over distance. But power does not exist unless it is perceived: it is not something at the level of matter or energy, but on that of information.

Of course there is a relation between the two, in that ultimately power is based on force and on its perception and ‘correct’ evaluation, which is obedience. Power does not work when the means of perception are lacking or when the evaluation is ‘wrong’ or, in other words, when prior information about force has been unintelligible or

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9 Fieldhouse, op. cit. distinguishes further between the process whereby a whole population moves to the new territory (colonization in its classical Greek sense) and that involving only an elite to manage the dependent society (colonialism).
10 See, e.g., North’s (1981, 8-9) appeal for more investigation into the underlying expansion-bearing structures in Roman republican society as opposed to debating the issue of aggressive v. defensive Roman imperialism by ancient historians.
12 Luttwak, op. cit., 196-7.
13 For a more encompassing analysis of power, including its manifestation at the individual level, and based on much the same fundamental considerations as discussed here, see Adams 1977.
insufficient. Unless, of course, an error was made or a risk taken. In all these cases force has to be used directly, but it is evident that a careful power-policy requires much less input of energy than the use of force, where the output is proportional to the input.

Luttwak uses this distinction between power and force in his admirable analysis of Roman strategies of imperial security, but it is equally applicable in a distinction between imperialism and colonialism. Imperialism is a patron-client relationship, based on a flow of information which allows a low-cost domination because of the client's perception of the patron's power. Colonialism, on the other hand, is characterized by the use of direct force. It is not just a flow of information but a flow of energy which is decisive in this case. This makes colonialism a relatively expensive enterprise and that is why there has to be some sort of ('emic') necessity or interest at stake for the colonizer. Another consequence of the use of energy is that, in contrast to information, it is finite. Depending on the capacity of the system and the general conditions under which the process takes place, somewhere a limit is reached, which brings us to the concept of the frontier.

It is not helpful to discuss this in a very general way, because there are evidently some major differences between imperialism and colonialism before and after the 15th century when they became intercontinental in scope. When thus limited to ancient Empires, or archaic civilizations in the sense as described by Service, it is possible to apply the ideas concerning the structure of frontiers developed by Lattimore. In an expanding empire, the frontier is dynamic. There is sufficient energy left to control or incorporate new units. In Lattimore's terminology, this would be a 'frontier of inclusion'. In the colonial situation, if a point is reached where an energy-surplus is no longer available, a static frontier develops, the 'frontier of exclusion'.

The reasons for a frontier to become fixed are, of course, more complicated than this and may be found in the natural as much as in the social environment, but in the end energy is the main factor. Even the very important role of innovation is, in principle, only of temporary impor-

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14 Service 1962 [1971], 166–9. See also Thapar 1981, esp. 409–13. It is perhaps useful to stress that the analytical concept of colonialism as employed here should not be understood in its 19th- and early-20th century meaning, which has many connotations that are anachronistic where archaic civilizations are concerned.

15 Lattimore 1962. They were developed on the basis of his studies on Chinese frontiers. Although sometimes disputed when applied to a particular case (see e.g. R. McC. Adam's paper and the comments on it in Moore 1974, 1-20), they fit well into modern theories of comparative frontier studies (see esp. Miller/Steffen (eds.) 1977).


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Centralization under uniform civil administration
Economic integration

The radius of military action is greater than that of civil administration. Following Lattimore’s ideas there is, therefore, an inner zone reaching into territories that can, after conquest, be added to the state and an outer zone into areas that can be invaded for profit or for the purpose of breaking up barbarian concentrations dangerous to the state, but that cannot be permanently annexed. Economic integration has the shortest range, because it is a function of the ability to transport bulk goods at a profit. These analytical zones presumably have some universal validity. They are the result of what, at a higher level of abstraction and remaining at the level of energy and information, can be called a difference in potential between two points, in casu, a core and a periphery. Across the potential, flows may exist to varying degrees, depending on the amount of ‘friction’, defined as the variables which counteract flows, such as technological difficulties. The result at any point in time may be spatially defined and interpreted as in Lattimore’s scheme. This is useful, for example, in examining the degree and direction of energy and information processing and the nature of friction. But as far as imperialism and colonialism are concerned, such a scheme represents only the static side of the coin. When the individual areas within an expanding empire are considered, a dynamic picture emerges, which also shows that both imperialism and colonialism are essentially temporary phenomena. An area at first under imperialist domination by a neighbouring group may then be formally colonized and finally fully integrated although, depending on the constraints posed by the social and natural environment and the capacity to overcome them, this development may halt at any stage.

For the periphery of an empire, this means that a frontier is created along a perimeter defined negatively by those areas that, for whatever reason, cannot be colonized, or positively by the maximum territory brought under direct administrative control (which may lead to a very different outcome). Such a formal frontier is, of course, only one of many boundaries on a transect of control from a core area outwards that could be analytically relevant according to the subject of study. In addition, the kind of formal frontier and the way in which it functions will vary widely depending on the relevant circumstances in different cases. The above-mentioned literature contains various approaches to these problems. Instead of elaborating further what is discussed there it is more appropriate to examine next the processes underlying or leading to developments in the Dutch river area during the Roman Period in the light of what has been said so far. After all, what theoretical issues are relevant or can be fruitfully examined in a particular case is also determined by its specific context and there are particularistic elements which no general theory can fully cover, such as natural catastrophies or the highly significant role of one individual.

11.2 Roman Expansion

There is so much to say, and so much has been said, about Roman imperialism that only a few aspects directly relevant to the present study can be mentioned. These do not include the early development of the Roman state, but rather the influence of that state on surrounding areas leading to its great expansion from the 2nd century BC onwards.

A number of recent studies have provided considerable support for the idea that with the growth of the Roman state the intensification of trade contacts with societies on its northern periphery caused changes there. A process such as described by Nash for central Gaul, with increasing social stratification and wealth concentrated in the hands of successful elites, followed by a need to consolidate this position and thus to create an efficient administrative structure, may indeed have led to a process of secondary state formation there. According to Nash, the role of mercenaries and later the slave trade was of crucial importance. However this may be, the archaeological and historical evidence show that Celtic society was permanently changed from weakly organized tribal groups into centralized and hierarchical polities. Undoubtedly this development was strongly influenced by information about conditions in the Mediterranean, but information flows both ways, and the knowledge of and contacts with the Gaulish area allowed not only imperialistic domination but led to actual annexation of

17 The conceptual framework is derived from that discussed extensively by Van der Leeuw 1983.
18 An example is Hedeager’s (1978) study on Roman–Germanic exchange.
19 See Nash 1978b, Bloemers 1983a, Haselgrove 1984, to name only a few.
territory at an early stage: the creation of the republican province of Transalpine Gaul (later Gallia Narbonensis) between 125 and 121 BC.

More than sixty years elapsed before the next phase of conquest and a change from imperialism to colonialism took place in a northerly direction. This was Caesar's Gallic war, and its designation as Caesar's war is not at all inappropriate. It can be argued that the changes in Celtic society in Central Gaul, brought about by contacts with Rome, had effectively prepared the way for this conquest. But that does not make such a major enterprise the logical next step. After all, client states under imperialist control were still of major importance elsewhere more than a century later. Perhaps it is true that Caesar's personality was the decisive factor. But even then, the conquest as such can still be seen as yet another consequence of peripheral imperialism.

As was recently argued by Bloemers, the sociological concept of peripheral imperialism is eminently suitable for understanding the process of Roman expansion. The idea is that apart from, or even instead of, a deliberate policy formulated by the rulers in the core area, expansion also arises from the activities of their representatives in the periphery, who deal directly with local elites. Support of one party in a conflict, personal ambition, and the like, may then trigger a series of events starting with some actual interference and ending in a true conquest. Roman history has recorded many such situations.

For Gaul, and especially for its northern parts, Caesar's untimely death may have prevented completion of the colonialist incorporation. But, as discussed in chapter 10, it did proceed far enough to allow effective manipulation of events in those northern regions. The limited available data show, however, that this manipulation could not rely simply on a power-policy and that direct interference, the use of (military) force, was necessary. The reasons for this are presumably related to what was said in the previous paragraph about the nature of power, which rests on the perception and correct evaluation of it by the dominated party. The demonstration of Roman force in Caesar's campaigns should have been quite sufficient for that, but at the very least it was apparently not entirely effective. One might attribute this to a case of incorrect evaluation or initially still some lack of information, especially on the part of the trans-Rhenish German tribes, but there may be more to it than this. It can also be argued that, despite all demonstrations, the power of Rome was not perceived although it was surely seen.

The reason for this can be found in the social organization of the German and North Belgic tribes. In a centrally organized hierarchical society it is a paramount chief or the decision-making upper stratum of society who do the relevant perceiving. This institution, in organizational terms a high-level regulator, allows both the perception and - at least in principle - the correct evaluation of power, which is why the institute of client kingdoms was fairly effective in the East. In a tribal organization, however, such a central information processing and control unit is lacking and it can be argued that, for a tribal society as a whole, there are no efficient direct means of perception. In terms of systems theory, it can be said that there is not enough channel capacity to process the information input because the cultural system as a whole lacks a high-level regulator who can act with speed and flexibility. In practical terms, any petty chieftain can take a chance and there is no central authority to hold him back.

One obvious response of a cultural system in its effort to cope with changed circumstances more effectively is organizational change, and that is one way to view the process of secondary state formation in Central Gaul in the last century BC and also the developments in northern Gaul during and after the Gallic war. But there is another side to this, namely, that of the larger organizational structure controlling new units, in our case the Roman empire. For effective control, a multi-level (hierarchical) regulatory system is needed. Given a certain amount of time, as in slow imperialist expansion, this may develop gradually as a response of dominated societies around the perimeter. But in the case of rapid colonialist expansion by conquest, new units cannot be controlled permanently, let alone be incorporated, unless their structure is somehow altered more directly.

Roman interference in the North Belgic area between Caesar and Drusus can also be seen in this light, but other explicit examples are recorded in the classical literature. For example, the Frisians, in Tacitus words, 'had imposed on them a senate, a magistracy, and laws' by


22 See Peebles/Kus 1977, esp. the paragraph on 'organization, energy, information, and ritual', 427–31.
HEGEMONIC EMPIRE

- provincial territory
- client state boundaries
- client tribe boundaries
- combined task forces of legion and auxilia

- disposable and concentrated imperial forces available for wars of conquest and intimidation of clients

TERRITORIAL EMPIRE

- guarded or fortified frontier perimeter
- provincial boundary
- legions and auxiliary units distributed for frontier defense

- all clients annexed or abandoned.
- all imperial forces deployed for frontier deterrence and defense.
- no disposable forces for offense or defense.

imperial forces directly responsible for perimeter defense and internal security.
Corbulo, precisely because they needed to be controlled. The Roman administration even had specialized officials for this task, namely, the military praefecti civitates, whose main purpose was to establish local self-government and the training of local leaders in its methods. The same Frisians had had such an official since their treaty with Drusus until AD 28, when an incompetent office-holder by the name of Olenius caused a successful rebellion and was forced to flee.

All this is in accordance with the long-established view that Roman control over new territories acted through their own pre-existing or newly created administrative structure. But there are further consequences. As Haselgrove recently put it as a central premise of his paper, ‘Rome ... looked to adopt intact whatever of the existing structure she could, and to alter or abolish only those features which ran counter to her long-term interests. The nature of indigenous society was therefore a key factor in Roman expansion, and also in the occasional checks she experienced, as in Germany.’ The last conclusion is very much in accordance with what has been said earlier. A major underlying reason for the ultimate failure to conquer Germany can be seen as the inability of German societies properly to perceive Roman power. Their structure made them the subject of much direct use of military force but on the other hand it prevented that force from having its intended effect. After all, the ability of the empire to apply such force was limited, certainly in view of the magnitude of the task, if not with respect to the capacity for staging campaign after campaign then at least as far as imposed changes of the indigenous social structure were concerned.

From a historical or economic point of view there is more to say about all this, but basically it is considered here the main reason why expansion into Germany stopped and a frontier was established. One may, for example, point to political difficulties or to the ‘environmental conflict’ which arose from the difficulties to find enough food locally to feed the army (see note 140 below). But that situation was not due to a harsh natural environment but to its limited exploitation and thus again to the socio-economic structure of the groups living there. Outside the established frontier, in what Latimore would call the outer zone of military control, force, diplomacy, and other, commercial, contacts were a constant factor. But only inside was there centralization under uniform civil administration. It is in this area that processes of (further) acculturation and integration into the socio-economic system of the empire are triggered.

However, during the 1st century AD, a transition can be observed from what Luttwak describes as a hegemonic empire (with ‘frontiers of inclusion’), towards a territorial empire, with most of the military apparatus deployed along its ‘frontier of exclusion’ (fig. 125). This introduces a vital difference not only between areas within or outside the designated perimeter, but also between those directly at the frontier and the areas more to the interior of the empire. These are not merely closer to the core area and thus more easily incorporated into Latimore’s inner zone of economic integration. There is another difference as well, namely, that after the conquest troops move on and the regions concerned can develop further within the new context. At the frontiers, once established, the situation is different because there troops are stationed permanently, thus perpetuating aspects of the otherwise transitional colonial situation.

When almost the total instrument of imperial force is located in the frontier regions, it is clear that these have a disproportionately large infrastructure imposed on them. At the northwestern frontier of the Roman empire it is this factor in particular which may initially have prevented and later led to a very different kind of integration. This is a subject which can be examined further with data from the eastern river area, but it is important to see what the reasons for this assumption are. These have to do with the way in which the socio-eco-

23 Tacitus, Ann. XI, 19.
24 See Bogaers 1960–61, 268 and note 21 with further references.
25 Tacitus, Ann. IV, 72. See also chapter 8, 263, and note 149.
26 Haselgrove 1984, 6.
27 Which did result eventually in permanent changes there. In principle, this is also a process which can be seen as one of the necessary conditions for the developments in the 3rd century and later.
28 Distance from the core, when measured by the cost of transport, is less relevant on a river frontier connected by excellent waterways to the hinterland, and environmental problems are virtually irrelevant here.
29 See below, paragraph 11.4.2.
nomic empire system functioned. As a basic principle, this can only be seen as exploitative, founded on a chain of dominance/dependency relations comprising taxation and unequal exchange in commodities and centred on the core area. A model of this exploitative system is represented in fig. 126. Although there are some technical deficiencies, the model as such is adequate as a description of principle. In practice, however, it should be realised that such a model only works within Lattimore’s inner zone of economic integration. Hingley disposed of this problem by demonstrating the possibility of energy transfer over long distances by various means. Whether this is entirely justified remains to be shown but even if it is there are three considerations not taken into account. First, exploitation as such need not only be translated in terms of exchange or taxation. There can also be another vital interest at stake, namely, security, and an area may be primarily exploited for its potential in this respect. Second, imperial expansion is a complicated process and, as entailed by the ideas of peripheral imperialism discussed above, this may lead to expansion beyond effective limits from an exploitative point of view. This, however, can only be a temporary situation which will eventually, or very quickly, be amended.

30 See Hingley 1982 for a discussion.
31 As noted by Hingley, op. cit., Appendix 3, it does not take into account the bypassing of notably the provincial level, where not all exchange is channelled through the provincial centre.
32 Op. cit., 22. In addition to the African and Sicilian sea-transports of corn to Rome, which are not to the point because these areas were, by its definition (above, p. 380), in the inner zone of economic integration, trade in slaves and precious metals are mentioned.
33 See, e.g., Luttwak’s penetrating analysis of the strategical value of Trajan’s conquest of Dacia (1976, 97–104).
34 General Corbulo’s recall by Emperor Claudius is an example, although not a good one. The enterprise can be seen as a case of peripheral imperialism with independent action by a representative of the core on the periphery, but in this case there are good reasons to doubt that (cf. below, p. 390).
More important structurally is a third factor, namely, the basic character of the dominance/dependency relationship which allows the exploitation. This is based on power and, ultimately, on force. Originally, these were applied outwards from the core. This remains true for power, which is the essential element of the empire system as such. But the change from a hegemonic to a territorially empire implies that the instrument of force, the army, is no longer located in or around the core but at the periphery. For a truly general model, in terms of energy flows, this does not make all that much difference, because it can be argued that such flows still pass through the core and from there to the periphery, notably in the form of coinage. But for a model such as in fig. 126 it implies that a substantial part of the raw/basic goods does not pass up the pyramid but goes more or less directly to the periphery, or stays there.

From these arguments it may be concluded that the outer provinces, while having administrative relations with the core (which include taxation), are not necessarily also integrated in the imperial economic system. This brings us to the much debated question of the economic dependency of the northern frontier provinces: were they exploited, or was there self-sufficiency or indeed support from outside? An attempt to contribute to the answer with data from the Dutch river area is included below, but it is perhaps not very plausible that a general answer exists because the actual situation may vary widely between different areas and in different periods. This despite the fact that there are quite convincing arguments for the view that the budget of the empire as a whole showed a surplus raised in the provinces of the Mediterranean and spent in the northwestern provinces where most of the army was located.

In principle, these findings are thus in agreement with theoretical expectations and they can also be illustrated by literary evidence such as Strabo and Appian writing about Britain or, for our area, Tacitus' remark about the army in Germany, which Galliarum opibus subnixus, was backed by the resources of Gaul. But all this does not, as most writers on the subject have observed, conflict with a generally accepted basic policy of having provinces at least pay for themselves. As has also been observed, this premise is not directly relevant for any factual situation. But provinces are different, for example in environmental potential, and troops were never evenly distributed around the frontiers. Therefore, the situation in each area and possible changes during the successive stages of the Roman occupation need to be examined separately.

This is easier said than done, as is demonstrated by two recent studies about the burden represented by the army in Britannia, published in the same volume and arriving at diametrically opposed conclusions. These papers also illustrate the danger of simplistic reasoning based on theoretical crop yields to establish a kind of provincial carrying capacity. The value of such data for calculations is disputable, and they need to be studied in perspective. That is, taking into account variables such as the pre-Roman situation and all manner of change, notably in the size of the army, the volume and direction of trade, or the internal organization of the market system.

These are factors to be examined below with respect to the Dutch river area, even though is it just a small region and only part of an in itself already small province. It is, however, a section of the zone directly behind the frontier of exclusion constituted by the limes and, within the framework of particularities of historical events and re-
gional characteristics, some consequences of the general perspective outlined above should emerge.

11.3 FROM DRUSUS TO CORBULO

11.3.1 Imperial Policy and the Formation of the Civitas Batavorum

From what was said in chapter 10 and the concepts introduced in the previous paragraph, the events in and around the river area before 12 BC can now be described as the result of an imperialist policy of a hegemonic empire securing its outer reaches of influence. These had been greatly expanded by a war of conquest which was most successful in those areas where previous energy/information flows had created internal socio-economic change. Indeed, it can be argued that Caesar’s campaigns were in many respects a case of peripheral imperialism made possible by those antecedent processes. Apart from historical peculiarities and obvious difficulties on the levels of matter and energy, the lack of previous information flow can be seen as one of the limiting factors determining the outcome of the Gallic wars in terms of expansion to the north. The following decades were characterised by the effort to control the new northern periphery by the intermittent use of force but primarily by the manipulation of client tribes. This is normal policy for a hegemonic empire, and precious little information has been recorded about it.

Not surprisingly, this is very different for the next phase with large-scale military campaigns designed to expand the empire even further. Whatever else there is to say about this enterprise, one thing is very clear: it was a major effort planned and executed by those at the apex of the imperial core itself, who must therefore have felt that there was a necessity or interest at stake to justify the enormous expenditure of energy. It was surely not a relatively autonomous kind of operation triggered and steered by processes of peripheral imperialism, although this interpretation does not preclude that such processes played no part, and neither does it implicitly entail support for a view which sees Roman imperialism as aggressive instead of accidental. The causes of the Augustan campaigns of expansion in the north can be and have been studied from many perspectives and an overview is rather beyond the scope of the present discussion. They range from pure imperialist aggression to such factors as the possibility of employing those legions still under arms after the end of the civil war, the grave measure of geographical error which caused the necessary effort involved to be seriously underestimated, and other more circumstantial causes. If anything, as appears from classical as well as modern literature on the subject, the problem is too complex to be attributed to any single cause. The evident availability of an energy-surplus is only a necessary, not a sufficient condition, although it may be both once the surplus is converted into a large standing army.

The history of the numerous political and military events is rather well known and need not be extensively repeated here. Instead, it is more appropriate to examine briefly only the major events directly related to the river area as well as the information and deductions about the position of the region in a wider context. Augustus himself stayed in Gaul from 16–13 BC, obviously to reorganize personally the hinterland for the planned campaigns into Germany. This involved a new provincial organization of Gaul and moving the legions into the assembly area along the Rhine. The following census, in 12 BC, was directly related to this administrative ordering and a precondition for regular taxation. In the new ordering, the Rhine zone itself seems to have been set apart, presumably to become part of the planned province of Germany. As things turned out, this resulted in two military districts which only under Domitian, between AD 83 and 90, were formally converted into provinces.

44 See, e.g., Timpe 1975, esp. 141 f., and Bloemers 1983a, ii.
45 See Garnsey/Whittaker 1978, chapter 1; see also note 10 above.
47 Arguments from A. Klotz expanded by P.A. Brunt, followed by others (e.g., Luttwak 1976, 50 and note 158, Wells 1972, 6).
49 Cf. Von Petrikovits, op. cit., 54. In contrast to most other writers on the subject, Drinkwater (1983, 20–1 and 95) proposes an earlier date (27 BC) but his evidence is not very convincing.
50 Rüger, op. cit., 14–5, 19.
51 Rüger, op. cit., 50.
52 E.g., Bogaers 1960–61, 265 and Rüger, op. cit., 16–31, both with older literature.
The historical evidence for the role of the river area in the first offensive under Drusus, which started in 12 BC, has already been mentioned in chapters 2 and 3. The operational nucleus of his army consisted of five or six legions supported by at least three times as many auxiliary troops. These consisted of various kinds of Gaulish units brought to the assembly areas, but also of locally recruited irregular units (tumultuarii, Volksaufgebot). Although this is not explicitly mentioned, it can be deduced that Batavians from the river area also provided such units for Drusus' army. The same may well have been the case when new operations in Germany were necessary and Roman troops under Tiberius in AD 4–5 again assembled in the river area. This was a successful operation and P. Quinctilius Varus, who took over the command, was apparently given the task of converting Germany into a province, completing Drusus' prior efforts. After his defeat in AD 9 new campaigns for the third time brought an expedition army to the river area in AD 15–16. In this campaign, under Germanicus, Batavian participation in the large army is explicitly recorded.

Little is said in the literary sources about conditions in the river area except various geographical and hydrographical details. From what is revealed, it may be concluded that the Batavian area was initially very much the domain of a client tribe, with its own military organization and bound by a treaty to the hegemonic Roman empire. The inhabitants were not subjected to taxation and, although the intermittent presence of large numbers of troops and the drain of manpower caused by the German campaigns surely had an effect, economic life must have continued very much as before.

The same can presumably be said about the social organization, although the Roman policy of working through the native social structure must have reinforced it and notably the position of the tribal elite. In addition, there are sure indications for a transformation of the tribe and its territory to fit the needs of an administrative organization designed to encompass the new province of Germania Magna. In other words: the establishing of the civitas model. One of these indications is the gradual granting of Roman citizenship to the native nobility mentioned earlier. Active interference such as the stationing of a praefectus is not recorded, but there are other clues. Very important is the evidence for centralization which, as discussed in chapter 10, is lacking for the pre-conquest phase. Even apart from the archaeological data the literary evidence, which has recorded the names of Batavodurum and Oppidum Batavorum, shows that such centralization did take place. Another valuable clue is the epigraphical evidence for the office of summus magistratus (fig. 127), datable to the first half of the 1st century AD.

Equally important as centralization is the changing nature of the Batavian troops, which can be deduced from the relatively numerous literary data. This has been done by Callies, who demonstrates that by the end of the '60s the Batavian troops had already to a very large degree become regular auxiliaries. The final departure

53 Von Petrikovits, op. cit., 55.
54 E.g., Alföldy 1968, 78, Furger-Gunti 1981.
56 There is literary evidence for units provided by the Nervii (Livius, Periocha CXL I; see Callies, op. cit., 142) and even by the Frisii (Cassius Dio LIV 32, 3; see Byvanck 1943, 92, Callies, op. cit., 143, note 58) and for an even earlier recruiting of Batavians for the imperial bodyguard (Bellen 1981). Moreover, there is ample evidence that such irregular units were quite common in this period and before (cf. note 55), there are Tacitus' references to the treaty with the Batavians and there is explicit evidence for Batavians only somewhat later (see note 58).
57 See Rüger 1968, 20, with further references.
58 Tacitus, Ann. II, 8 and 11. The army consisted of 12,000 legionary soldiers, 26 sociae cohortes and 8 equitem alae (Ann. I, 49) in AD 14 while in AD 15 (Ann. I, 56) from Germania Inferior four legions are mentioned with 5000 auxiliaries (10 cohortes and alae?) and a number of tumultuarii; and in AD 16 (Ann. II, 16) eight legions, two praetorians cohorts, and numerous auxiliary troops.
59 See Bogaers 1960–61 and chapter 3.
60 Chapter 2, 24. See also e.g. Vittinghoff 1976, 74. Men such as Chariovalda, leading the Batavian tumultuarii under Germanicus, and Flavius, the supreme magistrate, still lacked citizenship but Julius Civilis, for example, may have been granted citizenship when he was recruited by Gaius (see Bogaers 1955, 190, Hassall 1970, 133, Bloemers 1978a, 183) and others, such as Claudius Labeo and Claudius Victor, may have received it from Claudius.
61 See chapter 8, 236.
On the basis of what was said in chapter 10, it can also be concluded that a fairly quick transformation of socio-political units south of the Rhine may well have been a logical step, if not a necessity, in the situation after the officially denied but, in reality, evident failure to annex Germany and Tiberius’ termination of Germanicus’ efforts in AD 16. The native organization in these regions – whether or not one is prepared to accept the degree of Roman engineering proposed earlier is not even relevant – was not a balanced and long-established one but quite recently disrupted and restructured. A similar conclusion has been proposed by Vittinghoff, who argues that: *Anders als bei intakten, historisch gewachsenen Stammseinheiten, die schon lange in ihrem Gebiet seßhaft waren und in überkommenen soziopolitischen Ordnungen lebten, waren hier die verantwortlichen römischen Machthaber, losgelöst von solchem vorgegebenen Rahmenbedingungen, weitgehend frei in ihrem Handeln.* In an earlier paper the same author argues that such early self-governing *civitates* may have been necessary because the military apparatus as such was not equipped for the administrative burden. Hence his conclusion that the formal status of Germania Inferior (and Superior) as a military district does not at all imply that these *civitates* should therefore also be considered military territory. There is no evidence of a large-scale military presence in the river area after AD 16, and of the eight legions in Germany the four stationed in Germania Inferior were encamped upstream, the two nearest to the delta only in Xanten. It must be concluded that the troops in the

from the *antiqua societas* took place in AD 69, when a *dilectus* was instituted, the drafting of regular auxiliaries. However, as Alföldy convincingly argues, Batavian units still operating in the old style under their *dux* in Germanicus’ time probably were already regular auxiliary troops in AD 43 at the latest, when they were posted to Britannia with the 14th legion to which they were attached. This surely implies a changed relationship as yet another symptom of the administrative transformation taking place during the half-century preceding the Batavian revolt.

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64 Tacitus, *Germ.* 29 uses the term but in a different context, from which the usual deduction (e.g. Bogaers 1960–61, 265 and note 8) is that the conditions from before the revolt were reinstated. There is no need to question this, although the notion that it was a favourable event should not be readily accepted. See also p. 402–3 below.
66 Alföldy 1968, 45–8. For a summary of various opinions, see Bloemers 1978a, 82–3. The possibility that Batavian cohorts were only sent to Britain in AD 61 (Stein 1932, 166) has been persuasively disposed of by Hassall 1970.
67 Note that this last major campaign contains several elements of peripheral imperialism again, if one is prepared to see Germanicus’ ambitions in this light (see Timpe 1968).
68 Vittinghoff 1976, 75.
69 Vittinghoff 1974. See also chapter 6, 191 and note 261.
71 For an overview, see Gechter 1979, esp. 123–8, figs. 56–7 and *Tabelle* 14.
river area were auxiliaries or some *tumultuarii*, or both, most of them conceivably Batavians because we know of the existence of one *ala Batavorum* and nine Batavian cohorts, eight of which were sent to Britannia in AD 43.\(^1\) But before this, the troops were probably involved in the campaign against the Frisians in AD 28;\(^1\) Batavians were certainly involved in Caligula’s aborted campaign of AD 39–40\(^2\) and perhaps also in the war against the Chauci in AD 41.\(^3\)

As will be discussed below, the number of Batavian soldiers is in itself also an important source for further inferences. Unfortunately, although there is little doubt about their nature, the size of the Batavian units is not entirely certain. The total number of Batavian units in Claudian times, and probably even earlier, is eleven. These were nine cohorts\(^4\) (because of their demand that the cavalry element be increased\(^5\) these nine can be identified as part-mounted *cohors equitatae*), one cavalry unit (*ala Batavorum*),\(^6\) and the often ignored but quite important elite troop of the *numerus Batavorum,*\(^7\) the emperor’s bodyguard. Regrettably, no direct clues are available as to the strength of all these units, because it is evidently important to know whether they were *quingenariae* or *miliariae.* For the cohorts, the most plausible indirect clue is surely that forwarded by Alföldy, who used the known strength of the post-AD 70 Batavian auxiliaries as evidence. At that time, we know of a *cohors I quingenaria Batavorum* and the *cohortes I, II, III,* and *IX miliariae Batavorum.* Alföldy argues that these were not troops newly raised after the revolt, but the reorganized rebel troops,\(^8\) which therefore must have been quingenary cohorts to keep the total number of men at approximately 4500. In addition, there is the general evidence assembled by Birley which shows that milliary units normally came into being only from the Flavian period onwards, in part through the doubling of existing quingenary units and in part through new creations.\(^9\)

As far as the *ala* is concerned, this same evidence should also imply that it was not of double strength.\(^10\) However, a reasoning similar to that for the cohorts, with a total number of soldiers remaining approximately the same, could lead to the conclusion that it was milliary because the newly formed *ala* after the revolt was the *ala I milliaria Batavorum.*\(^11\) Of course, it is entirely conceivable that new troops were in fact raised or that the *ala* only became milliary in the 2nd century. But there is another interesting option, because it is conceivable that the new *ala* was formed from the existing one together with the Batavian *corporis custodes,* which were definitely mounted,\(^12\) and had been disbanded and sent home by Galba.\(^13\)

This would mean that the *numerus Batavorum* in Rome was of cohort strength at least. But this designation is only one of a number of names by which the German

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\(^1\) Sprey 1953, 27; Callies 1964, 147; Alföldy 1968, 46; Hassall 1970.

\(^2\) Tacitus, *Ann.* IV, 78. See also Alföldy 1968, 139.

\(^3\) Suetonius, *Gaius* 43 and 45: when not as auxiliaries, then at least as the main body of his *corporis custodes,* Bellen 1981. The sometimes disputed visit of Caligula to the river area (see Bloemers 1978a, 77) is supported by an inscription on a wine-barrel from the first fort at Valkenburg which is likely to have belonged to the emperor’s personal supply: C(aius) CAE(sar) AUG(ustus) GER(manicus). See Glasbergen/Groenman-van Waateringe 1974, 37, sub 5 and plate 11, and Haalebos 1977, 284. This same conclusion was also forwarded by J.E. Bogaers: *Wat staat er op het vat van Velsen,* *Westerheem* 27, 1978, 8–12 (esp. p. 10–11).

\(^4\) Cassius Dio LX 8, 7; Suetonius, *Claudius* 24, 3.

\(^5\) Tacitus, *Hist.* IV, 19.

\(^6\) Alföldy 1968, 13–4. There may, in fact, have been even more Batavian cavalry. This is suggested (cf. Sprey 1952, 85; Bogaers 1955, 190) by the fact that among the picked horsemen of the *ala Singularium,* under the command of Julius Briganti- cus (Tacitus, *Hist.* IV, 70), there may have been many Bata-

vians. There is also evidence for Batavians in yet other units, such as the *eques* Flavus, son of Blandus, who was probably enlisted before AD 70 and served in the *ala Frontoniana* (cf. Alföldy 1968, 39 and 190, no. 75 = ER III, 241, no. 1207 A).

\(^7\) Suetonius, *Gaius* 43; in *Augustus* 49, 1 he refers to the same unit as *manus Germanorum*.

\(^8\) Additional evidence for this simple and elegant view could be provided by the fact that Agricola’s Batavian cohorts at Mons Graupius were probably veterans, as is suggested by the phrase *vetustas militiae* applied to them (Tacitus, *Agricola* 36). For a new evaluation by K. Dietz, see note 189.

\(^9\) Birley 1966, 58.

\(^10\) Alföldy (op. cit., 141) therefore implicitly considers the pre-Flavian Batavian *ala* as quingenary, although he shies away from stating this directly (op. cit., 13–4). It is also implicit in Von Petrikovits 1978, 238.

\(^11\) This is, at least, the universal opinion of writers on the subject although the earliest dated record for the unit is a Dacian diploma (*CIL* XVI, 108) from AD 158.

\(^12\) Cf. Cassius Dio LV 24, 7, who described them as ‘picked horsemen’. See esp. Speidel 1984, 38–9 for further evidence.

\(^13\) Suetonius, *Galba* 12, 2.
bodyguard of the Julio-Claudian emperors is known. It is even referred to as *cohors Germanorum,* for which Bellen has proposed a strength between 500 and 1000 men and which consisted for two-thirds of Batavians. These figures are probably realistic, which means that a number of some 500 Batavians is quite well possible. In addition, if other troops were only reorganized after the rebellion it is inconceivable that the elite troops, which according to Bellen and Speidel were carefully chosen for the emperor’s horseguards, should have been wasted by not re-enlisting them.

From the above considerations, it appears that the most realistic estimate of the total number of Batavians in the Roman army, at least from Gaius to Nero and quite possibly considerably earlier, was some 5500 men. Although it might be reduced by a few hundred, this figure is in principle also the minimum estimate. It does not even take into account Batavians serving in other units (cf. note 78). When an *ala* or even cohorts of double strength are assumed, total numbers would reach 10,500. As will be shown below, this is also highly unlikely for other reasons.

From what has been said so far, it is evident that the population of the river area must have been under considerable pressure before the middle of the 1st century, both in terms of the demands made upon them for military support and in terms of the imposed or manipulated changes in socio-political organization. From a supra-regional point of view the strategic if not already economic importance of the region also increased greatly during this period, due to its geographical location. An example is the trade between Gaul and Britain; there is evidence that the Rhine route drew off trade which had previously passed through more southerly channels.

But the region as such, although at the actual perimeter of the zone of direct control after the Frisian rebellion of AD 28, still remained a zone along the frontier of inclusion of an expanding, hegemonic empire.

That this is not merely an abstract analytical judgement but that it also indeed correctly evaluates what was in the minds of people at the time is shown by Corbulo’s expedition against the Frisii and Chauci in AD 47. This enterprise may well have been partly caused by the general’s personal ambitions and thus can be described as a case of peripheral imperialism. But, as argued convincingly by Van Es, it is inconceivable that, even while acting on his own initiative, the newly appointed commander of the army of Lower Germany would on purpose and knowingly have acted against the emperor’s explicit wishes. Therefore his recall, sweetened by the bestowal of the insignia of a triumph, can only be taken as the result of a conscious change of policy from the decision-making group in the core area, the emperor and his advisors.

This decision, which was never reversed, led to a concentration of troops south of the Rhine and infrastructural measures such as the digging of the Fossa Corbulonis and triggered the establishment of a chain of frontier forts by adding to and improving the already available dispersed installations. This can properly be seen as one of a series of events marking the change from a hegemonic into a territorial empire and the final institution of a frontier of exclusion, the area beyond which became the outer zone of military (and diplomatic!) action. Inside, in the river area, the increased activities and the visible, permanent presence of troops as well as other measures which we can only guess at, designed to complete the process of welding the native population into a

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87 Suetonius, *Galba* 12, 2. However, as Speidel (1984) has shown this designation probably means nothing and Bellen’s arguments concerning the military organization and title of the unit are not correct.
88 Bellen, *op. cit.*, 53 and 36, respectively (the remainder consisted largely of Ubii).
89 Cf. Speidel 1984, 31. The ‘military’ designation as *cohors,* which he does not accept (cf. note 87), is not a vital argument for the estimate of the strength of the guards.
90 In contrast to all other units, for which this can only be assumed, it is reported that the horseguards were in fact dissolved (cf. note 85).
92 See p. 397. The regional evidence thus also indicates that the conclusion about reorganization of the pre-Flavian units instead of new drafting for the troops formed after the revolt is also quite probably correct. Otherwise the number of recruitable men in early-Flavian times must have been almost twice as high as need be assumed when all rebels were re-enlisted and only casualties replaced.
93 Cf. Cunliffe 1984; see also chapter 10.
95 Van Es 1981, 35.
96 Cf. chapter 2, 25–6. See also below, 403–4.
true Roman *civitas*, further accelerated this development. But before exploring this further, it is necessary to examine the evidence from the river area itself in relation to the general processes and historical developments outlined so far.

11.3.2 The River Area in the Early-Roman Period

Within the framework outlined above and on the basis of the data and inferences presented in paragraph 8.3, a number of issues can now be further investigated. These refer in the first place to changes in the region brought about by the phase of direct contact initiated by (the preparations for) Drusus’ campaigns and leading to its factual annexation at least after AD 47.

Influence of the army

Only comparatively little direct evidence is yet available concerning the considerable military infrastructure necessary for these and subsequent campaigns. For the famous waterworks, the mole and the fosse, for which such direct evidence can hardly be expected, indirect clues have nevertheless provided certainty, respectively convincing arguments, regarding their presence and location as discussed in chapters 3 and 7.2. The camps, however, and especially the earliest ones, remain elusive. Evidence of occupation by Drusus’ troops of the early camps indicated on fig. 19 (see also fig. 128) is, for those in the river area, only available for Nijmegen, and then only from site 412 (Nijmegen-Hunerberg) when 12 BC is concerned. The first occupation of site 417 (Kops Plateau) is, however, not much later and, on the present evidence, must be dated to c. 10 BC. In addition, some kind of camp, however temporary, must have existed near the Bijlandse Waard (the hypothetical site 183, Lobith-Tolkamer) where the mole was built. The site is simply too far away from any known Drusian camp and the presence of the mole is a certainty, and therefore this conclusion is obvious and inevitable even in the absence of further archaeological proof.

A similar line of reasoning can lead to acceptance of a fourth Drusian camp in the eastern river area, somewhere near the fork of Lower Rhine and IJssel. But in this case the reason for its presence, the fosse, is not a certainty but a probability, though a very strong one for which convincing arguments have been provided in chapters 3 and 7.2. The camp has, however, not been found although, as outlined in chapter 9, it is entirely possible that there is still a phase preceding the occupation of Period 1 in Arnhem-Meinerswijk (site 126). But there is not a shred of evidence at present to include site 126 among the Drusian camps.

Fortunately, we can be quite confident that Meinerswijk did figure as a camp in Germanicus’ time and, although excavations are needed to confirm it, the same is probably true for Driel (site 117/8). The pottery from Meinerswijk 1 and the available earliest pottery from Driel so far do not reach further back than the 2nd decade AD. On the western fringes of Batavian territory, however, the well-known camp in Vechten is surely older and, although it is improbable that the site was occupied in Drusus’ time on the basis of currently available evidence, the finds indicate occupation at least contemporaneous with Tiberius’ campaign in AD 4–5.

Apart from the camps along the Lippe, three other sites beyond the Rhine should be mentioned. The first is the small base with extensive harbour works at Velsen in Frisian territory, which was occupied from c. AD 15–30. The second is the as yet difficult to interpret, presumably temporary, installation in Bentumersiel at the mouth of the Ems which may well be datable to approximately the same period and a third might be indicated by a few finds from Winsum (not on fig. 128).

97 Other occupied sites were Xanten (Vetera I) and Moers-Asberg; along the river Lippe the huge camp of Oberaden and the adjacent establishment of Beckinghausen should also be mentioned (cf. Von Schnurbein 1981, esp. 20–1).
98 Cf. Bloemers 1979a, 27; Bogaers/Haalebos 1975, 139. It is not certain whether this pottery dates the large two-legion camp which may be somewhat later but in any case the site as such must have been occupied for some time between c. 15–10 BC.
99 Bogaers/Haalebos 1975. Gechter’s (1979) proposal of 12 BC is rather early on the basis of the available evidence, but not entirely impossible.
100 Although forwarded by Bogaers/Rüger 1974, 90 it is, curiously enough, neglected on such recent maps as Gechter 1979, Abb. 54 or Von Schnurbein 1981, Abb. 6 which are on a small enough scale to allow its representation without difficulty.
101 Cf. Haalebos 1976, 200–2. The limited excavations are not, however, sufficient to conclude that an earlier phase is definitely lacking (see the overview in English by Wells 1972, 101–11).
102 The most recent overview is by Von Schnurbein 1981.
Fig. 128 Early-Roman military camps. Open squares: camps used during (preparations for) the first campaigns under Drusus, and usually also later, black squares: later Augustan-Tiberian camps, shaded: major supply-routes to the assembly areas on the Rhine and routes for the offensives. The level of investigation of the sites varies. Sites in the rear, where the interpretation is the most problematical, are only indicated when there is evidence for direct military involvement. 1 Bentumersiel, 2 Velsen, 3 Holsterhausen, 4 Haltern, 5 Oberaden, 6 Anreppen, 7 Vechten, 8 Driel, 9 Meinerswijk, 10 Nijmegen, 11 Altkalkar, 12 Xanten, 13 Moers-Asberg, 14 Neuss, 15 Köln, 16 Bonn, 17 Aldernach, 18 Urmitz, 19 Koblenz, 20 Bingen, 21 Mainz, 22 Wiesbaden, 23 Frankfurt-Höchst, 24 Friedberg, 25 Bad Nauheim, 26 Rödgen, 27 Trier, 28 Titelberg, 29 Tongeren, 30 Liberchies, 31 Elewijt, 32 Velzeke, 33 Tournai.

View of the supra-regional situation with respect to the river area is presented in fig. 128, which is derived from several recent overviews.\textsuperscript{105}

While all sites can be regarded as military camps it should be added, however, that only for those on and beyond the Rhine can the stationing of troops be taken for granted.\textsuperscript{106} For those in the rear, which are often not yet precisely datable, this is more problematical. As far as they are not Drusian or pre-Drusian camps associated with the moving of troops into assembly areas along the Rhine, they should presumably be interpreted as supply bases, in the words of Mertens ‘run by and for the army, and probably laid out by military engineers’.\textsuperscript{107}

From a supra-regional perspective, it is clear that the bases at Mainz, Xanten, and Nijmegen stand at the head of three main invasion routes along the Lower Main, the Lippe, and the Drusian fosse (IJssel), and in their turn are connected by lines of supply bases directed towards Mainz and Neuss. But this is no more than the basic concept which rapidly evolved in various ways, with Köln soon replacing Neuss as the main focus for supply lines,\textsuperscript{108} and additional invasion routes, such as that along Vechten (if not there from the beginning) and Velser in the north. The dispersal of troops after Tiberius finished his brother’s work in 7 BC and the construction of a large fort – obviously intended to last – in Haltern perhaps in the same year,\textsuperscript{109} testify to the intended annexation of Germania Magna terminated in AD 9.

With regard to the impact of the various structural measures and the intermittent presence of large numbers of troops on the society in the river area, a number of deductions can be made. As a very basic fact, we know that the landscape itself was changed, but not only because of the waterworks. The period of the large campaigns until the 2nd decade AD was characterized by large-scale clearances and arable farming, as evidenced by the very precisely dated pollen diagram from Nijmegen.\textsuperscript{110} Although this deforestation also served to provide wood and there are other examples of materials used, such as the harvesting of heather used in the Meinerswijk camp, this is a very important discovery. It proves beyond any doubt that at least a serious effort was made to provide food locally. It is virtually certain that the diagram in question has no regional significance, but that does not alter the conclusion. It is, in fact, what was to be expected because it is hardly conceivable that the existing agricultural regime could be changed so abruptly and geared towards corn growing as happened from the very start of the occupation at the Kops Plateau. Perhaps even more important: it is extremely unlikely that the Roman general staff would have wished to force their Batavian allies to do such a thing. In this respect it is useful to repeat what was mentioned earlier, that the early-Roman camps in Nijmegen were established at a location – on the Pleistocene ice-pushed ridge – which had some military advantages and which was virtually uninhabited. Seen in the present perspective, the latter fact may well have been of some importance.

As far as the available data permit us to reach a conclusion, the general pattern of agriculture, which was earlier found to have been strongly directed towards cattle-

\textsuperscript{105} Especially Gechter 1979, Von Schnurbein 1981, and Mertens 1983. For further literature and details see there.

\textsuperscript{106} With the restriction that at Bentumersiel a true camp has not yet been convincingly demonstrated and in Driel no excavations could be carried out so far.

\textsuperscript{107} Mertens 1983, 163. The sites have ditches; sometimes an earth-and-timber rampart has been discovered.

\textsuperscript{108} Cf., e.g., Rüger 1984, 16. Also, more direct routes to Xanten and Nijmegen along the Meuse are entirely conceivably.

\textsuperscript{109} Von Schnurbein 1981, 43–4, 94; a slightly later date is possible.

\textsuperscript{110} Teunissen/Teunissen-van Oorschot 1980. It is worth noting that, as discussed in paragraph 7.2.2, the Kops Plateau is in principle not very suitable for agriculture of any kind. The fact that it was nevertheless used as such is thus an important conclusion in its own right.
breeding throughout the Late-Iron Age, changed little or not at all during the phase under discussion. In fact, the diagram provides strong support for this general conclusion because shortly after c. AD 10, with the possible start of a second major occupation phase, the indications for cereal cultivation are reduced as abruptly as they started but are not replaced by those of a growing forest. Instead, there is clear evidence for extensive cattle-breeding.

Bogaers and Haalebos took this as a further indication for their – unfounded – suggestion that the site could have become a civilian settlement. The finds do not offer any basis for such a conclusion although the stratigraphical and pollen-analytical data surely point to a change after c. AD 10. Until further excavations are carried out, it is therefore more logical to assume a changed nature of the troops stationed at the Kops Plateau; these were at first probably (at least partly) legionary as is evidenced by the graffiti with mainly Latin names and even one indication for a legion, namely, the Legio I G. The growing of corn in connection with such troops is perfectly understandable, indeed precisely what was to be expected. Their departure or, as Bogaers suspects, destruction under Varus in AD 9 should have left the site to auxiliaries or, most likely, to Batavian allies who would surely not have been provisioned from elsewhere with basic foodstuffs and, if anything, have practised agriculture in their own traditional way. Although there is, except for chronology, nothing to substantiate this, it is entirely possible to envisage Chariovalda’s herds grazing on the Kops Plateau on the eve of Germanicus’ new campaign while he and his troops assembled there.

Irresponsible as it may be from a historical point of view, such an interpretation fits the known facts about the Kops Plateau, about the nature of society in the river area, and about the use of irregular units from the region by the Roman army. It is even consistent with the model outlined by Nash for the pre-conquest Belgic groups as warrior societies having an economy with an especially well-developed pastoral sector. Late-Iron Age society in the river area, the periphery of this Belgic core area, would seem to fit this picture rather well and the arrival of Chattian immigrants and resultant development into a people repeatedly and explicitly described by classical authors as particularly qualified in warfare will hardly have changed conditions. As Nash points out, what she terms a ‘warrior economy’ affects a society’s use of territory, because pressing military demands upon men of productive age during peak seasons of the agricultural year means that less labour-intensive but territorially more demanding pastoral activities often increase at the expense of intensive arable farming. In addition, she sees the accumulation of herds not just as a subsistence strategy ‘but as an important form of wealth necessary for articulating contractual relations among compatriots’.

Such a line of reasoning is entirely plausible and from the known demands on the Batavians, attached to the Roman armies as tumultuarii in no small numbers, it is probable that their economic system, at least during the first decades of Roman presence, was further strengthened in its existing pattern. Moreover, it can be argued that Batavians could never have had such good relations with Roman authorities and served in such large and at first presumably increasing numbers if their economic system had been very different. It is notoriously difficult to part a farmer from his plough, let alone hundreds or thousands of them, and not provoke reaction. From this perspective the Batavian military involvement can thus be seen as an additional argument for the economic system outlined earlier.

Population size
When the above line of reasoning is pursued in another

111 Bogaers/Haalebos 1975, 169; see above, p. 236: there is no indication for an increase in native pottery. In addition, the civilian settlement and Batavian capital has already been identified as site 403 on the Waal, and the Kops Plateau, because of its high location and as evidenced by the lack of pre-Roman habitation, was surely not an attractive place for a settlement.

112 These will be started in 1986.

113 See Bogaers/Haalebos 1975, 163–7. For an overview of comparable data from broadly contemporary camps, see Galsterer 1983, 70–2. Bogaers would apparently prefer to read legio I Gallica.

114 Although this is no way conclusive evidence, it is nevertheless remarkable that the military metalware from the 1971–2 excavation contains many finds associated with mounted troops: three out of four items from the first (pre AD 10) phase (nos. 2–4) and five or six out of seven from the second phase (nos. 5–9 and perhaps 11) as well as two of the four stray finds (nos. 14–15). See Bogaers/Haalebos, op. cit., 157–8. In addition, the graffiti mentioning the legio I G could conceivably have been from an aquilifer or an eques from that legion (op. cit., 164).


116 Nash, op. cit., 100–1.
further on these same Suebi are reported to 'make not much
tavians. His calculations would give an - unstated - minimum
of one-sixth, but arrives at a maximum estimate of 100,000 Ba-
and women. Von Petrikovits (1978, 238) assumes a maximum
fied as consisting
piieri, senes mulieresqite,
of children, old men,
able to bear arms, but then the remainder is explicitly identi-
maximum beyond 6000 is very unlikely.
BG
IV, i. Interestingly enough, in one sentence
117 See above, 390. The minimum may have been c. 5000, a
maximum beyond 6000 is very unlikely.
118 For an overview, see Steuer 1982, 61-2.
119 E.g. Caesar, BG I, 29 (Helvetii), of which one-fourth is
able to bear arms, but then the remainder is explicitly identi-
ied as consisting pueri, senes mulieresqite, of children, old men,
and women. Von Petrikovits (1978, 238) assumes a maximum
of one-sixth, but arrives at a maximum estimate of 100,000 Ba-
tavians. His calculations would give an - unstated - minimum
population size of 60,000.
120 Caesar, BG IV, 1. Interestingly enough, in one sentence
further on these same Suebi are reported to 'make not much
bearing arms were away from home. Applying this to the
Batavi, and there is no reason why this should be a dubiu-
ous procedure, results in a figure of 11,000 fighting men.
In view of the number of one-fourth mentioned earlier,
the minimum size of the population can thus be estimat-
ed at 44,000. This must indeed be a minimum when it is
considered that such factors as replacement of casualties,
or the conceivably very important impact of a dropping
birth rate due to postponed marriages or separation of
couples,121 are not taken into account. Before AD 43 this
may not have been much of a problem.122 But from then
on, with eight cohorts away in Britain, in addition to
those serving the emperor as his bodyguards, it certainly
was. The functioning of a warrior society, witness the
Suebi system, may well have been naturally suited to al-
low a client tribe to provide large numbers of numultuarii
for Roman campaigns. The permanent absence of these
people, however, is a fundamentally different matter,
with consequences liable to provoke a reaction. But
then, that is precisely what happened.
There are, evidently, other ways to arrive at a population
estimate. Bloemers, calculating on the basis of a demo-
graphic reconstruction for the population of the settle-
ment in Rijswijk and regional carrying capacity,123 ar-
ries at an estimate for the Cananefates of between c.
3200 and 9200 people from which c. 1000 auxiliaries
were raised. The large margin of error results from the
complicated statistics involving many different and var-
ying factors such as fertility, infant mortality, and the
available productive land. In general, it seems that for
men available for the auxiliaries (permanently absent) an
average of 1.2 men per household every 20 years is like-
ly,124 perhaps slightly higher (1.5 men) under favourable
conditions.125 For the Batavian population, following
Bloemers' calculations, this would imply that in order to
provide 5500 soldiers on a permanent basis, roughly be-
tween 4600 and 3700 households (estimated at between
use of corn for food, but chiefly of milk and of cattle, and are
much engaged in hunting'. For further comment, see Thomp-
son 1965, 3 f.
121 For a penetrating analysis of this and many related prob-
lems, see Imhof 1977, chapter 3.
122 Assuming that after Germanicus most troops except the
corporis custodes were still in the vicinity and at least regularly
at home.
123 Bloemers 1978a, chapters 5b and 6d.

235 (395)
5 and 8 people per household) are necessary. This is, however, only the number of households (and thus between 18,500 and 36,800 people) to keep the number of soldiers constant, not the number necessary for providing them initially.

It is difficult to believe that in the first half of the 1st century the average existing household could have provided more than one person on a permanent basis, especially when it is considered that the household size of 5–8 people is a well-founded estimate. Although more than one soldier from one household is possible, it is even attested that there were surely other households not able to contribute. The maximum population of 44,000 (5500 households of 8 people) calculated this way equals the minimum proposed earlier, but smaller numbers are possible. However, in view of the argument provided above, it is not very likely that the total population involved could have been much less than c. 40,000.

It is extremely difficult to relate this figure to the carrying capacity of the Batavian tribal area, as Bloemers did for the Cananefates, because the eastern river area covers only part of this area for the rest of which detailed geological information is not yet available and, moreover, its true extent unknown. There is, however, an alternative approach based on a general estimate of the tribal area and the data from the eastern river area. The maximum boundaries should enclose the area south of the Rhine between approximately the mouth of the Vecht or somewhat further (e.g. Bodegraven, Woerden, see figs. 130 and 135) in the west and Qualburg in the east, extending over the entire delta to an unknown distance into the Pleistocene sand area in the south. The analytical border from figs. 3 and 135 may serve as an estimate of the southernmost extension because if anything it is too far south. In terms of habitable area, the central and eastern river area should be roughly comparable, while the southern part is somewhat larger but, at the same time, agriculturally less attractive.

From all we know it is surely not an exaggerated estimate when it is assumed that the population in all three areas was approximately equal. This may be used to calculate the total number of settlements on the basis of the figures for the eastern river area during the Late-Iron Age and Early-Roman Period, which is a maximum of 250–300 settlements. This gives a maximum total number of 900 ‘Batavian’ settlements, which should accommodate a minimum number of 40,000 people. Even allowing for a very large central place, for example, of 1000 inhabitants, this gives an average of 43.5–44.5 people or at least 6 up to 9 households per settlement.

From the little information we have about settlements, this figure cannot be rejected absolutely, but as an average it seems to be excessive. Above, an average of ‘one or a few’ was considered, although the loosely connected household units forming the larger dispersed settlements known from the Middle-Roman Period may also have existed. A settlement such as Rijswijk-De Bult consisted of only one household in the Early-Roman Period and this situation is not likely to be an exception. In all probability, therefore, the two lines of reasoning do not lead to an acceptable conclusion. What is acceptable is that it is not so easy to define, but on the basis of what is known or suspected about sites, an average of 3–4 households could be possible, because it allows both a number of known or suspected one-household settlements as well as larger communities. What, then, are the implications of this assumption?

A first possibility might be that the estimated minimum number of 40,000 Batavians is far too high. This is rejected here because it is impossible to reduce it to the 20,000 people which should have populated c. 900 settlements of 3–4 households each, but which could never have permanently provided 5500 soldiers. A second possibility could therefore be that the estimated number of soldiers is too high. This is conceivable, as not all units need to have been full strength. But even when all units consisted of no more than around 400 Batavians there would still have been around 4500 men, tio such as caused by regular female infanticide is improbable. The practice of infanticide was perhaps forbidden (cf. Tacitus, Germ. 19), although presumably not under all circumstances (cf. Much 1967, 293).

References:
127 See L’Année Epigraphique 1952, 147–8: The Batavian brothers Gamo and Hospes as well as Indus and Eumenes. Although there must have been more cases like this there is no reason to assume an unequal sex-ratio with a surplus of males. Except for a factor like maternity mortality, an unequal sex-ratio such as caused by regular female infanticide is improbable.
128 See e.g. Van Tent 1984.
129 Cf. chapter 7.2. The available evidence also indicates a less, if not considerably less, denser occupation.
130 Above, p. 223 and 232–3.
131 See e.g. chapter 8, p. 284.
perhaps as few as 4000 when the contingent in Rome was only small and the evidence for a ninth cohort is rejected. This number would, however, still presuppose a population of c. 32,000 people. This difficulty does have another interesting implication, namely, that it is wholly impossible that the number of Batavian soldiers was higher than hitherto assumed. If anything, it was smaller and the possibility of milliary units can be completely rejected, which conforms well to the general absence of indications for such units in pre-Flavian times. Yet another possibility is that the total number of settlement sites was not 900 but 1800. As it is inconceivable that the other two regions of the tribal area were more densely populated than the eastern river area, this would mean that the estimated maximum number of settlements in our study area is not 300 but at least 600. As there is at least 300 km² of habitable surface,¹³² this is theoretically possible, but there is no way in which so many sites can be fitted into the evidence on site densities in the river area.¹³³ Even when the estimates put forward in chapter 8 should still prove to be too low, the overall picture of known sites and the control of this picture by the extremely detailed soil surveys in a few areas excludes even a remote possibility to double the estimated maximum; but it may be higher than hitherto assumed and when combined with an average of more than 8 people per household could provide a partial solution to our problem. An additional possibility is that current assumptions about the density of habitation in the Pleistocene area are wrong, which could also increase the total number of sites. A fourth way to bring our estimates into line, a reconsideration of the size of the tribal area, is not an attractive procedure. The perimeter outlined earlier is surely the maximum extent conceivable and cannot be extended when room is to be left for such groups as the Tungri, Cananefates, or Ubii. Only north of the Rhine could some area perhaps be added. An alternative option, that the Batavian units did not consist almost entirely of Batavians, can be rejected because the available information clearly indicates that this was not the practice for units from Lower Germany and Belgica.¹³⁴ In addition to all these deliberations, there may be other implicit or explicit assumptions or deductions which are not entirely correct. The discussion so far has shown that figures first designated as minimum estimates can easily be turned into almost untenable maxima by changing one or other basic assumption. As a whole, however, there are a number of probabilities or even certainties to be derived from all this. These can be formulated as follows:

1. The size of the Batavian population can be estimated at between 30,000 and 40,000 people, constituting between 4000 and 6000 households grouped into between 1000 and 1500 villages, or an average of 1250. In all probability, this figure is valid not just for the mid-1st century AD but for at least the century preceding the Batavian revolt and for some time after.

2. To accommodate this population the maximum size of the tribal area is not really large enough when earlier site density estimates are maintained, which means on the one hand that this maximum size is indeed plausible and on the other that population densities were presumably even higher than assumed in chapter 8; for the eastern river area at least 400 settlements need to be assumed, which means that site densities on deposits not surveyed in detail, or eroded, should be much higher than indicated by the available evidence. Especially the later eroded banks of Roman-period rivers or those covered by later bank deposits come to mind in this respect.

3. It is certain that the Batavian auxiliary units were not of double strength. They may have been just up to regular strength, but if they were the pressure on socio-economic life in the area must have been enormous. This pressure must have been quite considerable even when all units were in fact undermanned or in case of the (remote) possibility that there was one unit less than is generally deduced.

Socio-economic structure

As mentioned earlier, there are no indications that the native subsistence system in the river area, with an emphasis on stock-breeding, changed at all during the Early-Roman Period. It was presumably even well suited to the demands upon the population in the form of manpower for the army. Only after AD 43, when many men were permanently absent, this system may have come under considerable stress. Obviously, the Roman troops in the area – which were probably not numerous except during relatively short periods when they were very large indeed – will have obtained supplies from the local population, but the exchange remained limited.

There are a number of indications for this. First, there is

¹³² Chapter 7, p. 221.
¹³³ See also chapter 3, p. 73–5.
¹³⁴ Cf. Alföldy 1968, 100.
no evidence for an increase in arable farming except on the Kops Plateau and only for a short period which can be directly linked to the presence of Roman legions. Evidently, the army was forced to grow its own corn to reduce or save costly imports. Other foodstuffs, except probably meat, were imported, for example, wine, olive oil, and other exotica. Second, the number of early-Roman artefacts in native settlements is very limited indeed, which also testifies to the limited exchange. Although an interpretation in purely economic terms is not justified, the same is true for native burials, where only very few individuals received Roman pottery. This is marked contrast to the Nijmegen cemeteries. Third, native pottery is very scarce in military camps where all pottery which could not be manufactured on the spot is imported. In addition, other artefacts were also produced in the camps, in Nijmegen as well as elsewhere. Fourth, the Batavians are especially reported to have been exempt from taxation, which also rules out one-sided 'exchange'.

It can be concluded, therefore, that the new military and administrative structure was supported and maintained by outside means in so far as it could not provide for itself. Obviously, the Rhine and the Meuse are excellent transport routes for imports, although many of these goods, in particular before AD 9 and later during campaign years, must have been intended for personnel beyond rather than on the Rhine. It is very significant that this interpretation of maintenance by outside means can also be extended to the capital Batavorum, which can only be identified with site 403 on the Waal, in the centre of Nijmegen. The excavations have shown that this site was as Roman as it could possibly be, and that it was a new foundation in Augustan times. It is thus almost a perfect case of an administrative centre 'given' to the Batavians as the nucleus of their emerging civitas. It is not at all certain that the settlement quickly became more than the official centre with little more than formal ties to the native socio-political structure. This, at least, can be observed for the early stages of formation of the Gaulish civitates, where the direct involvement with the capital was left to underlings while the chief magistrates of the civitas were people with residences and interests elsewhere. Evidence for this situation among the Batavi could be the findspot of the altar dedicated by the supreme magistrate, Flavus, in Ruijmel, the fact that a major temple was built, in about AD 50, not in Nijmegen but in Elst (site 105), and the agrae and villae of Julius Civiliis on the insula Batavorum. In contrast, official monuments, such as a column which may have been erected on the occasion of Germanicus' victory in AD 17, were placed in Nijmegen.

But even apart from such interpretations, the archaeological evidence clearly indicates that the settlement system in the river area can be seen as a collection of little differentiated settlements tied into a social and economic network that only very gradually came to be administered through an imposed and artificially created and maintained centre in Nijmegen. At least at a material level, evidence for the beginning of a process of acculturation is very scarce, for example, the limited burial evidence. But the developing trade and industry in Nijmegen, the influence exerted by other military installations in the river area, and not to forget the Batavian participation in military enterprises surely served to accustom the native population to Roman concepts and values. On the other hand, large numbers of Roman troops were present only for short periods and the Batavians were only gradually, perhaps in the '20s and '30s of the 1st century AD, converted into more or less regular auxilia.

135 Chapter 8, p. 240-1 and fig. 68.
136 Chapter 8, p. 243-6.
137 Von Schnurbeim 1981, 69 f. See also chapter 6, 160, fig. 35, for early kilns.
138 See Bogaers/Haalebos 1975, 156.
139 There are early sites along the Meuse, notably Maas-tricht and Venlo, where e.g. Arretine terra sigillata has been found. They are not indicated on fig. 128 because there is no direct evidence for military activities there.
140 Although the suggestion that Roman generals were not able to winter their troops in Germany (Drinkwater 1983, 23, 26) is not correct as such (e.g., Haltern), it is surely true that they had to be provisioned from Gaul and the known installations could all be reached from the Rhine over water.
ries and then still remained native units under their own commanders. Direct interference with native affairs was presumably minimal. Sites on the bank of the Rhine were perhaps moved, or used for a military settlement, but, as noted earlier, the only really sizeable area occupied for Roman installations and other uses is in Nijmegen, on previously uninhabited land. Admittedly, this may partly have been a fortunate coincidence because the sites were probably chosen with a military objective in mind, but other objectives cannot be disregarded: the choice of location was ideal from various perspectives.

Significant changes occurred only in the '40s, in the early years of Claudius’ reign. Caius had only just recruited additional Batavian troops when the eight cohorts were moved to far-away Britain in AD 43, an event that must have considerably upset social and economic life in the region, even though archaeology has little to say about this directly. In AD 47, Corbulo’s recall and the subsequent withdrawal of troops to behind the Rhine was followed by an outburst of activity in the entire river area, with the building of frontier forts and all manner of other infrastructural works which decisively changed the degree and nature of Roman interference in the region.

In addition to a (perhaps rebuilt) fort in Meinerswijk, a new fort was probably built in Duiven-Loo waard (site 194) and perhaps in Kesteren. Driel (site 117/8) has also yielded ample evidence for Claudian occupation, and so have a number of forts further to the west mentioned in chapter 2. In Cananefate territory, Corbulo had a fosse dug which connected the mouths of the Rhine and Meuse behind the coastal dunes. In Batavian territory the perhaps obsolete Drusian fosse is no longer mentioned but the moles or agger, as an important element in the limes system, is also reported to have been worked on. For Nijmegen, literary or archaeological data indicating major changes are lacking, but the activities there surely increased. The Kops Plateau (site 417) continued to be occupied, at a cemetery such as that of the Museum Kamstraat (site 409) the majority of the burials dates from the reigns of Claudius and Nero, and Batavodurum (site 403) also grew considerably, and there is convincing evidence for stone buildings in this same period.

Elsewhere in the hinterland, a fort was built in Cuijk (site 499), obviously to control traffic along and across the Meuse at the intersection with the major road from Tongeren to Nijmegen. Finally, in the centre of the Overbetuwe, in Elst (site 105), a true Gallo-Roman temple was built in about AD 50. As concluded by Bogaers in his dissertation, this was evidently the work of (Gallo-)Roman engineers and soldiers and the initiative of Roman authorities. He sees it as a ‘national’ sanctuary, the location of which may have been determined by conditions in native society, and was a politically motivated ‘gift’ from Roman authorities as a symbol of the Batavian–Roman societas. Although it remains somewhat curious that such a symbolic site should be located outside Nijmegen, the interpretation as such is surely acceptable and all the more so in the light of the general context. One is inclined to see the temple as the Batavian parallel to the ara Ubiorum in Köln, which has been interpreted as the central cult site for Germania Magna, and to view it in a similar way as the formal epitome for the civitas Batavorum. By about AD 50 the stage for two centuries of further development had been set in its main outlines.

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147 Cf. site 132, see p. 239–40.
148 Cf. sites 117–118 (Driel), see p. 237.
149 See note 74.
150 In addition to what was said in chapter 2, p. 25, recent (1984) excavations in the vicus at site 38 have produced pre-Flavian finds. See jROB 1984, chapter II B, sub 2 c.
151 Tacitus, Ann. XI, 20 and Cassius Dio LX 30, 6. For archaeological evidence, see Bloemers 1978a, 27, with further references.
152 For references and a discussion of the context, see chapter 3, p. 52–3.
153 Bloemers 1979c, 36.
11.4 FROM CORBULO TO POSTUMUS

11.4.1 Revolt, Consolidation, and Growth until the Frankish Invasions

The revolt

Neque enim societatem, ut olim, sed tamquam mancipia haberis. Thus, in the words of Tacitus, was the gist of Civilis' complaints in his speech on the eve of the Batavian revolt. Although this was surely not the way he said it, there is every reason to believe that Tacitus has left us an adequate representation of Batavian sentiments. Evidently there is more to the revolt than its immediate causes, and its roots go back further than the last year of Nero's reign and the opportunities created by the confused events leading to and following his death. In fact, as was suggested in the previous paragraph, they can be traced back to at least a quarter of a century earlier.

When eight cohorts were sent to Britain in AD 43, this had serious consequences. As argued earlier, the permanent absence of all these men must have affected agricultural productivity. Due to the nature of the economy, with an emphasis on cattle-breeding, presumably this did not drastically undermine the subsistence base, but it must have caused a set-back. At least from this time onwards the Batavian troops were not relatively autonomous allies employed as tumultuarii when needed, but increasingly or even entirely pressganged into an organization of regular auxiliaries. Another consequence must have been a considerable social and demographic disruption. Fathers and sons were away from home for many years; casualties had to be replaced; young men in the army postponed marriage; married couples were separated. These or other, comparable, factors can only have led to a strong drop in the birth rate and, in combination with a higher male mortality, have caused a population decline which may have taken several decades to overcome.

When the effect of this development was still at its peak, new upheavals occurred. The river area became the scene of feverish activity with numerous foreign troops - where previously there had been only a few after Germanicus' day - engaged in building forts, dams, and other infrastructural works about which nothing has been recorded, such as roads and the temple at Elst. During the two decades following the events of AD 47, the population thus became subject to Roman interference in many different ways and on a scale never before experienced. Although much material may have been imported, the region's resources must have been exploited where possible. Where at first only the uninhabited forest on the ice-pushed hills at Nijmegen was used, now at least some tribal land must have been expropriated. In addition, trees were felled, fish caught, gravel dug, game hunted, and other resources tapped, to which there are bound to have been natives feeling that Romans had no right. With the increase of material and personnel flowing into the region came traders and, as formulated by Dyson, other unofficial and usually more obnoxious members of the ruling power.

Moreover, it should be remembered that Batavian tribal society was still being moulded into a civitas model. Similar to the tumultuarii being forced into the straight-jacket of regular auxiliaries, so society was transformed into a Roman civitas. It may well be argued that whatever progress was already made in this direction in the first half of the 1st century it needed to be brought to a satisfactory conclusion after AD 50, when the military commanders needed an effective civil organization to deal with the administration. In this respect, the building of the temple at Elst at about this time, is an ominous gesture. There may have been advantages in all this for the native elite, but no doubt there were most unwelcome consequences as well; this elite group, in particular, must have been under considerable acculturative pressure. The apparent controversies among them during the actual revolt may stem from these conflicting effects.

All such developments probably contributed to the general resentment which must have existed, as is shown by the immediate success of the revolt in its early stages. Perhaps this can even be supported archaeologically, with reference to the in itself not inexplicable but nevertheless possibility seems decidedly more attractive, especially for people who started their rebellion in a sacred grove (Tacitus, Hist. IV, 14: sacrum nemus), a native, not a Roman symbol. Besides, why would a Roman general destroy a Roman temple.

159 Tacitus, Hist. IV, 14: 'For we are no longer regarded as allies, as before, but as slaves'.
160 Cf. on this specific passage, Callies 1964, 146–7, note 77. In general, e.g., Sprey 1953, Much 1967.
161 Dyson 1971, 269.
162 As noted by Bogaers (1955, 192) there is no way of telling whether the building was destroyed by the rebels in AD 69 or by Cerialis when he devastated Batavian lands in AD 70, but the
163 Julius Briganticus (Tacitus, Hist. IV, 70 and V, 21) and Claudius Labeo (Hist. IV, 18; IV, 56; IV, 66; IV, 70) were opponents of Civilis.
164 Cf. above, p. 240–1.
ertheless striking absence of Roman artefacts in settlements almost adjacent to major early-Roman sites.¹⁶⁵ This may seem to be rather far fetched, and in any case one would not wish to use it as hard evidence, but there is also abundant literary evidence to show that the revolt was very strongly put into a context of traditional native values and customs. Anthropology has provided us with a good insight into such uprisings, which are a very common phenomenon indeed,¹⁶⁶ and in his admirable analysis of native revolts in the Roman empire Dyson has shown that these fall into the same pattern.¹⁶⁷ The Batavian revolt shows a number of characteristics which identify it as a strongly ‘nativistic’ uprising with unmistakable undertones of a ‘prophetic’ movement as evidenced by the important role of the Bructeran prophetess Veleda.¹⁶⁸ The revolt was initiated by Civilis, himself undoubtedly strongly Romanized as an auxiliary commander with 25 years of service behind him, during a ceremonial feast in a sacred grove.¹⁶⁹ Also, he dyed his hair red and allowed it to grow uncut until the destruction of Vetera, according to Tacitus some kind of native vow, and gives his son a truly barbarian upbringing.¹⁷⁰ Other nativistic elements are the customs of having women and children view the battlefield,¹⁷¹ the clattering of arms,¹⁷² the election of a leader by lifting him up on a shield,¹⁷³ and probably even human sacrifice.¹⁷⁴ In addition, there is evidence for the ethnographically well-attested phenomenon of the use of symbols from the dominating culture but converted into native terms or practices.¹⁷⁵ Thus, Roman military training was counted as an asset,¹⁷⁶ but the men were bound by Roman as well as native-style oaths,¹⁷⁷ and they did carry standards but native symbols were used as such.¹⁷⁸ All these indications are ample evidence that the rebellion was indeed a nativistic revolt resulting from a longer period of stress and acculturative pressure. But it might still never have happened had it not been for three vital elements, namely, the favourable conditions created by the civil war, the presence of a charismatic leader (praepotens inter Batavos),¹⁷⁹ and the immediate cause which was the increased recruiting combined with excesses caused by cruelty and incompetence of those in charge. An additional important fact in this respect, which is explicitly mentioned by Tacitus (Hist. IV, 13), may also be that Vespasian’s agents Primus Antonius and Hordeonius Flaccus actually incited Civilis to stage a revolt, to tie down Vitellian troops. The development of the uprising as such has been so extensively discussed elsewhere that it need not be repeated here.¹⁸⁰ It should suffice to note that its initial success led to the destruction of all symbols of Roman presence: not only is there literary and archaeological evidence for the destruction of all forts up to Xanten, and the dam across the Waal, but the temple in Elst was also demolished, while even Batavodurum may have been sacked.¹⁸¹ In retaliation, Batavian lands¹⁸² were devastated by the

¹⁶⁵ For a similar reasoning with indications for what they term contre-acclturation, see Gruzinski/Rouveret 1976, 200. But see also p. 412 below.
¹⁶⁸ Tacitus, Hist. IV, 61; IV, 65; V, 24.
¹⁶⁹ Tacitus, Hist. IV, 14.
¹⁷⁰ Tacitus, Hist. IV, 61 and Germania 31, 1. On further data about this vow, which seems to have been a deeply rooted symbolic act, see Much 1967, 385-8.
¹⁷¹ Tacitus, Hist. IV, 18.
¹⁷² Tacitus, Hist. V, 17 and Germania 11.
¹⁷³ As happened when Brinno, the Cananeate, was chosen as a leader (Tacitus, Hist. IV, 15).
¹⁷⁴ This might be deduced from the fact that the legionary commander Munius Lupercus was sent as a gift to Veleda (Tacitus, Hist. IV, 61). Compare Germania 9 and especially Ann. XIII, 57 where such sacrifices in relation to a victory are mentioned. Also, the fate of the captives given to Civilis’ son for target practice, mentioned in the same paragraph of the Histories, will hardly have left them in good health.
¹⁷⁵ A particularly horrible example is that of the Mexican Maya insurrection of 1869, where the anarchonomic custom of human sacrifice was revived but in the form of a crucifixion so that the rebels might have a messiah of their own race (Gruzinski/Rouveret 1976, 201-2).
¹⁷⁶ Tacitus, Hist. IV, 17: militaris disciplinas in castris Romanorum.
¹⁷⁷ Tacitus, Hist. IV, 15: barbaro ritu et patriis execrationibus.
¹⁷⁸ Tacitus, Hist. IV, 22: depromptae silvis lucisque ferarum imagines, ut cique genti intire proelium mos est.
¹⁸⁰ See Byvamck 1943, 220-79, Brunt 1960, or Van Soesbergen 1971 to name only a few.
¹⁸¹ When Batavodurum = Oppidum Batavorum = site 403 there is literary as well as archaeological evidence (Tacitus, Hist. V, 19 and JROB 1982, 34). When Batavodurum = site 403 and Oppidum Batavorum = site 417, the latter may not have been sacked but destroyed when it (as a camp of Batavian auxiliaries?) had to be cleared on the arrival of Cerialis’s army.
¹⁸² In this case evidently only the area north of the Waal, the present-day Betuwe.
army of C. Petilius Cerialis, a man who had had his experiences with a native revolt in Britain.\textsuperscript{183} He used not only military action but replaced it with diplomacy as soon as possible: a replacement of the use of force by that of power that already had its effect when Tacitus' narrative breaks off at the peace conference between Civilis and Cerialis. The attempt to become once again a client tribe, instead of a \textit{civitas} under direct Roman control,\textsuperscript{184} had thus failed. As a result, and quite certainly because Batavian troops and lands were as valuable or indispensable as before, the relation between Romans and Batavians was restored under its former conditions.\textsuperscript{185}

\textbf{Flavian consolidation}

In view of the measures that could have followed the suppression of the revolt, it can indeed be argued that this was a very favourable outcome for the rebels. But the almost jubilant tone in which this termination is often hailed in recent literature is utterly unjustified.\textsuperscript{186} Both literary and archaeological evidence clearly indicate that the conditions which were restored were not those of Augustus' day but the very different ones which had come into being under Claudius or even earlier and which had contributed to provide such fertile soil for the revolt. Undoubtedly, the excesses which were its immediate cause, and which may have been the result of the administrative breakdown and the additional pressure following from the troubles that started in AD 68, were remedied. But everything else changed for the worse, at least from a native point of view.

\textsuperscript{183} Tacitus, \textit{Agricola} 17; \textit{Ann. XIV}, 32.

\textsuperscript{184} Although his analysis takes a slightly different perspective, this is essentially the – entirely convincing – conclusion reached by Van Es (1981, 37–41). Civilis was not just a charismatic leader but an intelligent man who showed good sense at various stages of the revolt and who must have had a profound grasp of Roman policy and force. Although perhaps pushed by those behind him he cannot possibly have aimed at complete independence or even more. This despite the fact that in the wake of Julius Vindex' insurrection in Gaul (not a separation movement either, cf. Drinkwater 1983, 41) there were hotheads who entertained the notion of an \textit{Imperium Galliarum} (Tacitus, \textit{Hist. IV}, 59). These were, however, rapidly disavowed by their own people (\textit{Hist. IV}, 69).

\textsuperscript{185} This can be deduced from Tacitus, \textit{Hist. V}, 24–6 and \textit{Germ.} 29; cf. Byvanck 1943, 279, Sprey 1953, 109, Bogaers 1955, 188, 193–4, and many others.

\textsuperscript{186} It shows, in fact, the bias of Romanists reasoning from the Roman point of view and much along the same lines as any Roman historian would have seen it.
The leaders of the revolt, if they escaped with their lives, were surely neutralized. In general, the role of the native elite as officers in the auxiliaries was also terminated: they were replaced by Italians, a situation which is typical for Flavian-Trajan times. The number of Batavian troops was not diminished but, as mentioned earlier, they were reorganized and all of them sent elsewhere, first to Britain and later most of them to the Danubian provinces. The largely Batavian horseguard of the emperor was, understandably, not reinstated. In addition, the 10th legion was stationed in Nijmegen and the river area received its fair share of the large number of auxiliaries that were concentrated in Germania Inferior. In AD 70 for the Lower German district alone there were at least six alae and 23 cohorts. These were all mixed, not national units, some consisting of Roman citizens and most raised in Spain, Britain, and the Danubian provinces. Particular care was taken to recruit many of the necessary new supplements from elsewhere, notably from Thracia.

In addition to the direct damage as a result of the war, people in the river area were thus confronted with even more of their men being sent away and the permanent presence of many more foreign troops than before. These started to work with due speed on the repair of the burnt-down frontier forts and added some new ones in the process, which led to an even more closely spaced chain of such establishments all along the Rhine (fig. 130). A camp on the site of the former Augustan one in Nijmegen was rapidly converted into a true legionary fortress. Other infrastructural works must have been repaired and further developed, and the temple at Elst (fig. 129) was 'rebuilt by the Romans in a grand style very shortly after the revolt', according to the author of these words 'a splendid confirmation of the very favourable peace-treaty'. The symbolic meaning of the rebuilding is indeed almost self-evident, but when viewed from a native context such an interpretation is either naive or a cynical overstatement.

Shortly after AD 70, a new capital was founded on the bank of the Waal to the west of the largely deserted Batavodurum (site 403). The latter continued to be occupied, but covering a much smaller area and probably in direct connection to the military sites. Whether the new site (no. 399) ever developed into a fully fledged town is still rather uncertain, but it is known that the work on its construction began in Flavian times. The traditional and the only logical interpretation of this is that it was decided to 'give' the Batavians a true civil capital, as was done in so many other areas where towns were founded. Although, evidently, great and noble deeds were performed and tasks accomplished from a Roman viewpoint, one can be quite certain that especially in the first years under Vespasian and Titus the modal Batavian in the river area must have been decidedly unhappy with his conditions of life. On the other hand, the enormous input of capital in the form of army pay and building programs must have boosted economic activity and been the source of wealth for increasing numbers of the local population. We shall never know how long and to what degree feelings of enmity persisted. But there is every reason to believe that already the following generation coming of age under Domitian, and surely the next one under Trajan, no longer shared their fathers' or grandfathers' feelings to the same extent, if at all. At least the archaeological evidence, to be discussed below, shows that they did profit from a relative prosperity and participated in the social and economic life developing in the consolidated civitas and protected by an effective system securing imperial borders.

The nature of this system of forward defence has been admirably analyzed by Luttwak. It is a strategy of border defence connected with a territorial empire, in which virtually all troops are located at the frontier. As however, at least half of the horsemen and perhaps even all of them were again recruited in Lower Germany (cf. Speidel 1984, 43–5).

187 Civilis' fate is unknown. There is evidence that Veleda was later shipped to Italy and installed in the service of a temple there. See Dyson 1971, 265, and notes 65 and 66 with further references. Also Tacitus, Germ. 8 and Cassius Dio LXVII, 5 and the comment by Much 1967, 169.


189 Cf. Alföldy op. cit., 14 and 47, with further references. A very recent overview, with new details, is provided by K. Dietz, Das älteste Militärdiplom für die Provinz Pannonia Superior, BRGK 65, 1984 [1985], 159–268.

190 The emperors employed regular auxiliary cavalry until Trajan instituted the equites singulares Augusti. By that time,
can be deduced from what was said earlier, this strategy was introduced in Germania Inferior at least from early Claudian times onwards in a systematic way and the Flavian period merely saw its restoration and further extension.\footnote{199 As was also objected by Mann 1979, 179, Luttowk's opinion that the preclusive defence system with 'scientific' frontiers was a Flavian invention is thus surely not correct. But this is only a minor matter of historical accuracy and Mann's further comments on Luttowk's models are unjustified and stem from a partial misunderstanding of their purpose. They are not designed to describe historical particularities and not intended, which Mann (op. cit., 180) seems to think, as general laws covering all situations. They are merely a heuristic device to help understand the nature of an empire and its means of ensuring security. Mann is right in pointing out that Roman frontiers were normally the result of practical \textit{reactions} instead of \textit{actions} and that Rome lacked institutes of strategic studies, but that does not relieve us of the task of performing such studies and thereby providing a new dimension to our understanding of historical 'facts'.} As far as major threats are concerned, the defensive strategy operated in a tactically offensive manner. The \textit{limes} as such was designed to serve as the supporting infrastructure but armed conflict was to take place beyond, not at or within, the border. This, of course, implied an early warning system and control of the frontier. The fixed defences on the frontier were not designed to endure prolonged attacks as in a siege. Smaller threats could be dealt with by intercept movements carried out by forces stationed in the frontier forts, while the legions were deployed in such a way as to ensure battlefield superiority in each sector of the frontier. At the lowest level, small-scale incursions could be thwarted by the troops from individual forts or fortlets and the border patrols.

Necessary elements in such a system are, of course, watchtowers and sometimes outpost forts, good roads, and an excellent communications network. In addition there is the use of power in the diplomatic control of the tribes living beyond the frontier. In this way, when the system functioned properly, all manner of outside threat could not only be countered but that could be accomplished beyond the frontier, thereby providing continuous, day-to-day security even for the frontier provinces and the actual frontier zones.

In fig. 130 the \textit{limes} of Germania Inferior is indicated, from Claudius' reign onwards.\footnote{200 For further literature, see Bogaers/Rüger 1974, Gechter 1979, and chapter 2, note 31.} In its developed form, after AD 70, it is essentially one line along the Rhine, with only one possible (not indicated) and intermittently

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{fig130.png}
\caption{The \textit{limes} of Germania Inferior from Claudius-c. AD 270: 1 legionary fortress, 2 fort, 3 possible fort, 4 fortlet, 5 modern border, 6 canal, 7 possible border of the province. Not all sites were occupied during the entire period.}
\end{figure}
During this period, and somewhat later, some reduction of the number of auxiliary troops in the province began to take place, but substantial changes were made only after the Flavian period.


This entire phase can thus be seen as the rapid and in part compulsory transformation of the river area and other regions into a well-ordered frontier province with a secure frontier and internal peace. The latter was not achieved so much by changing the basic reasons for the revolt but rather by stamping out all thoughts of resistance by overwhelming force and presumably by proper
and competent management. This not only prevented further excesses such as took place in the late '60s but should also have led to a fairly quick and surely profitable adaptation to the new conditions on the part of the generation coming of age after the revolt.

It is difficult to judge from the literary sources whether this involved a conscious policy to alter conditions in the frontier zone other than those already described. It is not likely that the established subsistence pattern of the native population could have been changed, especially not when so many men were in the army and, at least initially, the population was declining. But we do know that the central government did not hesitate to take regulating measures when deemed necessary, as is testified by Domitian's edict promoting the cultivation of corn by restricting the making of wine. But then, the emperor had to abandon this policy rather quickly, which shows that such measures were not easy to enforce.

Further developments

In AD 98 Trajan, the experienced general and governor of Upper Germany, rose to the purple as Nerva's heir. He was well acquainted with the situation in both Germans and as the emperor's adopted son presumably had assumed responsibilities in Lower Germany as well. That, at least, may be deduced from the fact that his accession to the throne was in Köln, not the most logical place for a governor of Upper Germany. Although he was personally primarily engaged in Dacia and Mesopotamia, Trajan's reign brought a number of important further developments for Germany Inferior in general as well as for our area. First, there is the fact that he revived the imperial horseguard in the form of the equites singulares Augusti, of which, initially, at least half the men came from Lower Germany. Thus the honour of serving in the emperor's personal guard was restored to the Batavians and adjacent tribes, which can only mean that in Trajan's expert judgement they were worth it. Thus, at the beginning of the 2nd century, conditions in the river area must have been satisfactory from a Roman point of view which testifies to the effectiveness of Flavian policy as well as to changes in the native attitude. Further evidence for this is, of course, provided by the withdrawal of the 10th legion from Nijmegen and the rapid reduction of troops in the first two decades of the 2nd century for the province in general. The civil settlement in Xanten was elevated to the status of a colonia, Colonia Ulpia Traiana, and the town at Nijmegen, Ulpia Noviomagus Batavorum, was presumably granted the ius nudinariurn as suggested by the Celtic name (new-market). The measure as such reflects a conscious concern that Nijmegen should function as a true central place, irrespective of whether the grant was an attempt to create a new function or the sanctioning of an established practice.

Under the favourable conditions of the 2nd century the somewhat reduced and temporarily declining civilian population can be expected to have increased again. Population growth may also have been stimulated by settlers from elsewhere, notably the veterans of the legions and auxilia who appear normally to have settled in the provinces in which they had been discharged. Even more important in this respect could be that in the 2nd and 3rd centuries units recruited soldiers in the areas where they were stationed and in Lower Germany there is no evidence for the Flavian practice of bringing recruits in from elsewhere or for placing units under reliable Italian commanders, a procedure which must have died out under Trajan. We might thus assume that many Batavian soldiers served at home again, but such a conclusion may not be entirely justified. There is epigraphical evidence showing that Batavian units may—

209 Suetonius, Domitianus 7, 2 and 14, 2.
210 Eutropius, Breviarium VIII, 2.
215 For summaries of the evidence, see Sebesta 1972, esp. 116–9 and 144–6 (auxiliaries) and Mann 1983, esp. 25–8 and 97–9 (legionaries).
217 Two inscriptions, L'Année Epigraphique 1935, 163 (= ER III, 240, no. 1141A) and 1944, 97: Batavian officers of the Cohors III Batavorum. See also Holder 1980, 300, no. 1161A: a Batavian in the Cohors I Batavorum, recruited in about AD 88, and most recently Dietz (cf. note 189).
to an unknown extent – have retained their national character until at least the early-3rd century, if not even much later, an exceptional situation indeed.\textsuperscript{218} In general, however, we may assume that in addition to the new settlers more Batavian soldiers served at or near home than was the case in the second part of the 1st century.

From Trajan times onwards only two legions were stationed permanently in Lower Germany.\textsuperscript{219} The largest concentrations of troops in the empire were no longer to be found on the Rhine but in the Danubian provinces and in the east. Hadrian’s concern for the frontiers is, however, well known and also had its effect in the river area where no additional works on the frontiers were necessary. In AD 120/121 he may have visited there himself.\textsuperscript{220} The bestowal of the \textit{ius nundinarum} on the capital of the \textit{civitas Cananefatium}, Forum Hadriani, has often been connected with this visit, and the river area will presumably have profited from the emperor’s general policy to further economic development,\textsuperscript{221} and from government expenditure on the troops in particular.

No major events are recorded for most of the 2nd century, which in itself is sufficient proof for an undisturbed and prosperous development. The \textit{times} system was able to cope with high intensity threats such as those on the Danube frontier,\textsuperscript{222} although the effort must have been a considerable drain on the imperial resource potential.\textsuperscript{223} Intermediate-level threats, such as the Chaucian raids which affected Lower Germany in the ’70s,\textsuperscript{224} were also no real problem. But, gradually towards the end of the 2nd century, more troubles arose in the interior as well. An example is the brigand Maternus in Gaul who turned into a usurper so quickly that the implicit popular support has probably correctly been interpreted as a reflection of growing social unrest.\textsuperscript{225} An important cause for this was the continuing drain on state finances which may well have been virtually exhausted at the time of the assassination of Commodus in AD 192.\textsuperscript{226} This started a civil war, with another ‘four emperor year’ in AD 193 and ending only in AD 197 with the death of the usurper Clodius Albinus. The events must have affected Gaul and thus, although not directly, implicitly the German provinces as well.

But Severan policy resulted in amelioration of the economic situation,\textsuperscript{227} and in active use of force\textsuperscript{228} and diplomacy\textsuperscript{229} in Germany; moreover there is epigraphical evidence for repairs to the frontier forts.\textsuperscript{230} In this same period, in AD 212, Caracalla’s famous edict conferred Roman citizenship to all free subjects of the empire, the significance of which will, however, have been quite minimal for most people.\textsuperscript{231} More important, as noted by Van Es,\textsuperscript{232} is the apparent enlistment of foreign (Frisii and Tuilanti) cavalry in the region north of the river area, evidently client tribes called upon to supplement troops no longer available inside the empire. Such northern tribesmen may well have constituted Caracalla’s bodyguard. Their description as Germani\textsuperscript{233} is strongly reminiscent of the (largely Batavian) bodyguard of the Julio-Claudian dynasty, but Caracalla’s guards apparently spoke no Latin which is hardly conceivable when they came from Lower Germany.

As in so many other provinces, the further history of Germania Inferior in the 3rd century is one of continuing troubles, although the literary evidence is rather scanty. Thus, the probability of large-scale military activities in the Batavian area under Elagabalus, around AD 220, can only be deduced from the presence of the le-

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\textsuperscript{218} Cf. Bogaers 1960–61, 285, note 130. In addition to the cases mentioned in note 217 one may also point to the case of the Batavian \textit{dux} Postumus (discussed below) and to the \textit{dux} Aurelius Ianiarius (chapter 12, note 27). Although the context is different, one might even refer to a remark by Ammianus Marcellinus (XVI 12, 45) about Batavians fighting \textit{cum regibus}, under their own kings, in the battle at Argentoratum in AD 357. This reading is, however, a very doubtful one and it is much more realistic to translate this as ‘with the Regii’, one of the \textit{legiones comitatenses} (cf. Hoffmann 1969, 68).

\textsuperscript{219} These were the 30th legion (replacing the 6th) in Xanten and the 1st legion in Bonn, both remaining there until the late-3rd century.

\textsuperscript{220} \textit{Vita Hadriani} X, 1 and Cassius Dio LXIX, 9.

\textsuperscript{221} \textit{Vita Hadriani} XX.

\textsuperscript{222} Cf., e.g., Drinkwater 1983, 64.

\textsuperscript{223} Cf. Bloemers 1978a, 85, and note 147.

\textsuperscript{224} \textit{Vita Didii Juliiani} I, 6–8.

\textsuperscript{225} Drinkwater 1983, 79–80.

\textsuperscript{226} Crawford 1975, 562, note 6.

\textsuperscript{227} Crawford, \textit{op. cit.}, 566–8.

\textsuperscript{228} The campaign of Caracalla (Aurelius Victor XXI, 2; Cassius Dio LXXVIII 13, 4). See also Schönberger 1969, 173–4. Unrest beyond and at the frontier may also be deduced from a number of hoards (cf. paragraph 8.4.5 and note 271).

\textsuperscript{229} Cassius Dio LXXVIII 14.

\textsuperscript{230} Glasbergen 1947, 305; Bogaers 1976, 230–2; Bloemers 1978a, 94. See also chapter 8, note 272 and below, note 396.

\textsuperscript{231} Cf., e.g., Drinkwater 1983, 86.

\textsuperscript{232} Van Es 1981, 46.

\textsuperscript{233} Cassius Dio LXXIX 6, 1 and Herodianus IV 13, 6.
tionary legates of the 1st and 30th legions in Vechten as revealed by inscriptions.\textsuperscript{234} But it is not at all certain that life in the river area was already seriously affected by such events. Even under the serious attack of the Alamanni in AD 233, which brought Severus Alexander to the Upper Rhine where he was murdered by his own troops at Mainz in AD 235,\textsuperscript{235} the \textit{limes} held and the invaders were driven back successfully by Maximinus Thrax. Repairs were made,\textsuperscript{236} and the new ruler, the first of the ‘soldier-emperors’, could even entertain the notion of conquering Germany as far as the ocean (the North Sea?).\textsuperscript{237} There are very few direct clues for events in the following decades. Barbarian incursions may have taken place, but much of the older interpretations of what happened after AD 240 can no longer be upheld.\textsuperscript{238} Decius seems to have had to cope with a revolt in Gaul as well,\textsuperscript{239} but on the other hand administrative measures such as the placing of milestones indicate real political and military efforts in such different areas as the Agri Decumates\textsuperscript{240} or the western Dutch river area.\textsuperscript{241} The incessant pressure on the \textit{limes} and the barbarian incursions, in part surely evoked by the withdrawal of troops for civil wars, apparently necessitated Gallienus to move his headquarters to Köln in AD 257.\textsuperscript{242} It is under his reign that Franks are mentioned with certainty for the first time,\textsuperscript{243} from then on to appear more and more frequently in the sources. As a group these Franks remain somewhat elusive, but it is generally assumed that the name refers to a kind of collective designation of a number of transrhenish tribes such as the Chauci, Amsivarii, Chamavi, and others. The majority of these were undoubtedly rather warlike tribal groups at the periphery of the empire which, by the mid-3rd century, had been subjected for two centuries to a varying degree of control, direct contact, and – in the general analytic terms discussed in paragraph 11.2 – information flow from the Roman state. This led to processes of internal social change and a more hierarchically organized society.\textsuperscript{244} Perhaps to some degree initiated by internal warfare and population movements in Germania Libera but certainly accelerated by the increased recruitment of transrhenish soldiers for the Roman army in the 3rd century,\textsuperscript{245} these groups were now able to muster enough military force on a permanent basis to be a continuous high intensity threat. As shown by the developments in the first half of the 3rd century the \textit{limes}-based system of forward defence was, in principle, capable of countering such a threat. But it could not cope when sufficient military force was lacking, and the use of power was impossible with adversaries who had a clear perception of the true extent of the available forces. Moreover, it could not cope with too many major threats in many different sectors of the frontier at the same time, such as posed by the Franks and Alamanni, the Goths, or the Persians. Evidently, although his initial efforts were not at all unsuccessful,\textsuperscript{246} Gallienus was bound to fail in the end, as is so well summarized in the judgement passed on his reign a century later by Eutropius: \textit{Gallienus ... imperium primum feliciter, max commode, at ultimum pernicieose gessit}.\textsuperscript{247} The Franks, whom Gallienus initially succeeded in defeating, must also have invaded and passed

\textsuperscript{234} CIL XIII, 8810 and 8811. The deduction is from Alfeldy 1967a, 54–5.  
\textsuperscript{235} Herodianus VI 7, 9 and Zonoras XII, 15.  
\textsuperscript{236} Herodianus VII 2, 6–8. See also Schönberger 1969, 175, for epigraphical evidence.  
\textsuperscript{237} Herodianus VII 2, 9 and Vita Maximiunum XIII, 3–4.  
\textsuperscript{238} Notably those cited by Van Es 1981, note 139, which were already, and quite correctly, objected to by Bogaers 1964, 50–1.  
\textsuperscript{240} CIL XIII, 9103, 9111, and 9123; see Schönberger 1969, 176. In this area, on the eastern bank of the Rhine, there is even a milestone (of disputed significance: Schönberger, op. cit., 177–8, Von Petrikovits 1978, 174) dated to AD 269/70 and presumably evidence of a display of loyalty to the Gallic empire.  
\textsuperscript{241} L’Année Epigraphique 1965, 118. See Bogaers 1964 and Bloemers 1978a, 85: league-stone from Rijswijk dated to AD 250.  
\textsuperscript{242} Zosimus I 30 and 38; see Von Petrikovits 1978, 171.  
\textsuperscript{243} De Boone 1954, 13–4: there is every reason to believe that the action against the Franks by the later emperor Aurelianus, tribune of the 6th legion at Mainz, recorded in the Historia Augusta, vita Aureliani VII, 1, did not take place before Gallienus.  
\textsuperscript{244} For an overview, see e.g. Steuer 1982, chapters 5 and 6.  
\textsuperscript{245} An early example are the Frisii and Tuilhantes (in the Cuneus Frisiorum) and the Germanic Numerus Hnauifridi on Hadrian’s wall (e.g., Frere 1978, 208, 212; compare chapter 10, note 92).  
\textsuperscript{246} For overviews, see e.g. De Boone 1954, 32 f. or Von Petrikovits 1978, 171–5.  
\textsuperscript{247} Eutropius IX, 8: Gallienus ... administered the empire at first with luck, later bearable, and in the end calamitously.
through the river area and general conditions of life there cannot have been very favourable. The dramatic invasions of the Franks and Alamanni in AD 259 or 260 and other disasters such as the capture of Gallienus' father Valerianus by the Persians, can only have added to the disarray. Although as a frontier zone the river area must have been harassed to some degree before then, the first major calamity can thus be dated to the 6th decade of the 3rd century.

It is uncertain whether this was also the final one. Presumably in AD 259 Latinus Postumus, a dux or army commander (or governor of Lower Germany?) intercepted a raiding party, probably Frankish, carrying valuable plunder. When this was claimed for Gallienus' son in Köln Postumus' troops revolted and proclaimed him emperor. But unlike so many others in those days, Postumus did not become a usurper striving for control of the empire. Instead, he formed an independent 'Gallic Empire' in the Gaulish and Germanic provinces, with his residence in Köln, and extended it to Britain and Spain as well. Without the troubles of the whole empire on his shoulders, he was thus able to carry through effective military and administrative reorganizations.

There are a number of very good reasons to assume that Postumus himself (fig. 131) was a Batavian, and the nucleus of his forces may even be identified as Batavians form a reference in which the rebelles Gallicani are discussed in the context of the Gaulish empire as a rebellio Batavica. It is most likely to be the Batavian alumnus under whose reign Batavian lands were settled by Franks. This conclusion by implication shows what must have happened in our study area. The Frankish invasion which caused Gallienus to come to the Lower Rhine must have caused large-scale disruption in the river area and at least part of its inhabitants to be killed or to flee. But the Batavian 'nation' as such survived as a social unit, as is evident from its later role in the army, although some (other?) people must have been moved, perhaps not very much later under Constantius Chlorus, to new locations in northern France as laeti. It is probable that a Batavian population remained in the part of their civitas south of the Waal. This can be deduced from the fact that the Franks who were apparently so vigorously repulsed by Constantius Chlorus are explicitly reported to have occupied only the land encompassed by both branches of the Rhine, thus the area between the Waal and Lower Rhine. Further confirmation of this conclusion is provided not only by other, less precise, references to this occupation but also by the same passage which men-

248 For a short account, see Von Petrikovits 1978, 174 f.
249 See the discussion by De Boone 1954, 36.
250 Panegyrici Latini VI (VII), 4: Panegyricus Constantino Augusto.
251 Not Carausius, who was Menapiae civis (cf. Aurelius Victor XXIX, 20).
252 Panegyrici Latini VI (VII), 5: Panegyricus Constantino Augusto. See De Boone 1954, 36, 42.
254 These are recorded in the Notitia Dignitatum, Occ. XLII; the area of Bayeux-Courtances, Arras, and Condren-Noyon. See also chapter 12, 435.
255 Panegyrici Latini VIII (V), 8: Panegyricus Constantio Cassari.
256 See De Boone 1954, 15 and 57 f. De Boone (p. 36) also called attention to the fact that Hercules Deusoniensis, the special protector of Postumus, is almost certainly a deity relat-
tions in addition that the Frankish conquest extended further southwards along the coast and included the Scheldt region. The total occupied area thus covered only part of the civitas Batavorum but entirely the lands of the Cananefates and Frisiavones (as indicated on figs. 3 and 4 in chapter 1). It is hardly coincidental that these tribes are precisely the only two who are lacking in all late-Roman sources in which all of their neighbours (the Menapii, Nervii, Tungrhi, Batavi, and also the Frisii from north of the Rhine) are still mentioned.

In how far Postumus was in direct control of events in the river area is difficult to judge. It has been argued that he allowed the Franks to settle along the frontier as his allies, to defend it. For general strategical reasons, this conclusion is in fact inescapable because the Gallic empire could not have survived if those Franks south of the Rhine had been in the van of a movement against it. Moreover, Postumus is explicitly reported to have enlisted many Germans and Franks. Precisely what the balance of power was, and the extent of Postumus’ true options, remains, however, obscure.

The Gallic emperor’s murder at Köln in AD 268, at the hands of his own troops, ended the administrative, military, and certainly economic conditions that had already been rapidly deteriorating since the mid-3rd century. The Gallic empire existed a few years longer but the recorded events show that neither his successors nor the legitimate Roman emperors in the following years were able to control events or even run a proper administration. In the river area Batavi and Frankish tribes (such as the Chamavi?), must have maintained some kind of uneasy balance.

11.4.2 The Development of a Frontier Zone

Introduction
Between the hard-pressed Germanic Batavians of the mid-1st century AD following Civilis in a native revolt against their oppressors, and the harassed Roman Batavians of the mid-3rd century turning to Postumus, one of their own sons and as emperor of a large part of the western Roman world the last one to protect them to some degree against German invaders, there is a world of difference. The previous paragraph has presented the literary evidence and a number of deductions and interpretations concerning historical developments and underlying processes. But this evidence spans a period of two centuries and specific information about the river area is only available for the first and, to a lesser degree, for the last few decades.

Most of this period can thus indeed be described as ‘an age without history’. In principle, this can be taken as a result of the proverbial process where no news is good news, but then it has to be remembered that all the news that has reached us has been transmitted by a few very privileged and very Roman authors. They have left us the general outlines, but what happened in a peaceful and well-controlled border zone and what that meant for the population there can only be investigated by interpretation of the archaeological evidence. And this interpretation should not proceed from a historical, let alone Roman viewpoint, but from an assessment of what the native and Roman societies were like, the context of their interaction, and the processes thus set in motion.

In this paragraph, a number of such processes will be examined, based on data assembled in previous chapters. It is thus essentially an interpretation of a number of observed changes, resulting from a process of acculturation. However, changes need to be observed before they can be interpreted, and whether they are seen depends on the questions asked as well as on the availability and quality of data. For both these reasons, only a limited number of issues will or can be examined here.

Initial impact of the army
A number of consequences of the establishment of a *limes*-based forward defence system in the mid-1st century AD have already been mentioned above. A major change was, of course, the permanent stationing of troops, normally auxiliaries, in frontier forts along the Rhine. There is little evidence that auxiliary units were still stationed with the legions, although there are also reasons to assume that some troops, and notably cavalry units, did indeed stay with legions. Of these legions, two were stationed in Xanten and one each in Bonn and Neuss.

This is, in fact, a rather curious state of affairs because
the strategical redeployment evidenced by moving the two legions stationed in Köln to Bonn and Neuss was not effectuated with respect to Xanten. The reason for this is not, as has formerly been suspected,\(^{264}\) that the Batavians were charged with the defence of ‘their’ section of the frontier. Most of their units had been sent to Britain and there is enough evidence for auxiliary forts all along the Batavian part of the limes. Perhaps this is simply a case of old-fashioned traditional thinking by those in command,\(^{265}\) and there may have been many legionaries not actually in Xanten but serving in detachments stationed in the river area for various purposes, such as to garrison some of the forts,\(^{266}\) construction work such as the temple at Elst, or major enterprises such as the agger at the Rhine-Waal fork which the commanding officer in Lower Germany, Pompeius Paulinus, had his troops build in AD 55.\(^{267}\) But this does not explain why one legion was not moved from Xanten further down the Rhine; in fact, it makes it even more difficult to understand.

Part of the reason could be that the cost of stationing and provisioning a whole legion in the delta was judged to be too high. After all, as far as we know everything had to be set up there from scratch, whereas upstream there was already an infrastructure available at the sites mentioned and in Köln even a colony was founded in AD 50. But this cannot have been a compelling reason so that, unless further research in Nijmegen or the river area proves some of the current assumptions wrong,\(^{268}\) those in charge must have felt that the delta was adequately guarded without a legion. As a matter of fact, we do know that the region was unusually heavily guarded. For example, the installations in Velsen were probably occupied until the early years of Nero’s reign,\(^{269}\) thereby controlling the coastal area in front of the limes. Even more important are the indications for a line of forts in the rear, namely, one certain case in Cuijk (site 499) and the two forts in Grinnes and Vada which, at least in AD 70,\(^{270}\) are described as cohortium alarumque castra. As mentioned earlier, Grinnes could be the site at Rossum, where a pre-Flavian fort is possible but uncertain.\(^{271}\) Vada’s location is unknown, but both sites were located at least south of the Waal.

As for the forts on the Rhine, we know that a whole chain of them existed\(^{272}\) but very little about their sizes or garrisons. Alfföldy has calculated that after the known withdrawals under Claudius and Nero there were at least 8 alae and 12 cohortes left in Germania Inferior on the eve of the revolt.\(^{273}\) This number should probably be further augmented as far as the cohorts are concerned, in view of the many forts. The maximum number at that time for the limes downstream of Xanten is already 19, of which 13 can be considered fairly certain.\(^{274}\) By contrast, there are only two auxiliary forts (Moers-Asberg and Remagen) plus a fleet-base (Köln-Alteburg) known upstream, where the legions were stationed (perhaps with auxilia).

Unfortunately, all this is of little help in establishing the number of troops stationed in the Batavian area. There are simply too many uncertainties to give even remotely reliable estimates, apart from the fact that some units were transferred elsewhere and there were temporary task forces on special duties. With quantification being thus impossible, a discussion of the impact of the troops on social and economic conditions in the region becomes a difficult matter. The limited information about Batavodurum (site 403) shows that trade and industrial activities developed there,\(^{275}\) as finds and features indicate virtually entirely attributable to military personnel or Gallo-Roman immigrants, or both. While it was un-

\(^{264}\) Cf. Van Es 1981, 100.

\(^{265}\) Cf. the remarks of Ritterling in Stein 1932, 27. The habit of deploying legions in multi-legion camps was only explicitly forbidden by Domitian (Suetonius, Domitianus 7, 3).

\(^{266}\) Such as is suspected for the pre-Flavian occupation at Zwanmerdam (Haalebos 1977, 87) and repeatedly hinted at for the first fort of Valkenburg (De Weerd 1977). As long as excavations in Nijmegen, and notably on the Kops Plateau (site 417), are lacking it is always possible to situate a legionary detachment there as well, although (Batavian?) auxiliaries would seem to be a better proposal as argued above.

\(^{267}\) Tacitus, Ann. XIII, 53. See also chapter 3, p. 52–3.

\(^{268}\) See also paragraph 8.4.1, p. 249 and note 98.
doubtedly the administrative centre of the civitas, there is very little evidence that the site was also a true central place in the socio-economic sense of the word. Literary evidence indicates that the Batavian elite lived elsewhere and archaeological evidence shows that there was no widespread use of Roman material which could testify to intensive social and economic contact.\footnote{Cf. Tacitus, \textit{Hist.} V, 23, 3; see also above, p. 398 and note 144.} Admittedly, the potential of the current data-set to register such evidence is limited,\footnote{See above, p. 240–1 and 400–1.} and the material available at present overaccentuates the true state of affairs, but compared to the evidence for later periods its minimal quantity is nevertheless significant. It is conceivable, as suggested earlier,\footnote{See chapter 8, p. 232.} that this scarcity represents a kind of cultural resentment on the part of the native population. This may be true, but it is also conceivable that the major reason was simply one of supply. The pottery production in Nijmegen and at the sites of forts (such as at Cuijk, site 499) was limited and there is no evidence for an industry capable of supplying the whole civitas, if there was indeed a demand. The military was probably supplied from elsewhere and to a certain – and gradually increasing? – degree by local manufacturing, but for widespread use of wheel-turned pottery on native sites there may not have been enough.

Convincing as this may be, it can only be part of the reason, as is shown by the research in Frisian territory north of the Rhine. There, in the region around the Claudian-Neronian fort at Velsen, conditions applying to the eastern river area should be even more clearly visible. In reality, the number of finds from that period in the region is surprisingly high.\footnote{As Brandt has shown, however, the peculiar composition and condition of the imported pottery from native sites indicates a particular, selective, process of dispersal. His interpretation, in the context of native exchange patterns, offers an intriguing model. But it is not relevant for the present discussion because Batavians evidently, and understandably, had little use for Roman potsherds. But the evidence does show that already under Claudius there was a very different handling of Roman artefacts in a native context beyond and within the frontier, which is true even if Brandt’s model itself should not be appropriate: in any case the Frisii were keen on having material which the Batavi cared much less about.} As shown by the evidence for the Flavian period and later,\footnote{E.g., Von Schnurbein 1982, 132–4 or Fulford 1984, 135.} and thus implicitly for the phase under discussion, the necessary corn was probably largely imported. As mentioned earlier, in the native economy arable farming was second to stock-breeding. Undoubtedly, the fertile soils of the stream-ridges in principal produced good harvests. But with a traditional economy focused on stock-breeding and because so many men suddenly were absent after AD 43, it is hardly conceivable that the population could have increased arable production even if they so wished. Also, the well-datable pollen diagrams of the Kops Plateau and Meinerswijk do not show a strong increase in indications for the expansion of arable land,\footnote{Teunissen/Teunissen-van Oorschot 1980; Teunissen a.o. 1985.} and are thus evidence in support of this conclusion.

The required total amount of food imports was not necessarily very large. The quantity would depend on how many soldiers were involved, but at least some foods, such as meat and perhaps cheese, must have been available to its use. But there is no reason at the moment to reject the interpretation.

\begin{thebibliography}{99}
\bibitem{276} Cf. Tacitus, \textit{Hist.} V, 23, 3; see also above, p. 398 and note 144.
\bibitem{277} See above, p. 240–1 and 400–1.
\bibitem{278} See chapter 8, p. 232.
\bibitem{279} Above, p. 400–1 and note 165.
\bibitem{280} Brandt 1983, 135.
\bibitem{281} If the interpretation of the evidence itself should not be correct there is also a difference, because then the Frisii apparently had much more Roman pottery than the Batavi, which would provide strong support for the idea that the latter were averse to its use. But there is no reason at the moment to reject the interpretation.
\bibitem{282} E.g., Von Schnurbein 1982, 132–4 or Fulford 1984, 135.
\bibitem{283} See also chapter 6, 184 and above, p. 399 and note 155.
\bibitem{284} See below, p. 424 f.
\bibitem{285} Teunissen/Teunissen-van Oorschot 1980; Teunissen a.o. 1985.
\end{thebibliography}
able locally. It would also depend on the type of the troops concerned, for it is very likely that the auxiliaries used more elements of the native diet than the legionaries, who were recruited entirely from the Mediterranean in pre-Flavian times. Most of the auxiliary units, though not all of them, were Gaulish or Germanic troops. It is remarkable that an increase in cereal production in the river area can only be associated with legions: the sudden increase in Nijmegen at the time of the Augustan campaigns mentioned earlier, and the increase after the arrival of the 10th legion in the river area as a whole (discussed below). The implicit causal relation in this observation is perhaps more logical than the assumption that no legion was stationed in the river area because there was not enough food locally. Had there been compelling reasons to station a legion there, the food problem would have been solved.

Apart from the troops and their direct needs, the changes in the mid-1st century, perhaps even slightly earlier, may have also led to the expropriation of tribal land. The question of a military territory has already been touched upon in foregoing chapters. As far as archaeological data are concerned, there is no real evidence for the existence of large coherent military territories. As demonstrated in paragraph 6.5.1, in this respect tile-stamps are of no value, certainly not in the river area. The distribution of settlements as such does not provide any clues either, because there are native sites everywhere, even in the two areas which would first come to mind: the banks of the Rhine and the ice-pushed ridges south of Nijmegen. There is one example of a settlement on the Rhine which may have been moved in the mid-1st century, but the reliability of this evidence is very minimal indeed. The lack of sites on the Nijmegen hills can be attributed to the rather infertile soils there and to the deficient state of research as well as to difficulties in surveying the lower-lying areas. The only potentially relevant observation regarding the distribution of settlements is the near absence of villae along the Rhine. Strictly speaking, this can only be observed after AD 70 or, more correctly, in the 2nd century, but its relevance can be extended into pre-Flavian times. Above, the distribution of villae was approached from another angle, namely, their concentration along the Waal and also the Meuse and their apparent relation to larger centres. But their near absence along the Rhine is nevertheless remarkable, even though villae in the vicinity of forts and fortresses are found elsewhere in Germania Inferior and other provinces. Apart from this perhaps not very meaningful observation (see below, note 371), and in the absence of relevant inscriptions or boundary markers, possible evidence for military territories is lacking.

In a too often neglected paper Vittinghoff has made an important contribution to the question of military territories by showing that many aspects of the concept of a militärisches Nutzland, a large and coherent area for military use and under military administration, are not as acceptable as sometimes assumed. There are, apparently, no good reasons to assume a military administration, nor large military territories with a permanent status as such. In Vittinghoff’s opinion, military and related installations were situated in selected areas on ager publicus and used by the army ohne dass damit zugleich hohetliche Verwaltungsfunktionen über Personen und von anderen Besitzern genutztes Land eingeschlossen wären. In principle, a provincial governor or, on his orders, a le-

286 The results of the archaeozoological analysis (cf. chapter 1, note 8) have to be awaited for possible further proof. At least in Velsen, the troops ate beef obtained from the small indigenous species of cattle (Brandt 1983, 135).
288 Chapter 6, 190–2 (in the context of a discussion about the significance of military tile-stamps) and chapter 8, 239, 260.
289 As the discussion of population numbers in paragraph 11.3.2 has shown, the banks of the major rivers are the probable location of quite a few missing (eroded) settlements in addition to those surviving (see Appendix 3 or fig. 62). An excavated, fine example of a native site near a fort is Wijk bij Duurstede-De Horden (Van Es 1981, 101–3 and 1982, esp. 153).
290 This area especially after AD 70 and the arrival of the legio X gemina.
291 Site 132; see above, p. 239–40. Bloemers (1978a, 97) has suggested a similar forced evacuation for a few settlements along the Rhine in the coastal area.
292 See paragraphs 7.2 and 3.2.2 plus fig. 9.
293 See p. 281.
294 A number of instances have been assembled by Von Petrikovits 1979, 232–3.
295 The cippus of the legio X gemina from Nijmegen (ER II, 139) is apparently dubious (Rüger 1968, 53, note 250).
296 Vittinghoff 1974.
297 See Rüger 1968, 51 f., which has been very influential in recent literature, and the important paper by Von Petrikovits 1979. A recent overview of other literature in Sommer 1984, 13–4.
298 See also chapter 8, p. 268 and note 169; compare Von Petrikovits 1979, 236 and 242.
299 Vittinghoff 1974, 117.
gionary or auxiliary commander could always expropriate land for military purposes by a de facto measure that could be reversed when no longer needed. No large, coherent, and well-defined areas were necessary. Despite Vittinghoff’s detailed arguments, some of his conclusions perhaps need modification. For example, the striking continuity in the area originally reserved for troops on the Hunerberg in Nijmegen has often been observed. There is much to say for a military area all along the northern edge of the ice-pushed ridge in Nijmegen from the Valkhof area on the Waal over the Hunerberg and Kops Plateau to (after AD 70) the tile-works at the Holdeurn (site 433). But then this is only a small strip of land, some 6 km long, in a formerly (nearly) uninhabited area. It is possible that there was a much larger territorium legionis covering the entire area between Nijmegen and Cuijk in the west and Rindern and Qualburg in the east, or a smaller one between Nijmegen and Rindern, but this is pure speculation. As long as a legion was stationed in Nijmegen, an extended area reserved for use as training grounds, such as that from the Xanten legion(s) in Alpen-Veen, can be expected although not necessarily on the Pleistocene hills. The rather infertile hills may have been used by the army as well as natives without the necessity to assume a well-defined military territorium; it all depends on the opinion one holds about the true nature of such territories.

For the Holocene area, however, this is a different matter. This was a densely settled region, largely deforested by the end of the Iron Age, and used by the native population for agricultural purposes. Even without Vittinghoff’s arguments it is inconceivable that a huge part of these lands, and the entire Betuwe, should be militärisches Nutzland. On the other hand, troops in the forts did need grazing land (prata), as is testified from various parts of the empire. It is reasonable to expect that parts of the stream-ridges along the Rhine and especially the (uninhabitable) flood-basins were indeed expropriated to serve as prata cohortis, thereby contributing to the Batavian resentment which is evidenced by the revolt. The later absence of villae along the Rhine may be an indication of this, when interpreted as evidence that such private enterprises could not be established there because the land was already used by the army. But the only area for which there are reasons to presuppose military land are the banks of the Rhine where the forts were located and the limes road must have been constructed. All we can say about this zone is that it must have been at least a few hundred metres wide and definitely encompassed native settlements, which, according to Vittinghoff, would mean that it was not continuous.

Although even this minimal estimate, when true, involved hundreds of hectares of agriculturally valuable soils and is sure to have upset at least part of the population, the question remains whether it was large enough. From a historian’s point of view, Vittinghoff has argued that while the army was involved with animals and needed grazing lands as well as functionaries to supervise the animals, it did not engage in arable farming to cover its own needs locally. As the pollen evidence from the Kops Plateau has shown, this is not entirely true even for an Augustan army on campaign, although much food was imported. But the consumption of cattle and quite probably other animals was not necessarily covered by army production either. On the contrary, many of the animals are indeed likely to have been obtained locally from the native population. A miles pequirarius can then be seen as a soldier in charge of a

301 E.g., Bloemers 1979a, 27. On the other hand, it should be remembered that the discontinuity between the Augustan and Flavian occupation is not yet completely certain. On other sites, such as in Meinerswijk between periods 1 and 2, there may have been continuity. The example from Cuijk (sites 499/500) shows that an originally military area could be used by civilians even though the site as such remained militarily significant as shown by the (assumed) statio and the (demonstrated) late-Roman fort. See also Petrikovits 1979, 243, on the civilian use of former forts and fortresses.
302 As in Rüger 1968, 37, fig. 1.
303 In this area, a large number of practice camps have been discovered by air photography and partly excavated. See esp. Scollar/Andrikopoulou-Strack 1984 and Hinz 1984.
305 Op. cit., 120: prata means only grazing land and no functionaries involved with arable land are attested.
306 Kops Plateau: above, p. 393. See also below, p. 424. In addition, there is a reference perhaps to military arable land (agros vacuos et militum usui sepositos) by Tacitus (cf. note 309) although the further context refers more to grazing land (see below), and there is epigraphical evidence in the form of an inscription of the legio I adiutrix on a plough discovered in Eich, Kr. Alzey-Worms (Von Petrikovits 1979, 232).
307 See note 286; the famous – probably Tiberian – deed of purchase from Tolsum (ER II, 396).
308 CIL III, 10428.
more limited group of primarily draught-, carry-, or riding-animals, with additional herds of slaughter cattle and other animals depending on local circumstances. There may be vast differences in this respect between different areas or in the same area during different periods. An evaluation of the situation in the river area, and developments therein, may be possible in the near future when the archaeozoological research has been completed.

Apart from the above considerations, there is an additional argument for assuming only limited territories for military use in the Batavian area. This is the fact that we know from literary and epigraphical sources that there was a zone immediately beyond the Rhine which was kept clear of native settlements and reserved for military use. This is even specified, namely, the apparently rather extensive use for grazing *pecora et armenta mili tum*, the flocks and herds of the soldiers. This is confirmed by the inscription from Menden, which records the extension of the *prata* of the 1st legion beyond the Rhine, perhaps as late as the first half of the 3rd century AD. This shows that there is every reason to assume that, throughout the middle-Roman period, a zone beyond the Rhine was maintained undoubtedly for security as well as economic reasons, and also as a way of avoiding too much interference with native tribal property. As pointed out by Von Petrikovits, the extension of the *prata* as such also shows that the zone beyond the Rhine was not a continuous one. Unless one is prepared to accept the unlikely proposition that one *prata* was enlarged at the expense of another unit’s land, the conclusion must be that there was ample empty, or rather ‘not military’, land in between the areas reserved for military use.

That the area in between was not entirely empty is also shown by the few settlements north of the Rhine in our region (see Appendix 3 or fig. 62). The measures recorded by Tacitus (note 309) were directed against whole tribes settling in military lands and do not at all imply that nobody was allowed to settle (or continue to live) there. The people from the military zone beyond the Rhine were apparently considered inhabitants of the empire and in close contact with the army. In addition

to evidence for this from elsewhere, the military tile-stamps on two clearly native settlements across the Rhine from the Betuwe should be mentioned in this respect. As mentioned in paragraphs 3.4.2 and 6.5.1, both sites were located along an undoubtedly frequently patrolled land-route to the north. But they are not military sites themselves.

From the little and usually indirect evidence there is about land for military use, we may conclude that from about the mid-1st century onwards this included at least a small, not necessarily entirely continuous, strip of land along the Rhine, presumably a few limited parcels around existing forts, and probably somewhat larger areas north of the river. At least after AD 70 the (small?) territory on the Pleistocene hills at Nijmegen must have been enlarged, conceivably also over some of the Holocene river area and notably the present-day Ooijpolder and Duffelt. But if the treaty with the Batavians was indeed renewed on the old conditions and favourable for them from a Roman viewpoint, this is not very likely. In any case the palynological evidence indicates that after the arrival of the 10th legion the hills were once again cleared and used primarily as grazing land, which shows that there, at least, we may postulate a *territorium legionis* (of unknown size) with some confidence.

Socio-economic structure and development

During the quarter of a century preceding the revolt, economic conditions in the river area remained comparable to the earlier situation as far as the archaeological evidence is concerned. The various and for the population decidedly negative changes discussed earlier, no matter how much pressure on social and economic life they may have caused, cannot have brought about fundamental changes in such a short period of time. This is confirmed by the archaeological, palynological, literary, and other evidence we possess. On the other hand, the revolt itself should not be considered a caesura. Instead, it was a native reaction to pressure which led to a use of force and further, presumably much more conscious and precisely aimed pressure.

As both literary and archaeological data show, the almost complete destruction of the Roman superstructure
during the revolt had only a positive effect on that structure: it was rapidly restored, ameliorated, and expanded in a thorough Flavian manner. These measures have already been referred to in paragraph 11.4.1, and are reflected by an ambitious building program that included the new foundation of a civitas capital at site 399 in Nijmegen. As mentioned in chapter 8, it is questionable whether this settlement ever developed into a fully fledged town even though it later became a municipium. Nevertheless, the limited excavation data, stray finds, and the size of its cemetery (site 398, Hees) indicate a considerable population and size in the 2nd century. The Flavian foundation can be interpreted in direct connection to the reinstatement and perhaps further reinforcement of the Batavian civitas. At a supra-regional level, the administrative situation was also normalized by Domitian's formation of the province of Germania Inferior.

As argued earlier, the decline of the native population should also have turned into an upward trend again in this period, although this is very difficult to demonstrate archaeologically. The clear increase in the dispersal of Roman pottery from the late-1st into the 2nd centuries might be taken as an indication, but an interpretation in purely economic terms is much more likely as a main cause of this development. The troops stationed in the river area, and other immigrants, not only contributed to a general increase of people living in the region but undoubtedly also to the growth of the native population. Ironically, the only pertinent literary evidence indicates that soldiers were interested more in handsome young Batavian boys rather than girls, but the practice of soldiers (illegally) having native wives is known to have been widespread and tolerated. The best evidence for population growth apart from the large cemetery of Ulpia Noviomagus is the number of settlements. In paragraph 11.3.2 it was concluded that for the eastern river area in the mid-1st century (and by implication during approximately a century before the revolt), the number of settlements must have been around 400. For the Middle-Roman Period, when the wide distribution of wheel-turned wares allows a more direct estimate based on archaeological and geological data, a maximum number of about 500 settlements has been inferred in chapter 8. In contrast to all other phases, the chance of a grave underestimate of the number of settlement sites is not very large in this case, and the original number was, if at all, only slightly higher than this maximum estimate. If the average number of inhabitants per site remained constant and if the increase in the number of sites is valid for the entire civitas, this would mean that there was a population increase of about 25% from the 1st to the 2nd and early-3rd centuries, discounting soldiers. When the population of Noviomagus and a few other larger settlements (vici, see paragraph 8.4.2) is taken into account, even a somewhat higher increase could be acceptable.

The basic assumption of a constant number of inhabitants per site is, however, very difficult to substantiate in the absence of enough sufficiently analysed excavation data. The growth of a settlement such as Rijswijk-De Bult might be used to reject the assumption but the site need not at all be a typical one in this respect because it developed into a villa. Conversely, it can be argued that the changing economy – to be discussed below – and the relative security could have led to a dispersal of formerly more concentrated population groups which would result in more but generally smaller rural settlements. Indications for a suddenly increasing number of truly isolated farmsteads are, however, lacking. A well-investigated site such as the villa settlement at Druten (site 214; see fig. 78) may have housed a few families and at present we can only assume that its total population was not very different from an average late-Iron Age or early-Roman site.

On the whole, with an increase in the total number of sites and the accretion of at least some of them, a population growth in the order of magnitude of one-fourth is an acceptable proposal. This would bring the 2nd-to early-3rd-century population of the civitas Batavorum to about 50,000 people (in reality less when c. 5500 men were taken permanently into the army) not counting the army units. Based on Alfeldy's figures, these may have amounted to 21,000-22,000 soldiers for the entire province, one-third of whom should have been stationed in the present-day Netherlands and for the Batavian
stretch of the *limes*, roughly two-thirds of this, that would mean approximately 5000 men. As fig. 130 shows, there are more than enough forts for this number which agrees with the circumstance that some or probably many of these forts did not house entire or regular-strength cohorts. From the approximately equal number of troops posed by the Batavians and the number of troops stationed in the *civitas* in the 2nd–3rd centuries it may not be deduced that all new personnel was recruited locally. As mentioned in paragraph 11.4.1, Batavians were still being sent to their own national units and they also served in other units elsewhere. But many of them would indeed have served in their own province.

The total number of people living in the *civitas* can thus be estimated at roughly 50,000, while in Flavian times, when the increase had yet to begin, that number was not very much less due to the presence of more troops including the 10th legion at Nijmegen. At that time, the eastern river area was thus by far the most populous and must have profited the most from the Flavian investment program when distrust or resentment did not interfere. Recent estimates for total army costs amount to somewhat over a hundred million denarii a year for the empire as a whole. According to MacMullen's pay figures and Alföldy's unit numbers, it appears that under Domitian a minimum of 10% of this total was spent in Lower Germany, and in the 2nd century still at least 5.5 million denarii. With one legion and the large number of auxiliaries (a total of ten of normal strength?) we can safely assume that under Domitian the Batavian area accounted for one-fourth of the total, which amounts to at least 2.5 million denarii yearly. In the 2nd century, with only the auxiliaries left plus, for some time, a detachment of the Xanten legion in Nijmegen, the army pay spent annually on troops in our area can hardly have been more than 1 million denarii and probably somewhat less than that. Even though the figures mentioned above are only estimates, they do indicate quite clearly the relative amount of state expenditure and then only for the army. If Hopkins' estimates of the empire's gross tax revenue are correct, the cost of the army was somewhat less than half this revenue. This means that in Flavian times no less than 1% of the entire income of the whole Roman empire was spent in the *civitas Batavorum*, in the 2nd century still around 0.5%, or somewhat less if the tax income increased, which is in fact probable. From the approach used by Fulford for Britain and Hopkins' average tax contribution of 15 sestertii per individual at a 10% tax rate, it follows that in Flavian times the tax product of some 670,000 people was expended in the Batavian area, and later that of 270,000 which is still at least five times its total population. These figures demonstrate the obvious in that they show that the area did not pay for itself. They are more properly put in context at a supra-regional or rather imperial level, as was done by Drinkwater who demonstrated that the entire expenditure in the two German provinces is approximately equal to the revenues from the three Gauls. Although this does not prove a true 1:1 relationship because on the one hand wealth was accumulated from the Germanies also and on the other Britain was probably economically dependent throughout the 2nd century, it illustrates the mechanism of tax-exporting and tax-importing provinces already referred to in paragraph 11.2.2. Nevertheless, under average conditions we should expect that as a general principle conscious ef-

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322 Alföldy 1968, 156–7 and 162.
323 E.g., Bloemers 1978a, 125. On the other hand, the interpretation of some of the indicated sites as forts is still to vary degrees hypothetical.
324 Alföldy 1968, 104.
325 MacMullen 1984, 572; Hopkins 1980, 117 (the same amount calculated in sestertii). Figure valid for the reigns of Domitian up to Septimius Severus.
326 MacMullen 1984, Appendix, and Alföldy 1968, 151: In the early-Flavian period, 6 alae + 2 cohortes mill. + 1 cohors mill. eq. + 12 cohortes eq. + 7 cohortes quing. + 4 legiones = 6 × 418 BP (BP = 1 unit 'base pay') + 2 × 418 BP + 468 BP + 12 × 234 BP + 7 × 209 BP + 4 × 6622 BP = 34,571 BP, at 1 BP = 300 denarii is more than 10 million denarii. For the 2nd century, the figure is 18,410 BP of 300 denarii is over 5.5 million denarii. Alföldy's figures are taken to be the minimum estimate of the number of auxiliary units.
327 Hopkins, op. cit., 116 f.
328 Fulford 1984, 130 f. There is much to be said against this approach, and notably the fact that the Batavians are known to have paid no taxes (which must be accepted at the very least for the entire 1st century), but the calculations only serve to put expenses in perspective.
329 Drinkwater 1983, 65. Evidently, this pertains only to the 2nd century and not to the enormous expenses in Flavian times.
forts were made to make areas pay for themselves as far as possible. And other circumstances should have worked in the same direction, such as the technical limitations to the transfer of matter and energy under the then prevailing conditions and also the boost to local economic systems which must have resulted from the input through the army.

As demonstrated above, in our area this input, when army pay is taken as a yardstick, was considerable from the mid-1st century onwards and reached truly gigantic proportions in Flavian times. But in the 2nd century it was reduced to about (very) roughly one-third of the Flavian level and that reduction was quite sudden as well. It occurred within a decade at the beginning of the century and must have had a considerable impact in the region. That this was indeed the case can be deduced to some extent from the analysis of regional developments discussed below. But perhaps there is a much clearer illustration possible, namely, the development of the civitas capital Noviomagus.

As argued in chapter 8, the regional centre is in many ways a curious town. Despite the deficient state of research, there are several reasons to suspect that it was an ambitious project in Flavian times, but that it probably lacked a number of facilities (such as an amphitheatre) which should normally have been present. Also, we know for certain that within the perimeter in the first half of the 2nd century there was a pottery industry, which is entirely incompatible with a dense occupation. Also, and even though some tactical advantages are obvious and there could be other reasons, it is nevertheless remarkable that the town was given up around AD 270 in favour of site 403 (Valkhof). Perhaps it is also no coincidence that the excessively wealthy burials recently discovered at the town's cemetery in Hees (site 398) date to the Flavian period. There is nothing even remotely comparable to the amber, rock-crystal, and other precious artefacts from these graves in the huge collection of later finds from the cemetery assembled by Brunsting.

While the available evidence does not allow us to reject the identification of site 399 with Noviomagus there is, in the present state of the evidence, every reason to suspect that Flavian investments did not lead to a further development in all respects. Neither Trajan's granting of the ius vendarium nor the later elevation to the status of municipium succeeded in bringing about an independent socio-economic development into a complete Roman town and central place for - and supported by - the civitas. Military stamps, not only from the 10th legion but also 2nd-century specimens, are evidence for continuing involvement (in whatever form) of provincial authorities. Thus we arrive at the hypothesis that Noviomagus, while started in high spirits under the Flavians, remained 'incomplete' as the result of a significant drop in state expenditure in the region. The implication is that the town was to some extent artificially maintained instead of self-supporting in later times.

That there are indeed reasons to think so can, apart from the aspects mentioned so far, also be deduced from a tentative analysis of the regional settlement system as discussed in an earlier paper, and based on the application of the so-called 'rank-size rule' as developed in geography. Essentially, this is an empirical observation of regularity in settlement-sizes. In its basic form, it states that the size of the nth settlement is 1/n the size of the largest settlement in the system. Thus, assuming that the largest settlement has a value of 100 and the system conforms to the rule, then the second settlement would be 100/2 = 50, the third 100/3 = 33, and so on. This re-

It is perhaps quite significant that of the three or four known town councillors one or two had an unknown occupation while the two remaining individuals were traders. Drinkwater (1983, 199), assuming there was only one such case, a Treveran decurion, attributed this to the exceptional situation after the revolt and the short-lived Imperium Galliarum. Nevertheless, his conclusion that traders were not normally town councillors remains valid. The evidence for Nijmegen can thus also be taken as an indication of an exceptional situation, showing that the town badly needed such people and that things were not going well early in the 3rd century.

331 See paragraph 8.4.2, p. 265 ff.
332 Excavations have been limited and no wall, ditch, or other bordering structure was known before September 1985, when excavations by the JOGA (U. of Nijmegen) for the first time revealed the existence of a wall and ditch.
333 Bogaers 1979b, 62.
335 Brunsting 1937. A later sarcophagus from site 397 might belong to the cemetery (see above, p. 298) and there is a rather wealthy burial from the early-3rd century as well (Brunsting, op. cit., 184, no. 25), but comparably rich burials are only those from Esch (Van den Hurk 1973, 1975, 1977, and 1980) which date from the later-2nd and early-3rd centuries.
336 Bogaers 1960-61, or 1972a, 312-18. See also note 393.
sults in a lognormal distribution which, if the values on the axes are transformed to a logarithmic scale, appears as a straight line with a gradient of 45° to the horizontal. Several explanations of this regularity have been advanced. Their rationale is based on Simon, who demonstrated that lognormal distributions are the end-product of a large number of stochastic growth processes. This view is criticized by Johnson, who argues that a linear distribution is not necessarily a limiting case (a systemic steady-state) or that, conversely, non-linearity need be indicative of system change as is implied by, for example, Berry's work on economic development and Crumley's hypothesis on urbanization. He suggests that lognormal rank-size distributions are the product of highly integrated settlement systems, while non-linear distributions are produced by a relatively low degree of integration.

There are two non-linear distributions: a convex and a concave (or 'prime') distribution, the first one having one or more too large settlements while the second is characterized by one dominant, or primate, settlement. Both these distributions are in some way a product of boundary definition. Thus, an almost classical interpretation of a primate distribution is its relation to a colonial system, but this only holds true as long as the system is defined by the boundaries of the colony. The primate settlement is (at a higher level) also part of a larger scale system which may well have a linear or convex rank-size distribution.

A more serious 'cause' of a primate distribution – for analytical purposes – is the deletion of the middle range section (the second, third, and further settlements) from an otherwise linear distribution, either because they have not been located or because they are outside the boundaries of the region under examination. On the other hand, effective control from a centre may have

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339 A review is given by Richardson 1973. See also Johnson 1977, 1981.
342 Berry 1961; Crumley 1976.
suppressed the development of such middle-range settlement, a situation which again reminds us of the colonial situation. The resulting primate distribution is normally connected with dendritic settlement systems (fig. 134) in which lower centres are tributary to only one higher level centre. 344

Estimates of settlement sizes in the Batavian area, whether expressed in hectares or numbers of inhabitants, are very difficult to give as will be evident from the sites discussed in previous chapters. The three curves in fig. 132 can therefore only be regarded as highly tentative, 345 but fortunately that is not a serious impediment to further use. The shape of the curve is determined primarily by the size of the first versus those of the largest other settlements and there is no way in which the primacy of the most relevant curve, that for the mid-2nd century, can be changed into something approaching linearity. 346

In the 2nd century we are thus dealing with a marked primate settlement distribution. Because there is not the slightest chance of missing middle range settlements these are indeed absent. The primacy of the system could, in principle, result from the fact that Nijmegen was differentially articulated with the larger scale system of the province and thereby with that of the empire, which would have increased its relative size. Interestingly enough, however, the provincial curve as such is neither linear nor concave either (as could have been expected for a frontier province). As fig. 133 shows, it is a convex distribution, 347 indicative of low system integration and as such of the rather artificial nature of the province from a socio-economic point of view. In fact, however, it may well be the result of the pooling of separate systems (constituted by the civitates) as described by Johnson. 348 Each of these systems, except perhaps that of the Cananefates with its tiny capital at Voorburg-Forum Hadriani, has a primate distribution even more marked than for the Batavian area.

Another possible approach to understanding the observed primacy is to view it as reflecting 'the political administration of an economy in which competitive forces, necessary to a regular commercial central-place hierarchy, are minimized'. 349 The central place is actively promoted in this case, and the development of middle-range centres effectively suppressed. Trajan’s inferred granting of the ius nundinarum to Noviomagus, hence Ulpia Noviomagus, could be a splendid illustration of such a process.

345 An account for their composition is given in Willems 1983a, 117–21.
346 This holds good even when the composite primate site Nijmegen is redefined as site 399 and a very pessimistic estimate of its population is employed, e.g. 3000 or 4000 (a quite reasonable estimate forwarded by Bogaers 1979b, 62, arrives at 5000 inhabitants). There are still no middle-range sites nearly large enough to fill the gap: without counting soldiers it is very questionable if there is another site even reaching 500 inhabitants.

347 Data on settlement sizes from Bechert 1982, 100 f. Note that the presence or absence of Tongeren, with a size slightly smaller than Xanten and a disputed status as to which province it belongs (Bogaers 1972a, 326 f.), only makes a difference in the degree of convexity, not in the shape of the curve. The indicated towns are the civitas capitals except for Aachen, whose administrative position is uncertain (see Von Petrikovits 1978, 118, Bechert 1982, 115, 117 f.; also Rüger 1968, 99 f.). Although larger than Voorburg, it was apparently not a chartered town but a vicus.

The above discussion has, of course, wider implications than showing the 'artificially maintained' character of Noviomagus or, for that matter of the province. When the provincial rank-size curve is correctly interpreted, Germania Inferior appears not as an integrated socio-economic system but as the product of a decision at the core for administrative, political, and military reasons binding together a number of weakly linked individual systems. The same weak linkage is also a characteristic of the relation between the secondary centres in a dendritic settlement system. A model of such a system is presented in fig. 134, and its characteristics have been analysed by Smith (cf. note 344). Important is especially the flow of goods which is vertical, to and from the primate centre, and only very limited between different areas (horizontal). The central place thus controls prices for rural goods and the distribution of commodities in the countryside, and there is no true competition in service functions. One of the consequences of this system is that rural producers near the primate centre are at a considerable advantage.

As indicated elsewhere, there is evidence for such a dendritic settlement system in the Batavian civitas. On fig. 135, the regional centre and a number of possible centres, and only very limited between different areas (horizontal). The central place thus controls prices for rural goods and the distribution of commodities in the countryside, and there is no true competition in service functions. One of the consequences of this system is that rural producers near the primate centre are at a considerable advantage.

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secondary centres have been indicated. Those in the area covered by the present study, Elst (site 105), Wijchen (site 315), and Cuijk (site 500) were discussed in chapter 8. They are interpreted as vici, and their location in different landscapes and in a half circle around Nijmegen almost by itself suggests an interpretation as secondary centres with their own hinterland. There is thus every reason to consider these different regions as pagi. Although there is yet only one certain piece of evidence for a pagus in Lower Germany, there is no reason to reject their existence. The relative proximity of the three vici to Nijmegen already suggests that the remaining parts of the civitas should have had their own centres. One such area is the western part of the civitas, bordered by the moors of the parimarine area (Holland peat). Especially because of its central and strategic position the site of Rossum (= Grinnes?), already often mentioned in different contexts above, is a likely candidate for a secondary centre there. Although its hinterland is less of a geographical unit than the others, its position is such that it could have functioned excellently as a central place for the areas north and south of the Waal.

The fifth region is constituted by the Pleistocene sand area south of the Meuse, in fact the drainage basin of the tributaries of the Meuse there. As repeatedly argued above, this region must be considered part of the civitas Bataavorum and presumably bordered on the Texuanian area on the plateau around the present-day frontier between Belgium and the Netherlands (see fig. 122). The unmistakable centre for this region is the vicus of Halder, in the immediate vicinity of the findspot of the altar dedicated by Flavus (fig. 127). Especially in Halder, but also in Cuijk and Wijchen, there is evidence for small-scale industries. The altar dedicated to Magusanus Herculis, the temple at Elst, and two smaller Gallo-Roman temples at Cuijk are indications for religious centres. Administrative involvement may be deduced from fragments of a monumental inscription found in Cuijk and mentioning Trajan, and the military stamps found on all sites except Halder could point in the same direction. All of them are, in fact, not just favourably but truly excellently situated in relation to traffic over land or water and obvious locations for such establishments as stationes beneficiariorum consularis, mansiones, or mutationes, or combinations of these. With the possible exception of Elst, none of the presumed pagus centres exceeded 10 ha in size. Although wealthy individuals could, and in fact did, live in all parts of the civitas, the dendritic nature of the system may also be deduced from the relative wealth in the region around Nijmegen. It is difficult, as long as recent overviews for the central river area and the Province of Brabant are still lacking, to substantiate this quantitatively. But it is nevertheless a long-recognized fact that the quality and quantity of Roman artefacts is superior in the eastern river area. In addition, there are other data such as the fact that in the 2nd century AD the only region where native pottery may have ceased to be used entirely or at least used to a far lesser extent than elsewhere is the eastern river area. The number of villae is also largest here, and includes the only certain villa urba- na (site 377, Mook-Kloosterberg I) and the only one with a mosaic (site 239, Ewijk-De Grote Aalst). But the best indication is probably the variety and quantity of Roman artefacts, notably pottery, found also at relatively simple native settlements.

Change
The Batavian area thus appears to have had a little balanced and non-integrated socio-economic system with industry. According to J.E. Bogaers (pers. comm.), the distribution of the stamps points to a more southerly located production centre.

352 Above, p. 271.
353 Cf. Rüger 1968, 101–2, or, for Britain, Rivet 1975, 112–3 or Crickmore 1984, 13 (with further references) and 45 f. The inscription from Lower Germany (ER II, 33; see Van Es 1981, 235, fig. 180) mentions a pagus Catus[inus].
354 E.g., p. 371, 396–7, or 410.
355 In principle, it may also have included some of Plinius’ (Nat. Hist. IV, 106) Texuandri pluribus nominibus. The Texuandri as such presumably belonged to the civitas Tungrorum.
356 On Halder, see Willems 1977 with further references. The particular kiln discussed in this paper is, by the way, not very likely to be from the mortaria-manufacturer Adiutor as was tentatively suggested. As mentioned by Peacock (1982, 94–5), mortaria are normally produced by a different kind of
distinct elements of artificiality and obvious connotations to colonial systems. It differs clearly from more integrated systems, such as, for example, the often discussed situation in southern Britain, which has been described as a ‘transport based’ as well as an ‘administered’ or ‘solar’ central place system.  

Both at civitas and provincial levels we are dealing with administratively bounded but essentially non-interconnected units, each through its centre connected to one higher level centre. Units can be added or disconnected relatively easy in this situation, as perhaps they were. What determined the organization along the frontier was thus largely the result of administrative and military measures as perceived to be necessary at the core, and from the primary motivation to ensure imperial security and thus to be in complete control.

To reach this goal it is understandable that in economic terms input exceeded the output at a regional level, because at the level of the empire the gains in terms of security were very valuable indeed. On the other hand, the whole complex of developments in the 2nd part of the 1st century did, of course, lead to various and fundamental changes. One important change is that in the rural economy, which, as outlined before, was originally dominated by cattle-breeding. There are several indications that this essentially Iron Age system started to change after AD 70.

One important clue is, of course, the appearance of settlements which can only be described as villae. On the basis of literary evidence, it is sometimes assumed that a kind of villa system with large landowners already existed before the revolt. However, although it appears that Civilis had his home in the Betuwe and undoubtedly, being praepotens inter Batavos, that home was an important settlement, the nature of tribal society as discussed earlier is not compatible with such large private landownership. Civilis may have controlled much land and many people through his large clientela, but it is difficult to see him leading such a strongly nativistic revolt when he was a Romanized landlord residing in a villa. His residence itself is also not likely to have been very different from others in its material aspects. In addition, the term villa has of course not the same meaning as in modern scientific parlance. Apart from these arguments, there are no settlements classified as villae which are datable to pre-Flavian times.

The evidence on villae as such was presented in chapter 8 and need not be repeated here. Site 214 in Druten (see fig. 78) is a good example of the kind of Romanized settlement with native roots that started to operate in the Flavian period, but there are wealthier and also later founded villae. Although the villa developed further in the early-2nd century, this quick start of apparently rationalized arable production (there are no indications to assume a specialization in stock-breeding in the excavated plans or faunal material) is surprising. It can only be understood when it is realized that there was not just a demand for the products but that the native economy was not yet geared to arable surplus production and that too much compulsion was not very feasible so soon after the revolt. Instead of enforced changes with dubious results, a much more reasonable assumption is that some more Roman-minded — and such people are even attested to during the revolt — and more enterprising types started afresh on a new site. There is every reason to believe that, even if they were not actually induced to it, such individuals surely received official support and encouragement. This may explain the rather strong degree of affiliation with the army observed on these sites, as evidenced by the presence of military tiles or fine wares (fine Nijmegen ware and quantities of terra sigillata. Although the excavation evidence is not nearly large enough to be quite certain in this respect, there may be a significant difference in the origins of villae, one kind being founded by native entrepreneurs on new sites soon after AD 70 (the ‘Druten type’) and one resulting from Nash 1984, 101. The lack of differentiation between settlements discussed in chapters 8 and 10 provides archaeological support.

A similar use of the term must be the ‘villa’ of Cruptorix in Frisian territory, where 400 soldiers died during the Frisian revolt of AD 28 (Tacitus, Ann. IV, 73).

Above, p. 272–81.

See esp. the discussion of various classes of artefacts in chapter 6 and also paragraph 8.4.3.

Chapter 6, 170–1.

Cf. paragraphs 6.5.1, p. 191 and 8.4.2, p. 278.
from more gradual adaptation and acculturation processes and growing out of native settlements in the 2nd century (the 'Rijswijk type').

Although the surface material collected at various villa sites does not allow generalization, we may even go one step further than this on the basis of the Druten and Rijswijk excavations. The fate of the Druten establishment is very clearly linked to that of Nijmegen. It started after AD 70, its owner grew wealthy and rapid improvements in the facilities can be observed which led to its developed form early in the 2nd century. In principle, it may thus have been a one-generation achievement by the original owner. But already after the mid-2nd century there are changes and the site was abandoned before or around AD 200. When viewed as a commercial enterprise dependent on deliveries to the army, this fate is understandable although its early demise remains unexplained.

In contrast, the villae of the Rijswijk type, most of them much more villa-like architecturally than the type site to judge from the materials found on eastern river area sites, were built after some internal developments in native society. Most or all of them were also production sites functioning by means of surplus production in a more or less commercial manner. But especially because of their native roots they can also be seen as status symbols, to display the wealth as well as the 'Romanness' of their owner. Although epigraphical evidence is needed to prove this, it is entirely conceivable that some of these owners acquired their wealth by other means (military or administrative career, trade) than agrarian production.

The above, rather hypothetical, conclusions do indicate that 'changes in the economy' must not be treated simply as economic change but put in the context of a socio-economic development. As in every society, there were Batavian entrepreneurs willing to change fast and capable of successfully doing so. But there was never any revolutionary change in the economy and Batavian lands were never able to provide the necessary food for all the soldiers stationed there. In addition to the degree of observed change, which will be discussed below, other information clearly shows that at least part of the army's food was always imported. A good illustration of this import is provided by a recently excavated Roman barge in the former Rhine channel near the fort at Woerden-Laurum. The barge must have sunk in about the last quarter of the 2nd century and the remains of its cargo were found on its floor. This consisted of wheat (probably Triticum dicoccum) but especially important are the weeds found among the wheat (a.o. the exotic Orlaya grandiflora). These show that the cargo must have come from a limestone/loess area and therefore in all probability from the 'villa belt' stretching from Belgium over the southernmost part of the Netherlands into the German Rhineland, but in any case it did not come from the river area.

A second important clue, which fits into this picture very well indeed, is provided by the epigraphical evidence for the presence in Nijmegen of M(arcus) Liberius Victor, a Nervian negotiator frumentarius: a private trader, dealing in corn, and originating from Gallia Belgica. He dedicated an altar there to the Matres Mopates (fig. 136), quite probably to thank them for the safe arrival of a transport.

As mentioned before, there are several lines of reasoning and different kinds of data which lead to the conclusion that the native economy was rather strongly directed towards stock-breeding. The changed circumstances from the mid-1st century onwards did not lead to much economic change but, instead, created a fertile soil for a native revolt. But in the Flavian period, the increased

by the subrecent and recent land-use data from the river area (above, p. 221). An illuminating study in this respect is Hassan 1981 (esp. chapter 10), in which a distinction is made between maximum, critical, and optimum carrying capacity of an area and the relation to its population level.

371 There is no point in calculating theoretical production capacities. As mentioned in paragraph 7.2.2, there was about 300 km² of prime arable land in the river area alone and much more if the lesser quality soils are included. Even taking moderate yield figures (see, e.g., Fulford 1984, 130 for figures and further references), this is more than enough to feed the army in the 2nd century. But what is theoretically possible and what actually happens are two very different things as is also shown
number of soldiers, the building activities, the imports and trade associated with the army having so many soldiers with money to spend, the undoubtedly increased pressure to maintain and support a smoothly running ci-vitas organization, soldiers having alliances with local girls, returned Batavian recruits or foreign soldiers remaining in the area settling as veterans and Roman citizens, and many other interrelated factors started to bring about change. At first, this change is archaeologically almost imperceptible. The scarcity of Flavian, and certainly early-Flavian, artefacts on settlements can be interpreted as an indication of aversion on the part of the native population and as proof of the fact that they had not yet much to offer in return for such artefacts. But already during the Flavian period the inclusion of Roman material in native burials as well as the increased archaeological visibility of graves indicate changes in the native system of norms and values governing disposal-of-the-dead practices.

Although a peak is reached only in the 2nd century and even then strong native traditions are still evident, this must be the result of social change. Even in the absence of a proper study on changes in the native burial program both the differences in wealth between graves and the spatial configuration of graves in excavated cemeteries do show that society became far less egalitarian and more differentiated (presumably hierarchically) in the 2nd century. Despite local entrepreneurs and the obvious opportunities as well as perhaps pressure resulting from the new situation, native society as a whole was thus slow to change. The post-hole swarm type of settlement, and with it the specific stock-breeding economy, did not disappear until the mid-2nd century. Earlier, this was connected to the increase of villae demonstrating ever more arable farming. The relation of both developments is indeed obvious, and in the mid-2nd century the number of villae must have reached its maximum level while the last post-hole swarms were abandoned or, as in Ewijk (site 232), their former sites now occupied by natives living in regular houses with a byre. But this evident relation should not be interpreted in merely economic terms, as one development causing the other.

As mentioned in paragraph 11.3.2, the stock-breeding type of tribal economy is not only a subsistence strategy. It is also a way of life. The role of herds of cattle in some societies as a form of social prestige and a necessary...
element in various kinds of social relations is well known from ethnography.\footnote{See also Nash’s description of a warrior society and Thompson 1965.} For example, the status of a powerful individual can be related to the number of cattle he owns, fines for deviant behaviour and breaking specific social regulations need to be paid in cattle, and in the usually patrilineal societies a bride-(or ‘progeny’) price needs to be given to the wife’s descent group.\footnote{Fines: Tacitus, Germ. 12 (see also Much 1967, 217-8), bride-price: Germ. 18 and Much 1967, 284-5. Much mentions the fact that in Germanic society a girl was literally called ‘cattle yielding’, cf. the Homeric Greek concept ἀφανίσθαι. An interesting parallel, although taking the opposite viewpoint, is offered by the Zulu saying that ‘cattle beget children’ (Kloos, op. cit., 67). Another context is the social or ritual gifts to chieftains mentioned by Tacitus (Germ. 15), again in the form of cattle. Also, cattle (and sheep) were used to appease feuds, even homicide (Germ. 21).}

These are not commercial transactions, but part of the mutual social obligations between groups. Not surprisingly, the same phenomena such as paying fines in cattle and cattle as a bride-price are also specifically attested for Germanic society.\footnote{Both anthropological and historical data thus indicate that basic changes in the native economy imply some rather basic changes in society as well. There is, of course, nothing to prove this, but the owner of the Druten villa in its first years may well have been regarded as a kind of Romanized quisling while his later, 2nd-century, counterparts were respected and powerful individuals who, by that time, could use a villa instead of a huge herd as a socially accepted status symbol.

But in the absence of tangible evidence it is not useful to pursue this line of reasoning much further. That the economy did change is, however, a fact which can be observed by other means as well. A very important clue is given in the pollen diagrams. It is very remarkable that the strongest human interference in the natural landscape, as demonstrated by the generalized regional curve for the relation between tree and non-tree pollen (fig. 137), does not occur in the 2nd century, but long before. Deforestation reaches its peak at the very end of the Iron Age. The trend of increasing deforestation during the Iron Age is reversed and forests start growing again, noticeably during the 2nd century and markedly during the 3rd. At first this may seem rather curious, especially when a direct relation is assumed between population density (and human activity) and pressure on the natural vegetation. But the nature of the economy is, of course, of major importance.

As mentioned in paragraph 7.2.2, the river area proper consists of vast areas which are very suitable as grazing land. The flood-basins are, in addition, uninhabitable and totally unsuitable for arable. The Iron Age deforestation can therefore be attributed to the clearance of the stream-ridges for habitation, arable, and pastures with additional and rather severe reduction of the moist alder-ash forests in the flood-basins by grazing cattle. The reduction of herds would then lead to a prompt return of these forests with their rapidly growing trees. As was also pointed out by Groenman-van Waateringe in a recent paper, normal pollen diagrams give relative values which means that an absolute increase and condensing of woods alone is enough to cause the relative decrease of indicators for human activity even if they remain in reality as numerous as before.\footnote{This effect is due to the fact that the dispersal of pollen is obstructed by forests and all the more when they are less open. To this can be added that a substantial number of diagrams are from samples taken in the flood-basins, which are very suitable for this purpose.}

That this explanation is indeed the only one possible is indicated by the archaeological evidence which shows that there may have been a population increase of about 25% into the 2nd century, and certainly not a decline. This indicates that as a general conclusion we can say that while stock-breeding was reduced, arable farming may well have increased or at the very least remained at the same level from Flavian times onwards. The stream-ridges must have been used very intensively, which is rather clearly visible from the distribution of sites as well (Appendix 3; fig. 62). When the explanation for the lack of culture indicators given above is accepted,\footnote{This can} this may lead to a prompt return of these forests with their rapidly growing trees. As was also pointed out by Groenman-van Waateringe in a recent paper, normal pollen diagrams give relative values which means that an absolute increase and condensing of woods alone is enough to cause the relative decrease of indicators for human activity even if they remain in reality as numerous as before.\footnote{This effect is due to the fact that the dispersal of pollen is obstructed by forests and all the more when they are less open. To this can be added that a substantial number of diagrams are from samples taken in the flood-basins, which are very suitable for this purpose.}

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also be deduced from the short characteristic of the regional pollen zone beginning in the 2nd century as given by Teunissen: ‘Strongly declining non-tree pollen, rapidly increasing *Alnus*, moderately represented *Fagus* and *Carpinus*, minimum for grain, *Plantago*, and *Rumex*.

Further developments
and the role of the frontier
As a conclusion from the foregoing arguments, we may picture the Batavian civitas in the 2nd century as a peripheral and still rather dependent border area, although socially as well as economically increasingly tied into the whole complex of Gallo-Roman society. The original socio-economic system had, by the middle of the century, changed into a more hierarchically organized society with evident differences in wealth and social standing being expressed in the usual (Gallo-)Roman manner. The civitas superstructure, the administrators, traders, artisans, and the like, located in Nijmegen and to some degree in the various local centres in the different parts of the Batavian area, may have been supported by the food produced in the area itself, especially when much (or most) food for the army was imported. The constant and reliable financial input provided by army pay and largely spent in the centres or in and around the forts can be regarded as a major stimulus. It enabled rural producers, and especially those in the eastern river area around Nijmegen, to acquire the whole range of Gallo-Roman artefacts and some of them to grow truly rich. The people buried at the rural cemetery of Esch, the owner of the Ewijk villa (site 239), and perhaps some epigraphically attested individuals, belonged to this category. The regional centre became the base of wealthy (to judge from the fact that they were *decuriones*) traders and there is evidence for artisans producing luxury items, such as mirrors. On the whole, however, the evidence assembled in chapter 5 and the available data from Nijmegen do not create the impression of the great prosperity which is so evident for the 2nd and ear-

384 See note 358.
385 See chapter 8, note 186.
386 See chapter 8, notes 158 and 186; also above, note 336.
ly-3rd century northern Belgic civitates and also in the southeast of Germania Inferior, in the German Rhine-
land.\textsuperscript{388} In the early-3rd century, civitates such as those of the Ubii and Treveri became the scene of feverish
economic activity and there is ample archaeological evi-
dence of the great fortunes that must have been made.
As observed by many writers, the original input through
state expenditure and the very favourable geographical
location from a supra-regional perspective, must have
boosted the economy to unprecedented heights. While
there is evidence that for the empire as a whole taxation
in money became increasingly difficult into the 3rd cen-
tury and the volume of trade diminished,\textsuperscript{389} this is not true for the Rhineland which became an exporting re-
gion.\textsuperscript{390}

Developments in the Dutch river area could well be a
reflection of this social and economic florescence, al-
though a rather pale one when viewed in perspective. On
a supra-regional scale, the river area may have profited
most from the trade with Britain, which also seems to
have reached a peak in the early-3rd century.\textsuperscript{391} Perhaps
in this period the regional economic system became
more balanced, in the sense that its superstructure was
more directly related to the regional capacity to support
it. As suggested earlier, Ulpia Noviomagus seems to
have been a town that never quite made it as such. Also,
although there are no good reasons to assume that the
town itself was abandoned before about AD 260-270,\textsuperscript{392}
and may even have become a municipium only under Car-
calla,\textsuperscript{393} the finds from its main cemetery at Hees (site
398) show that the number of burials there may have de-

\textsuperscript{388} See, e.g., Von Petrikovits 1978, 81 f., 89 f., 167 f. or
Drinkwater 1983, 219 f.

\textsuperscript{389} See Hopkins 1980, 112-6.

\textsuperscript{390} Cf. Von Petrikovits 1978, 83, 143-4; Bechert 1982,
187 f. See also note 390 below.

\textsuperscript{391} See, e.g., Hassall 1978, 44.

\textsuperscript{392} Cf. Bogaers 1979b, 62.

\textsuperscript{393} Cf. Bogaers 1960-61, 309-10. The altar from Colijns-
plein, dedica by Hilarus, d(ecurio) M(unicp) B(atavorum), in AD 227 (Bogaers 1972b), provides the only precise ter-
minus ante quem.

\textsuperscript{394} See Brunsting, 1937, 198-202 for a summary. Brunsting
interpreted the relatively underrepresented 'Niederbieber'
pottery as evidence for a rather early end of the cemetery's use
(c. AD 240). When the settlement was abandoned only towards
AD 270 (cf. note 392) his alternative, a steadily decreasing use
during the 3rd century, is a much more likely interpretation. A
similar conclusion was already forwarded in 1937 by H. von
Petrikovits in his review of the publication (BJ 142, 363-5).

\textsuperscript{395} Until very recently it was even unknown if Noviomagus
was ever walled although, by analogy with the late-2nd or ear-
ly-3rd century defences around Forum Hadriani (e.g., Bogaers
1972a, 326), one could assume it was. See note 332 above.

\textsuperscript{396} Haalebos (1977, 290) assumes that the renewals were
probably not all contemporaneous and part of one large-scale
program. See also note 230. Relatively late activities can also be
deduced from tile-stamps (sites 298 (Wiichen) and 433 (Hol-
deurn); see fig. 50) and an inscription (site 422, Berg en Dal).
mentioning the title Severiana Alexandriana for the Ist and
3rd centuries. See also Bogaers 1972.

\textsuperscript{397} Birley 1981, 43; MacMullen 1984, 572.

\textsuperscript{398} Cf. note 227.

\textsuperscript{399} See esp. Von Petrikovits 1978, 90 and p. 407 above. Also,
instances of an increasingly less pri-

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\textsuperscript{397} Birley 1981, 43; MacMullen 1984, 572.
fact that at least some of them still served with their own national units stationed elsewhere has already been mentioned. Perhaps it was a special (2nd–3rd century) distinction. But many of them must have served not so far from home, as was the usual practice. In addition, there are the foreign soldiers and, as clearly indicated by epigraphical evidence for legionaries and auxiliaries alike, those who lived long enough to become veterans normally settled in the region where they had served. Moreover, some of the soldiers on active service had wives and children, a practice to some extent officially recognized under Hadrian and legalized by Severus in AD 197.

All this can only have led to an increasing amalgamation of population groups, a classical frontier ‘melting-pot’ situation, in which by the 3rd century there can only have been very few if any people left without more or less direct relations with the army. Thus, we may confidently assume that the original Batavian warrior tradition was smoothly carried on and transformed (as was the nature of their society and to some extent even the genetic composition of the population) into a 3rd-century tradition of military service with sons following in their fathers’ footsteps. The relative prosperity of this militarized frontier population was increased in various ways. For example, if they were not exempted from taxation because they were Batavians, many would have been because of their military status.

400 Cf. note 215. The numerous inscriptions assembled by Byvanck 1935, chapters 3 and 4, illustrate this for Nijmegen and the river area.

401 As pointed out by Hopkins (1980, 124), it is often forgotten that the number of veterans after 16 or 25 years of service is quite small, according to his figures only respectively c. 6% and 3% of the men originally enlisted. In view of the age at discharge given by the enlistment at c. 17 years of age (normally perhaps around 20 or even later, see Alfoldy 1968, 98–9), plus 25 years of service, and general life expectancy with the additional occupational hazards, this does not seem to be very exaggerated. Mann (1983, 59) estimates the legionary survivors at ‘rather less than half’.

402 Edict of Hadrian allowing legionaries’ sons to inherit (see Mann 1983, 65, and note 594). Septimius Severus’ measure recorded by Herodianus (III 8, 4–5).

403 Soldiers’ families are evidenced by the epigraphical record, both for auxiliaries (Alfoldy 1968, 201 f., no. 119 = CIL XIII, 1892, and 215, no. 162) and legionaries (Mann 1983, 95: CIL VI, 3335 and XIII, 8278).

404 On the exemption from taxation of soldiers and veterans, see Birley 1981, 45–6.


407 See Much 1967, 385 and Von Petrikovits 1978, 157. Vagdavercustis was a goddess of Batavian soldiers and is attested to from sites 69 (ER II, 280 = CIL XIII, 8805) and 450 (ER II, 1513 and 1514 = CIL XIII, 8702 and 8703), and the 3rd Batavian cohort in Pannonia (ER III, 1141A, see note 217). Also from Altkalkar (ER II, 988 = CIL XIII, 8662), Köln (ER II, 448 = CIL XIII, 12057), and Old Penrith (ER II, 1516 = RIB I, 926). The related Viradegdis a.o. from Vechten (ER II, 297 = CIL XIII, 8815).

408 Haeva is mentioned on the inscription from site 128, close to the fort at Arnhem-Meinerswijk (ER II, 275 = CIL XIII, 8705) and could be incorporated in the name of the frontier fort Leuvenum (Rijswijk-G.) which has been interpreted as Haevae Fanum (see Van Es 1984, 281, with further literature); Hurstrga on the altar from Kapel-Avezaath in the Nederbetuwe, south of the presumed forts at Rijswijk and Maurik (L’Année Epigraphique 1958, 37; see Bogaers 1960–61, 287–9).

409 See also paragraph 8.3.3 and notes 77–78.
particular interest. By innovative use of the evidence he showed that the low-profile decorative fibulae (see, e.g., fig. 71, 1–3) are characteristic for the population, associated directly with the army, along the frontier and obviously the result of a tendency to personal adornment of a prosperous and distinctive group of people as well as tokens of social status. Although this is more difficult to prove, the absurd footwear of the late 2nd–3rd centuries presumably points to the same thing.

It may be that a substantial part of the frontier population developed into a socially distinct, caste-like, group. In so far as their families did not already own much land, former soldiers surely acquired it. In the 3rd century veterans regularly received their praemia in the form of land allotments. This is perhaps another circumstance which may help explain the above-mentioned increasing density and acreage of the river forests at that time, because such people were not very likely to have engaged in ancient Batavian agricultural practices. Perhaps it is not inappropriate to say that the Dutch river area as a whole remained a kind of reserve for breeding soldiers throughout its entire history as part of the Roman empire. From the fierce barbarian warriors of Augustus' day to the self-perpetuating Romanized soldiers’ caste of the 3rd century which remained an important group even in the late-Roman army, literary and epigraphical evidence as well as the archaeological evidence suggest this. The Batavians can, in many respects, not be considered unique. A sedentary militarized population had developed along most of Rome's northern frontiers in the 3rd century. But even at a time when the socio-economic periphery of the 1st and 2nd centuries had developed into an area of central importance, the supra-regional spatial differentiation clearly shows that as far as Lower Germany is concerned most of the action took place in the German Rhineland. The main role of the Rhine-Meuse-Scheldt delta is more that of a reliable shield for Gallia Belgica and an important and carefully supported channel to and from the Rhineland. Also, the local aristocracy may have been more interested in pursuing a military career than engaging in administrative activities or agricultural production. For example, they allowed merchants to run their civitas, and the palynological evidence clearly shows that developments in the 2nd century did not lead to a massive agricultural exploitation of the countryside. The differences between the frontier population and that in more southerly areas are thus rather obvious. There, the tribute flows and dominance-dependency relations channelled through a native aristocracy owning villae and acting as patrons for the rural peasant society, brought about a true integration in the social and economic system of the empire. That there was also a large difference from the population north of the frontier is, of course, evident. There was obviously a great deal of contact, as evidenced by the archaeological record, through trade, diplomatic relations, and sometimes military action (or, in the opposite direction, raids!). Especially from the 3rd century onwards it was also stimulated through the German soldiers employed in the army. The more or less gradual change from a rather egalitarian tribal into a more stratified social organization is also amply documented by the archaeological and literary record. To name just one example, the development of a settlement like Wijster shows growth as well as the emergence of an elite group, manifest in the increasing importance of one household.

Such settlements were, in fact, very different from those inside the empire. They are true villages with a nucleated and closed structure and it may be argued that they represent a spatially bounded combination of elements which is found in a dispersed form south of the limes. Thus, the large and wealthy household might be comparable to a villa of the Rijswijk type with the sur-

411 Van Driel-Murray, in press. Especially notable are the bizarre sandals (op. cit., fig. 4) which have been found on some sites (villae, towns) in the hinterland as well (pers. comm. C. van Driel-Murray).
412 Gechter, op. cit., 8. The anthropological notion of caste may indeed fit most aspects of the situation rather well (Kloos 1972, 207): endogamous and normally localized group with inherited membership, associated with a specific profession, and part of a hierarchy of other similar groups.
413 Mann 1983, 67.
414 Cf. chapter 2, 22 and below, chapter 12.
415 Cf. above, note 336.
416 For further theoretical discussion along these lines, and the concept of integration, see Slofstra 1983.
417 An overview of the evidence for the directly relevant area, the northern Netherlands, is given by Van Es 1981, 265 f. For a broader overview, see Kunow 1983 (with extensive references to most of the relevant modern literature).
418 See, e.g., Steuer 1982.
419 Van Es 1967; Willems/Brandt 1977.
420 See also chapter 8, p. 283–4, and note 205.
rounded farms equivalent to the dispersed native farmsteads.\textsuperscript{421} What developed in spatial isolation, through direct input, in the protected environment of the empire can thus be seen as developing in spatial context, through indirectly stimulated internal developments, outside the \textit{limes} as well. This mitigates the observed disparities somewhat and illustrates the effects of the material and information flows across the difference of potential (see also above, note 17) created by the \textit{limes} construction.

Nevertheless, the imposed superstructure and acculturation process south of the Rhine did cause substantial distinctions, and the frontier itself may have been in many respects more of a barrier than is sometimes assumed. Why else should such a dense chain of forts have been built? An interesting conclusion in this respect is that proposed in paragraph 8.4.3, on the basis of the absence of mortaria even immediately north of the Rhine.\textsuperscript{422} The absence of a Romanized kitchen there is all the more remarkable when it is considered that settlements so close to the Rhine – few in number as they are – may quite probably have been situated on military land and their occupants formally considered subjects of the empire. In general, the number of ordinary Roman artefacts in normal settlement debris also drops off steeply north of the Rhine.\textsuperscript{423} That more precious objects, in different contexts, travelled much further is a fact so obvious that it need not be discussed in the present context. \textit{There is a lot to be said about the phenomenon as such,}\textsuperscript{424} but the items concerned were in no way a part of daily life except as prestige goods for the emerging Germanic elites.

The increasing organizational and information processing capacity of transrhenish societies was undoubtedly an important structural cause (as opposed to other, 'incidental' or 'historical' events) of the increased effectiveness of their traditional raiding operations. As outlined in paragraph 11.4.1, these did affect the river area from the late-2nd century onwards, but without causing any major damage. Two of the three Severan hoards (see note 427) were in fact found north of the Rhine (sites 22 and 26) and their significance is rather limited. It is quite probable that the early-3rd century saw some raids and, as a result, perhaps quite sizeable campaigns by the army. Such activities may, however, have had a positive rather than a negative impact on the river area in that they enhanced the prestige and conceivably the wealth of its militarized population. Less favourable for that population was, of course, the withdrawal of troops; but the auxiliary units from Germania Inferior do not seem to have been transferred elsewhere, which means that in our area there were probably no major changes.\textsuperscript{425}

To what extent the river area was affected by raids and in how far life may have become unpleasant there in the fourth and fifth decades of the 3rd century is impossible to estimate on the basis of the vague archaeological and historical evidence. In the second part of the century the then sharply declining non-tree pollen shows that cultivation of the land in general was now indeed reaching much lower levels than before. Shortly after AD 250, when a league-stone was erected near Rijswijk (cf. note 241), conditions must have deteriorated rapidly and when Gallienus was forced to move his headquarters to Köln in AD 257, the first group of 'Franks' \textit{may have been raiding the river area. But the general coin evidence indicates that habitation continued for more than a decade longer,}\textsuperscript{426} and most of the forts presumably held out to at least c. AD 260, if not c. AD 270.\textsuperscript{427} The settlement of Rijswijk must also have been inhabited until approximately the latter date.

\textsuperscript{421} The argument is derived from Van Es 1982, 152.

\textsuperscript{422} Above, p. 290; a similar observation has been made for the transrhenish area in the Province of North Holland (Brandt 1983, 138).

\textsuperscript{423} See, e.g., Van Es/Verlinde 1977 on the poverty of Roman finds in an area actually well within a 60 km radius from the frontier. The numerous finds from the \textit{terp} area on the Frisian coast may be an exception (van Es 1960, 1965–66).

\textsuperscript{424} See Kunow 1983.

\textsuperscript{425} See Alföldy 1968, 160–3.

\textsuperscript{426} Bloemers 1978a, 113 and notes 308–309.

\textsuperscript{427} Valkenburg may have come to its end in AD 240, but there is no compelling reason to reject c. AD 260 (cf. Bogaers, in Bogaers/Rüger 1974, 40). For Zwammerdam this can actually be demonstrated, as well as for Utrecht and the \textit{civitas} capitals at Voorburg and Nijmegen (Haalebos 1977, 65 f.), and also Vleuten-De Meern (Bogaers/Rüger 1974, 55) and Maurik (Haalebos 1976) have coin evidence for a longer occupation. As noted in the latter paper (p. 203), the Zwammerdam evidence shows that an earlier end of the coin series is not necessarily indicative of an earlier end of the occupation. This observation is strongly supported by numismatic analysis, which has shown the dramatic increase of very debased issues. As King (1981, 89) observed, only reasonably good silver coins \textit{may} have been saved. In this way, the 'Severan' hoards mentioned in paragraph 8.4.5 could in fact date to the 260s, which definitely makes more sense.
Similar arguments could be forwarded for a sizeable number of sites in the eastern river area (see chapter 4), but surface finds do not constitute hard evidence. But there are, as outlined earlier, good reasons to assume that under Postumus’ reign at least the area north of the Waal was settled by Franks and, because he was presumably a prominent member of the Batavian soldiers’ caste, this may have been effectuated without too much bloodshed. The fort at Qualburg was probably built in his reign (site 460), while many of the others may still have been held by his countrymen even without pay, simply because it was their only hope of survival. They may also have been actually supported by the Germans and Franks enlisted by Postumus. The Gallic emperor — who of course chose his seat in the metropolis at Köln — must have had other things to look after than the fate of Batavia. But he did succeed in creating a fairly effective military and administrative organization, and the provisioning of strongholds along the Lower Rhine could not have been impossible.

Apart from the coins, indications for Postumus’ activities are exceedingly scarce. In a recent article attention was called to the possibility that an official of his court was buried at Veldhoven in the Province of North Brabant (approximately on the southern limit of the Batavian civitas, see fig. 135). Two symbols of the Köln-centred Sabazius cult associated with such individuals were found here. The nature of the social or economic circumstances during the Gallic empire cannot, however, be deduced from this. We can take it for granted that the insecurity which must have sharply increased in only a few decades thoroughly shook up the relatively prosperous communities. And it undoubtedly diminished the wealth and prestige of the military caste which ceased to exist in the region, at least in its previous form, and in some instances quite probably physically. That most land was lying waste can, apart from the palynological evidence, also be deduced from the general circumstances; famine, spread of diseases among people packed in cramped quarters or simply suffering from undernourishment, and many other factors must have severely reduced the population both south and north of the Waal. Within a couple of decades, the civitas Batavorum as it had developed after two centuries of internal change and external patchwork must have partly ceased to exist — although it evidently did survive, as will be discussed in the next chapter.

1 Above, p. 409.
2 Eutropius IX, 11, 1: Ita Gallieno rem publicam deserente Romanum imperium in occidente per Postumum ... servatum est.
4 Nijmegen: Wynia 1979, 65; Wijk bij Duurstede: ER II, 1082 = CIL XIII, 12507. The latter tile may have originated from the probable frontier fort at Rijswijk-G. (Levefanum?); see Van Es 1984. Perhaps a more likely original findspot (see also fig. 143, 9 below) is Maurik, which is also very close to Dorestad.
tion do at least allow the conclusion that it is most probable that the limes as a whole was held until the end of the Gallic empire or shortly after.\(^5\)

But already before that time there were Franks south of the Lower Rhine and it is entirely conceivable that the second line, which perhaps always existed along the Waal,\(^6\) grew more important to control these people even if they were officially Postumus’ allies.\(^7\) Although a historical date is always problematic in relation to archaeological evidence, it is rather likely that the final abandonment of the military installations as far as they were still functioning should be connected to the large-scale invasions after the death of Aurelianus, in AD 275 and 276.\(^8\)

Although the invaders were apparently repulsed again from Gaul by Probus, this did not include the river area as can be deduced from the lack of pertinent references and also the fact that this is explicitly reported for Constantius Chlorus, who cleared Batavia as well as the coastal region from invaders.\(^9\) In reality, this enterprise may have involved more the pacification and at least formal restoration of Roman administration (i.e., of the entire civitas Batavorum) than the actual removal of Frankish settlers.\(^10\)

The clearing of Batavia took place in AD 293, on the eve of the campaign against the British usurper Carausius. Of the intervening two decades little is known which pertains directly to our area. Probably in AD 286, Maximianus is reported to have defeated two Germanic tribes, one of them the Heruli.\(^11\) In view of their later association with Batavi in the auxiliary units mentioned in the Notitia Dignitatum, there is every reason to assume

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5 See Haalebos 1976, esp. 208; see also fig. 141 and, for the interpretation of the coins, King 1981. See also Van Es 1981, 121.
6 See above, p. 411.
7 An example (in addition to Nijmegen?) could be Rossum (Grinnes?), another perhaps nearby Kessel (see NKNOB 1976, 189 or JROB 1977, 47–8).
9 For an overview of the relevant sources, see De Boone 1954, 47–9 and 57–9.
10 In fact, this is also reported, e.g., Panegyrici Latini VI (VII), Pan. Constantino Augusta 5,3. On the continued existence of the civitas, see also chapter 8, p. 312 and notes 298–300.
11 Panegyrici Latini X (II), Pan. Maximiano Augusto 7 and XI (III), Pan. genethliacus Maximiano Augusto 5.
that this tribe settled in the river area. As noted by Hoffmann, the original unit was not formed much later than AD 290 (and definitely under Diocletian) and may indeed have been raised among the Batavi and Heruli in the river area very shortly after Constantius pacified the region in AD 293. At least from this time on, we must assume that the former civitas Batavorum was again part of the reorganized empire under the tetrarchy.

Organizational change
The various reorganizations need not be extensively discussed here. Especially important in the present context is the fact that the province of Germania Inferior was transformed into Germania Secunda, itself part of the dioecesis Galliarum, and probably encompassed four administrative units, the civitates Agrippinensium, Traianensium, Batavorum, and, as an addition, Tongrorum. Under Diocletianus, the process of changes in taxation was also virtually completed. The taxation in money, which had been an important factor in economic development in earlier centuries and was increasingly replaced by rather irregular requisitions during the 3rd century, was reorganized into a system based largely on taxation in kind. When Germania II was indeed an enlarged successor of the former province, this may well be related to the taxation policy because there is a strong tendency all through the observable reorganizations to create interdependent but as much as possible self-sufficient units at all levels. Thus, the addition of the civitas Tongrorum in the rear can be seen as necessary from an economic point of view.

The same can be said when the changed strategy of imperial security is taken into consideration. From Gallienus onwards the continuous need to repulse major invasions had already led to the formation of mobile regional field reserves. Under the tetrarchy these were maintained only as the relatively small field armies (comitatus) of Diocletian and his colleagues, while the increased size of the army went into the frontier troops still composed of legions with auxiliaries and also cavalry vexillations. The main reform concerned the defence of the frontiers and the hinterland. In general, this was not the restoration of the former limbus connected with a strategy of forward defence but the creation of a deeper zone of defence more resilient to major attacks. This included the building of heavily fortified strongholds at easily defensible and optimally situated locations for communication and traffic, road forts to block easy access into the hinterland, fortified settlements and granaries, and the like. Settlements thus had to rely on their own defences in time of need, at least for a while, which is another indication for the trend towards self-sufficiency.

The defence-in-depth system was, from an organizational point of view, brought to its logical conclusion under Constantine with the creation of a central field army of comitatenses by removing units from Diocletian’s static border troops, the limitanei (then called ripenses). This must have occurred before AD 325 and probably goes back to Constantine’s campaign against Maxentius in AD 312. Although there were obviously different developments in various parts of the empire in response to local circumstances, the decades around AD 300 thus saw the effective transformation of a forward defence into a defence-in-depth strategy which, at the level of

12 From their later role in various sources (for references, see De Boone 1954, 52) it can be concluded that they were not truly exterminated. They are also mentioned twice between the Saxons and Franks. As noted by Hoffmann (1969, 157), the unit was not formed from the laeti living in northwestern France in the 4th century.
13 Op. cit., 156–7. The rejection (note 258) of Heruli on the Lower Rhine is unjustified. It does not follow from the dismissal of Schmidt’s arguments cited there. Hoffmann does not take De Boone’s argument into account (cf. note 12) nor the sources mentioned there.
14 For a concise overview, see Von Petrikovits 1978, 213–22.
15 As indicated in the Verona list (Nomina provinciarum omnium 8).
16 Cf. Von Petrikovits 1978, 217. On the debate about the size of Germania Inferior and the civitas Tongrorum as a later addition, see Bogaers 1972a, 326 f. As noted in chapter 11 (p. 410) the civitas of the Cananefates no longer existed as a result of the late-3rd-century warfare, but presumably also due to inundations caused by the Duinkerke marine transgression phase (cf. Bloemers 1978a, 113). Perhaps there is literary evidence for this as well, in a passage where the area around the Scheldt and between the branches of the Rhine is described as ‘paene ... terra non est’, as ‘hardly any land’ (Panegyrici Latini VIII (V), Pan. Constantio Caesari 8, held in AD 297).
17 See e.g. Hopkins 1980.
18 See also Luttwak 1976, ch. 3.
19 The debate on this army reform now seems to have been settled in favour of Van Berchem’s conclusions (cf. Luttwak 1976, 187–8 or Jones 1973, 97 f., 608 f.
20 Jones 1973, 97.
21 Which is the central theme in Mann’s (1979) criticisms on Luttwak 1976.
the empire, was a reliable policy to ensure security. For border regions, with increased exposure to barbarian raids, this was not so beneficial and the obvious desire to attain as much protection as possible is undoubtedly the major reason for a return to more preclusive defence systems wherever and whenever possible.22

Another element in the overall strategy was, of course, diplomacy based on power whenever possible and, depending on the situation, alternated with the use of force such as in pre-emptive strikes across the frontier or backed by sometimes large payments to local leaders. Apart from the wealth from booty and to some degree trade, this should also have strengthened the position of Germanic leaders and the developing hierarchical structure of Germanic society. Even more important may have been the practice to allow Germans into the empire, to settle depopulated lands as laeti or to enlist them in the army,23 and not from weakness but as deliberate policy. The sources contain many references comparable to the measures taken by Constantius Chlorus mentioned above, indicating that barbarian invaders settling in conquered lands were first defeated and then allowed to stay. In addition, there were groups allowed inside the empire under a treaty and there was the renewal of a client system with groups outside.

Population groups and size
To return to the river area, the historical evidence indicates that from shortly before the beginning of the 4th century, the region must have been under fairly effective control again. At least part of the territory was settled by immigrants, perhaps by Franks who had been there since Postumus’ reign and new groups such as the Heruli. What happened to the remainder of the former (Batavian) population is uncertain. It is often assumed that they settled in Gaul as laeti, in the regions where they are recorded by the Notitia Dignitatum.24 But that is in fact rather unlikely, and there are two good reasons for rejecting this conclusion. First, laeti were normally barbarians established in a large group inside the empire, preferably with a fort nearby, and under military control, supervised by a praepositus or praefectus.25 This is also reported for the laeti Batavi,26 but in that case it is rather astonishing. Why would skilled soldiers and Roman citizens be treated in a fashion normally reserved for barbarians, and then quite unnecessarily? Their prominent position is not only evident from what has been said earlier about 3rd-century developments. It is also illustrated for the period under the tetrarchy when once again a Batavian rose to a high position, namely Aurelius Ianuarius, vir perfectissimus who was dux of the province Pannonia Superior in AD 303.27 In addition, we have the evidence from the auxiliary double unit of Batavi and Heruli of whom the latter are nowhere attested as laeti and which, as correctly noted by Hoffmann,28 is therefore very unlikely to have been raised from such groups.

The obvious solution to this problem is to accept that the auxiliaries were raised among the mixed population of the river area. That leaves the laeti Batavi unexplained, but there are in fact two possibilities in this respect. First, the Notitia Dignitatum was presumably composed around AD 400 and revised for the last time in AD 423, and the settling in Gaul may have been much later than c. AD 300. Second, when a relatively early date of settling is assumed, it is entirely conceivable that the settlers in Gaul were the earlier immigrants in Batavia, for example, the Franks who had settled there under Postumus.

Although far less precise than in the 1st century, the troops raised do also give an impression of population size when it is accepted that they were originally raised in the river area. The Notitia Dignitatum lists five Batavian units, two auxilia palatina and three vexillationes palatinae (cavalry units). For the original enlistment the junior units should be deleted (Batavi iuniores) and one of the vexillationes is mentioned twice.29 To the remaining two original units, one infantry and – as was to be expected from Batavians – one cavalry, should be added the auxilia Herulorum. The auxilia may have num

reach such high positions from Constantine onwards. Aurelius Ianuarius, however, was not a barbarian but a Roman. His position is rather similar to that held by Postumus before his usurpation.28

23 For an overview, see MacMullen 1963a.
24 E.g., Jones 1973, 620; locations in Not. Dig. Occ. XLII, see ch. 11, note 254.
26 And for two other Roman (!) groups: the Nervii and Lingones.
27 ER II, 1329 = CIL III, 10981. See Waas 1965, 103. As shown by that study (p. 7 f.), barbarians were only able to
bered between 600 and 800 men, a vexillatio about 500,\textsuperscript{31} which would bring the total number of men drafted to approximately 1900. This amounts to roughly one-third of the troops enlisted in the mid-1st century and, if the relation to population size were more or less constant, indicates a total population of between 10,000 and 14,000 people,\textsuperscript{32} with 6000–9000 of them descendants of the early-3rd-century population.

The reasons why the above calculations could be wrong are so evident that there is no point in discussing them. It can only be remarked here that if the original population still occupied most of the area south of the Waal with the immigrants to the north and west, as was also deduced by De Boone,\textsuperscript{33} then the relative sizes of these population groups are consistent with those of their territories. The figures of population size, taking those of the 1st century as a starting-point and taking into account the population growth of 25\% in the 2nd,\textsuperscript{34} imply a population decline between c. AD 200 and 300 to a minimum of 12\% and a maximum of 20\% when the immigrants are not counted, 20–28\% when they are. The latter figures compare very well indeed with the reduction in the number of settlements to about 25\% of the former level calculated in chapter 8 for the eastern river area.\textsuperscript{35} Although this cannot be used as proof because there are too many assumptions involved in both calculations, the basic independency of both data sets (site counts and number of soldiers drafted) shows that at least the order of magnitude of the observed changes may be correctly estimated. This despite the fact that the 25\% remaining sites is actually a rather vague figure because it is probably an underestimate for the eastern river area but, on the other hand, it refers to the entire 4th century.

Further developments

The continued importance of the river area, even if only from a strategical point of view, is quite evident from the fact that it was apparently necessary to pacify and control the region before the final blow could be dealt to the British usurper Allectus in AD 296.\textsuperscript{36} At a supra-regional level, the continued importance of the whole north-western frontier zone as a new centre of socio-economic development instead of a periphery is apparent from, as well as further enhanced by, the fact that the new caesar Constantius Chlorus chose Trier as his seat. After he became augustus of the west and shortly thereafter was succeeded by his son Constantinus I, there was apparently increased raiding and several invasions, but these were dealt with and answered by vigorous retaliation with search-and-destroy parties across the frontier. That the river area was probably affected by these events can be deduced from the fact that the Chamavi, who lived directly north of the area,\textsuperscript{37} were also involved.\textsuperscript{38} The remainder of Constantine's reign seems to have been relatively peaceful and, as elsewhere, the north-western border was secured by new fortifications, at the frontier as well as in the hinterland.\textsuperscript{39} Presumably caused by the trouble after his death, Frankish invasions are again reported shortly after AD 340. These were stopped, but only by some treaty with Constans.\textsuperscript{40} This may have included permission to settle, but it is unknown where or which Franks were involved. A quite uncertain,\textsuperscript{41} but nevertheless a possible interpretation is that Salii were involved. These are reported to have moved into Batavia before the days of Julianus,\textsuperscript{42} and had (later?) illegally settled also in Toxandria where they were properly subdued by the young caesar in AD 358.\textsuperscript{43}

The uncertainty of this historical reconstruction is, of course, due to the catastrophic events following the usurpation of Magnentius in AD 350. The resulting civil war depleted the frontier of troops, which the Franks and Alamanni were not slow to take advantage of. Presumably from AD 352 onwards,\textsuperscript{44} Franks poured into Gaul and conquered many towns including even, in AD 355, the occupation of Köln.

\begin{itemize}
\item 31 Hoffmann, op. cit., 4; Jones, op. cit., 681–2.
\item 32 Cf. chapter 11, p. 390 and 395–7.
\item 33 Op. cit., 52.
\item 34 Cf. chapter 11, p. 416.
\item 35 Above, p. 416, below, p. 450–1.
\item 36 That this importance of the river area was realized on the other side of the channel as well could be indicated by the antoniniani of Carausius with Hercules Deuseniensis on the reverse (RIC V (II), 800). See chapter 11, note 256.
\item 38 Panegyrici Latini IV (X), Pan. Constantino Augusto 18.
\item 39 Cf. below, p. 445; see Von Petrikovits 1971, 182–4.
\item 40 As can be deduced from Libanius, Oratio LIX, 130–1; for other relevant sources see De Boone 1954, 80–2.
\item 42 Zosimus, Historia Nova III, 6; De Boone 1954, 82, 91.
\item 43 Ammianus Marcellinus XVII 8.
\item 44 See De Boone 1954, 84 f. or Von Petrikovits 1978, 187 f. for detailed accounts.
\end{itemize}
These events must have thoroughly disrupted the nature of the river area that had existed since Constantius Chlorus pacified the region. Julianus, 'the Apostate', recaptured Köln in AD 356. After the battle of Straßburg-Argentoratum against the Alamanni in AD 357, participated in by the Batavi and Heruli (their unit then still in its original and undivided form), the caesar apparently needed to campaign in Germany II, the need being demonstrated by his encounter with a Frankish raiding party at the Meuse, far to the south between Jülich and Reims. This was followed by the above-mentioned expedition against the group of Sali in Toxandria, who were probably allowed to stay but on Roman conditions: again an example of an often repeated practice. In the same passage, however, Ammianus also reports that the Chamavi, who had invaded the river area, were partly defeated as well and that a treaty was made obliging them to return across the Rhine. This difference in treatment has, quite plausibly, been explained by De Boone as the result of another relationship with the Roman empire which could date back to a treaty between the Sali and Constans shortly after AD 340. But this need not be true, because the necessity to achieve security in the river area must have been a powerful motive in itself. After all, neither the civitas Batavorum nor even that of the Traianenses can be assumed to have still existed as such. The Chamavi presumably continued to make trouble and Julianus also had logistic problems but in the end they did return home across the Rhine and the free passage along the river was secured. In AD 359 the caesar himself accompanied a fleet of 600 corn ships from Britain up the Rhine and a whole program of structural measures was carried out. These involved the restoration of three fortifications along the Meuse and seven along the Rhine, in addition to a number of granaries. The troops received assistance in this from the subdued barbarians who had to supply building material.

Aftermath

It may be concluded that, by AD 360, the entire river area up to the Rhine was again under Roman control, and to an extent which was not even accomplished by Constantius Chlorus. But what remained there as a population is uncertain. The most likely interpretation is that the former inhabitants were largely if not totally replaced by Sali, now living as foederati not only in Batavia proper but also in the southern part of the river area (south of the Waal) as well as south of the Meuse (into Toxandria), and thus effectively the entire area of the former civitas Batavorum. That was a rather comfortable position compared to that of the people beyond the Rhine who, from the late 350s onwards, had to suffer under a vigorous and remorseless Roman policy. The exploits of the bloodthirsty expeditionary force sent into Chattiarian lands across the Rhine from Xanten in AD 360 are but one example. There are many more, all distinctly horrible, and on top of that came requisitions of material and food as mentioned above, provisions which could no longer be obtained in sufficient quantities in Gaul, at least not by normal methods.

45 Ammianus Marcellinus XVII 2. The obvious location for this is the Constantinian fortification of Maastricht (see Panhuysen 1984, 52–65), but there is as yet no (published) evidence which allows the conclusion that the site was interrupted in the mid-4th century as is implied by Ammianus' words.

46 On the Sali, see De Boone 1954, 91 and Böhme 1974, 197; also Hoffmann 1969, 158–9: Sali were enlisted at that time under their own name, the Chamavi not (but see note 57 below). On the end of the two northern civitates of Germany II, see above, chapter 8, notes 298 and 300.

47 A detailed discussion of the various references by Julianus, Ammianus Marcellinus, Libanius, Eunapius, and Zosimus in De Boone, op. cit., 89–95.

48 Julianus, Epistula ad Atheniensis, p. 280 A; Ammianus Marcellinus XVII 9, 1 and XVIII 2, 3–6.

49 For archaeological evidence see chapter 9 and below, 451–7.
In AD 360 Constantius II demanded troops, among whom the Batavi, for his eastern army. The unit at that time probably still consisted for a considerable part of Batavians, but there is no way of verifying this. By then there were no, or hardly any, Batavians left to recruit from and the later Batavian units, split into seniores and iuniores, had little or nothing to do with the river area. The troops recruited here - and there is conclusive evidence for that - were Salii, together with other population groups and Chamavian prisoners of war. In AD 358–9, these constituted the double unit of the Tubantes and Salii, which was formed at that time.

The active policy of brutal action across the border was continued by Valentinianus I, in particular against the troublesome Alamanni, and was complemented by diplomatic action, bribing, or the assassination of tribal leaders, all spelled out in detail by Ammianus. In addition, the fortification program was continuously developed further or reinforced, a hardly surprising measure in view of the resentment created beyond the borders. Apparently the seaborne Saxon raids on Britain were accompanied by trouble in the river area, but what happened is unknown. An encompassing building program to increase the strength of the defence-in-depth system in the Rhine and Danube sectors was initiated in AD 369. In general, trouble with the Franks in this period was rather minimal compared to developments elsewhere, and for our area the defensive system did work out as intended. The Saxon raid which was halted in AD 370 at Deuson, in the region south of the Meuse occupied by the Salii, is a good illustration. The invaders were stopped by local troops and then they were defeated - through particularly nasty tactics indicative of the spirit of those days by troops brought up from the hinterland. A similar event, though more dangerous and damaging due to the effects of the usurpation of Magnus Maximus, occurred in AD 388. But again, despite the destruction in Germania II, the system worked, and a few years later the Frank Arbogastes, the powerful supreme commander of the western army, followed the usual policy by invading Frankish land (across the Rhine from Köln).

The episode does show, however, that the Franks were able to unite under their kings to form ever larger armies: a phenomenon reflecting also the process of social change among such groups as the Chamavi, Bructeri, Amsivarii, and the like. This process was undoubtedly stimulated by the harsh Roman policy towards its neighbours in the second part of the 4th century, which can only have been highly disruptive for Germanic society. The same development can be observed in the growth of a stratified society and the formation of large socio-political units such as the Alamanni, Quadi, Burgundi, or Goths. Conversely, Roman society in the northwest be-

55 Hoffmann 1969, 81 and ch. 6, note 258 argues against the national character of the unit, but his arguments are based on evidence from the Concordia cemetery of Venezia, datable to AD 394/5 (op. cit., 101). Not surprising, the 7(!) buried soldiers of the numerus Batavorum do not show a Batavian origin, although this cannot be excluded either in some cases.


57 Notitia Dignitatum, Occ. 5. See Hoffmann 1969, 158–9. His explanation for the sudden reappearance of the Tubantes (or Tuihanti) in the sources is, however, very unsatisfactory. In early sources they are mentioned in connection with Tenceri and Usipetes (e.g., Much 1967, 393–4). The name survives in present-day Twente, a region next to present-day Salland which is connected with the Salii, both areas constituting the modern Province of Overijssel. The location of the tribe, which is also attested epigraphically (cives Tuihanti, belonging to the cuneus Frisiorum, erected an altar at Housesteads: RIB I, 1594), is discussed by Bloemers 1983, 185. See also fig. 139. The Tubantes thus probably moved south with the Salii. A second possibility is that they are Zosimus’ (III 6, 1–3) somehow mixed up ‘Kouadoi’, which would be more understand-

58 Panegyrici Latini II (XIII), Pan. Theodosio Augusto 5, with a brief reference to ‘what the Rhine or Waal have seen’ (qua Rheus aut Vachalis vidit), confirmed by Ammianus Marcellinus XXVII 8,5 and an inscription from AD 369 in which Valentinian is called Francicus Maximus (CIL VI, 1175 = ER II, 468).

59 Ammianus Marcellinus XXVIII 2, 1. For details, see Von Petrikovits 1971, 184 f.

60 Cf. chapter 11, note 256.

61 After a peace treaty the returning invaders were ambushed and slaughtered to the last man. Ammianus Marcellinus (XXVIII 5, 1–8) finds it necessary to defend the act, being aware that the commanding general, magister peditum Severus, was surely a war-criminal by any standard.


came more Germanized, not only by new settlers but also by the very considerable and steadily increasing role of Germans in the army.\textsuperscript{64} In fact, the socio-cultural 'potential difference' (see chapter 11, note 17) across the frontier decreased rapidly during and especially towards the end of the 4th century, to be replaced by more purely political and military differences.

The situation is rather well exemplified by Arbogastes, who was \textit{de facto} ruling the west from AD 388 until he was defeated by Theodosius in AD 394.\textsuperscript{65} His role was taken over by Stilicho, under the infant-emperor Honorius who had succeeded his father Theodosius in the west in AD 395. Apparently Stilicho immediately inspected the Rhine sector and concluded a number of treaties. From the various sources it can be inferred that a relatively stable situation was maintained. Relevant for our area is that the Salii were apparently able to cultivate their lands in peace.\textsuperscript{66} Presumably as \textit{foederati} they were almost certainly tied into the economic system of Germania II and produced some surplus to meet their obligations, but nothing is known about the area and inferences from the scarce information about more southern regions are very risky indeed. It may be that the relations with Roman society were largely limited to providing troops and to supporting units stationed in the river area in their function of securing river transport, as well as by delivering food to those troops. Transport and trade going through the area are likely to have stimulated the economy to some degree, perhaps also trade with transrhenish groups such as the thoroughly subdued Chamavi (cf. note 47).

When Stilicho withdrew both the field army and also border troops for his – successful – campaign against the Visigoth invaders of Italy under Alaric,\textsuperscript{67} everything remained as it was, which testifies to the relatively stable situation. As it turned out, when the Vandals crossed the Rhine at Mainz in AD 406 the resulting invasions and destruction ushered in the end of central Roman authority. But in our area, or for that matter in all of Germania II, everything still remained as it was.\textsuperscript{68} None of its towns were sacked, as were so many in Germania I and Belgica and, in fact, even Rome itself. Apparently the Franks, who had been the unsuccessful defenders at Mainz,\textsuperscript{69} at peace with their transrhenish counterparts, had become enough of a force themselves to prevent their territory from being invaded and laid waste.

\subsection*{12.1.2 The Frankish Kingdom}

The usurper Constantinus III may initially have been helpful in restoring some degree of order, but the same source also clearly indicates that this was not effective and that the population in the northwest rapidly formed independent socio-political units, defending themselves because nobody else could.\textsuperscript{70} The fact that Britain went its own way from Constantinus III onwards also meant that the Dutch river area lost much of its strategic value from a supra-regional point of view. In how far the region was affected by events during the remaining years of Honorius' reign is impossible to establish. In any case in the 420s Franks invaded and settled across the Rhine which can only have been in the \textit{civitates Traianensium} and \textit{Agrippinensium},\textsuperscript{71} where they were subdued again by Aetius in AD 428 or 432, but in all probability they remained there as \textit{foederati}.\textsuperscript{72}

The further developments in Gaul are not directly relevant here.\textsuperscript{73} In the present context it is especially important that the Salii also started to move southwards. In about AD 447,\textsuperscript{74} Aetius gained a victory over them, presumably in northern France, where they had moved under their king Chlogio (or Cloio). Only four years later the general had to call upon (a.o.) Franks, 'once Roman soldiers',\textsuperscript{75} to help defeat the Huns who had invaded Gaul. The description of the Salian warriors reminds us in some ways (tall, ginger-haired) of that of the Batavians, although their war tactics were different.\textsuperscript{76} Apparently Chlogio succeeded in establishing a kingdom reaching as far as the river Somme in those days.\textsuperscript{77} It is possible, though wholly uncertain, that the king was

\begin{thebibliography}{99}
\bibitem{65} Orosius, \textit{Hist.} VII 35, 12.
\bibitem{66} Claudianus XXI, \textit{De consulatu Stilichonis} I, 222. For an overview of the sources, see De Boone 1954, chapter 7.
\bibitem{67} Claudianus XXVI, \textit{De bello Pollentino sive Gothico}, 421–2.
\bibitem{69} Orosius, \textit{Hist.} VII 40, 3.
\bibitem{70} Zosimus, \textit{Hist. Nova} VI 3, 5, and 10.
\bibitem{71} Prosper, \textit{Epitoma chronicon}, 428; Iordanes, \textit{Getica} 34, 176.
\bibitem{72} Merobaudes, \textit{Pan. in Aétium} II, 5.
\bibitem{73} For a short overview, see Von Petrikovits 1978, 278 f.
\bibitem{74} Byvanck 1947, 406; the interpretation was confirmed by De Boone 1954, 141 and also by more recent research (cf. Von Petrikovits 1978, 279). The episode is recorded in Sidonius Apollinaris, \textit{Carmen} V, 206–18.
\bibitem{75} Iordanes, \textit{Getica} 36, 191.
\bibitem{76} Sidonius Apollinaris, \textit{Carmen} V, 238 f.
\bibitem{77} Cf. De Boone 1954, 142.
\end{thebibliography}
descended from a royal family to which belonged an executed king called Theudemer, son of a certain Richimer, who may have been the otherwise well-known *comes* Richomeres, who was definitely of royal blood.\(^78\)

Whatever the actual situation may have been, it is evident that the social structure of the Salii had developed into a hierarchical organization encompassing a quite sizeable group of people. Chlogio’s endeavours can be interpreted as an attempt, and a successful one, to consolidate his political power (and thus implicitly the structure of society) by expansion, in order to create a larger and more viable polity. But the roots of this development must be sought in the former *civitas Batavorum* where the Salii, as allies of the empire and in close contact with Gallo-Roman society in which their own elite undoubtedly participated (even though this cannot be conclusively demonstrated historically), further evolved from rank into stratified society.\(^79\) Around the mid-5th century the changed circumstances then led to the next step, that of state formation.

Of course the Salii were not alone in this. In AD 454, after the murder of Aetius, there were renewed invasions from Frankish groups east of the Rhine into Germania I and Belgica.\(^80\) A few years later they conquered the town of Köln,\(^81\) which became the seat of a Frankish king. This meant the genesis of another polity, that which the Anonymus Ravennas two centuries later calls *Franzia Rimensis*.\(^82\) Which of these two kingdoms was in control in the river area is unknown. Nor is it very relevant, because in the second part of the 5th century the kings of both polities, Childeric in Tournay and Sigibert in Köln, were not yet the heads of true states but the dominant rulers among a number of lesser kings, *reges critici*, who had some degree of independence.\(^83\) Although the truth of this cannot be established, Gregorius of Tours indicates that among the Salii these kings were all descendants of one lineage, the Merovingians.\(^84\)

The river area population, whichever group of Franks this may have been,\(^85\) was in any case a peripheral group again with respect to the people in northwestern France and Belgium or the German Rhineland. Thus, under very different social and political circumstances than before, the late-Iron Age situation, with the delta region as a periphery of a Belgic core area, had returned after more than five centuries.

**Epilogue**

From an analytical point of view, the turn of the 5th into the 6th century is a convenient end for a discussion centred on developments in the eastern river area in the Roman Period. Contacts with the south were to some degree maintained, but the region around the Nijmegen-Noita of the Anonymus Ravennas (see fig. 140) was at best a rather insignificant and rather depopulated outer zone of the Frankish state which was founded by Childeric’s son Clovis (Chlodwic, Chlodowech) through the unification of the various polities along the Rhine and in Gaul.\(^86\) On the other hand, it was not less than that: the eastern river area may be regarded as the northernmost region with ties to the new state. This is shown by the events recorded in connection with a raid of the Geti (Jutes?) in the Dutch delta area in about AD 523, mentioned by Gregorius of Tours\(^87\) and in the Beowulf epic. Under their king Hygelac (Chochilaichus) they traversed Frisian country into the *pagus* of the Chattuarii,\(^88\) which must have been Frankish territory and situated in Chattuari from the southeast and Chamavi from the north (see below, note 87). Presumably, however, they still controlled the delta to some extent. In any case Childeric defeated the Saxons on the coast (*Hist. Francorum* II, 19), between the Scheldt and Meuse/Waal. He was himself defeated there by the Goths under Euric, in which context mention is made of ‘trembling barbarians on the Waal’ (*Sidonius Apollinaris, Epistulae* VII 3, 3).

\(^{78}\) This is suggested by Gregorius of Tours (II, 9), but quite evidently he did not know (cf. De Boone 1954, 127 and note 912) if or which family ties existed. On Richomeres, who was the uncle of the exiled Arbogastes mentioned above, see Waas 1965, 119–22.

\(^{79}\) See Fried 1967 for these concepts.

\(^{80}\) *Sidonius Apollinaris, Carmen* VII, 372–3.

\(^{81}\) *Liber Historiae Francorum* 8.


\(^{83}\) E.g., Ewig 1980, 13; De Boone 1954, 144 f.


\(^{85}\) *Meroveus* was related to Chlogio and father of Childeric.

\(^{86}\) See, e.g., *Zöllner* 1970, 44 f.

\(^{87}\) *Hist. Francorum* III, 3; also in *Liber Historiae Francorum* 19.

\(^{88}\) Hattuarii, Attoarios, in *Beowulf* Hethware. For further comment, see also *Zöllner* 1970, 82, Blok 1979, 24–5, Halbertsma 1984, 325–6.
When returning with booty the invaders were attacked by a strong Frankish army under Chlodowech's grandson Theudebert, if the Beowulf is to be believed in coordination with an attack by a Frescyninge, a Frisian king. The event shows at any rate that a nucleus of Frankish influence always remained in the eastern part of the delta, the west being controlled by the Frisii, and that this was important enough to react immediately to a threat such as posed by the Scandinavian raiders. Perhaps a parallel to the late-Roman defence-in-depth system is not entirely out of place in this context, when the Nijmegen region is seen as a first outpost to counter attacks from overseas or, for that matter, from the Frisians. But conditions were, of course, quite different in the 6th century and the Frankish state was a very viable and expanding structure.

The internal developments and those along the northern periphery, which inevitably led to the integration of the whole Dutch delta into the Merovingian/Carolingian empire, are not, however, part of the present study. They represent the start of a new cycle of development in which the river area, from the 6th century into the 7th and at an increased pace after the defeat of the Frisians in AD 720, profited from the then again favourable geographical situation which stimulated economic development and the emergence of new centres.

12.2 BATAVI AND SALII. CHANGES IN THE RIVER AREA

From the previous paragraph it is evident that the various subjects related to the habitation of the eastern river area should be considered for three separate developmental stages. The first stage is the period between the Gallic empire and the invasions of the mid-4th century, which terminated what was left of the civitas Batavorum. The second covers the phase in which the area was occupied by Salii, as foederati living in relative peace and prosperity in a frontier zone which, though not protected, was effectively controlled up to the northern banks of the Rhine by the central authorities. It is difficult to decide when the third stage began. If the end of effective control from the Roman state is taken as a criterion, AD 406/7 is as good as any other date. The end of Honorius' reign (AD 423), with usurpations and other confusing events in the northwest during which nobody could know what the outcome would be, is a good but not a better alternative. At any rate not much changed parallel to the late-Roman defence-in-depth system is not entirely out of place in this context, when the Nijmegen region is seen as a first outpost to counter attacks from overseas or, for that matter, from the Frisians. But conditions were, of course, quite different in the 6th century and the Frankish state was a very viable and expanding structure.

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initially and the Salii undoubtedly guarded their territory as closely, or even better, as when they were still under Roman supervision. They must gradually (?) have started acting on their own behalf. This led to the expansion southwards and perhaps eventually to the settling of other Franks (Chamavi, Chattuarii) in the region. Archaeological evidence for such a three-stage development is not lacking, as will appear below. But archaeological evidence for historical reconstructions, welcome as it is, is not the major objective in the present context. It is very unfortunate, therefore, that the level of chronological control is rather restricted where the majority of sites is concerned. There is thus very little opportunity for detailed inferences about most sites or about regional habitation patterns. On the other hand, the information about the region as a whole, incorporating all datable material from all types of sites and not only settlements, does allow more general conclusions about the structure and development of the area. But it is quite obvious from the limitations of the presently available information that one of the main priorities of the ERA-project in the near future must be the investigation of late-Roman/early-Merovingian rural settlements in the river area proper, between Rhine and Meuse.

12.2.1 Ups and Downs until the End of the Batavian Civitas

As argued at the beginning of this chapter, the limes may finally have collapsed only in about AD 275. But whether this is true or not, and whether it was a single event or a more gradual abandonment of the frontier forts, is important but not crucial. The evidence as such shows that the Rhine frontier was abandoned at or shortly before a major, historically attested barbarian invasion and the evidence from rural settlements, in the river area as elsewhere, confirms this picture. In the absence of detailed chronological evidence, little can be said about developments in the region in the following decades. One thing, however, is very clear: ties with the Roman state were virtually non-existent in this period when measured by the only reliable source of information, the coin import. It is surely no coincidence that coins from the last quarter of the 3rd century are virtually lacking from the river area as well as from the rest of the southern Netherlands, the present-day provinces of Zeeland and North Brabant. In Zeeland curves break off abruptly in AD 273, in North Brabant, as well as in the river area, a few years later. Later coinage until the end of Diocletian’s reign is not entirely absent, but it is very scarce indeed. In principle, this can be taken as an indication for a total disorder and a lack of administrative control continuing throughout the period of the tetrarchy, its few coins being attributed to import during the following period. But there is a good argument against such an interpretation, namely, the spatial distribution of these coins. Currency from between AD 276–306 is available for Nijmegen (site 403), Cuijk (site 499), and Heumensoord (site 391), in a context that allows the conclusion that they were occupied. For sites such as Rossum and Qualburg (site 460) this is less certain, for Asperden (site 466) it is unlikely. But there are additional finds, notably along and south of the Meuse below Cuijk, from such sites as Grave, Lith, and Alphen. Although there are, admittedly, a few finds from the area north of the Waal, the evidence thus indicates that some degree of control was maintained (or restored after AD 293) in the area south of the Waal. Perhaps a better interpretation is that this applied to the area around a bridgehead constituted by Nijmegen and environs, because the relevant finds are all concentrated around Nijmegen, up to Rossum in the west, to Qualburg in the east, and to the south of that line.

This interpretation is supported by other evidence, namely, the survival of the secondary centres (pagus capitals?) mentioned in chapter 11. Of the five centres only three survived into the Late-Roman Period: Cuijk, Wijchen, and Rossum. The centre north of the Waal, Elst,

93 See chapter 1.
94 Cf. chapters 4, 77, 92; 6, 158, 164, 167, 180; 8, 303; 9, note 32.
95 Boersma 1967.
96 Boersma 1963. See also below, figs. 141 and 142 and Haalebos 1976, 208; a publication on all river area coins has not yet appeared (cf. chapter 6, 134 and note 4).
97 For data see in general Bogaers/Rüger 1974, with further references. Qualburg is a difficult problem. The occupation by the numerus Ursariensium in about AD 260 could have been interrupted after the end of the Gallic empire. On Rossum, see also Haalebos 1976, 203–4, on Asperden, Hinz/Hömberg 1968, 192.
98 See Boersma 1963. The information is thus not up to date, but details about more recent finds (e.g. Teeffelen: W.J.H. Verwers 1981, 52) are not available.
99 For example, site 59 (Hien), and an occasional coin from Wijk bij Duurstede and Vechten.
was apparently never reoccupied after AD 260/270, and neither was Halder in North Brabant, which is the furthest away from Nijmegen in the hinterland. Although very tentatively, it may therefore be concluded that the efforts of Constantius Chlorus were not implemented by a complete restoration of the instruments of force, archaeologically visible in the occupation of forts or other important sites. This does not at all mean that the literary evidence is invalid, because when his victory was as complete as is suggested the whole area in the west and north of the Waal may have been controlled by power (that is, without being occupied but through treaties imposed on and observed by the invaders). This control may have been effectuated from a nucleated area around Nijmegen, encompassing the area between Waal and Meuse (Land van Maas en Waal) and along the southern bank of the Meuse and in the east south of the line Nijmegen-Qualburg.100

The reasons for this situation are difficult to ascertain, but on the eve of the campaign against the British usurper a shortage of manpower, material, and, above all, time, is not difficult to envisage. A plausible conclusion therefore, is that the sites occupied under the tetrarchy were presumably continuously occupied and reflect the area controlled by the remainder of the original population. These were at the same time the only trustworthy allies in the region. Through their centre at Nijmegen, then at and around the Valkhof (site 403) and no longer in the former civitas capital (site 399), and the other still occupied sites, military and administrative control must have been restored in the rest of the delta. That this was increasingly ineffective further westwards need hardly be said, although there is every chance that there was not all that much to control there, due to the effects of the Duinkerke II marine transgression phase. The habitable surface must have been severely reduced.

Troops were recruited in the civitas that was more or less restored after AD 293. Perhaps this was not even an entirely new levy but in part a reorganization of some still existing Batavian units, for example, of the kind of the 3rd-century numerus exploratorum Batavorum.101 As mentioned above, the total number of soldiers recruited, in itself already uncertain, could be used for a very dubious estimate of population size. There is every chance that the calculations given earlier (p. 436) are not realistic and the comparison with the reduction in the number of settlements from AD 275–425 relative to that from AD 50–275 is hardly if at all permitted. The favourable outcome by itself thus means nothing, especially not when it is supposed to be relevant for Diocletian’s reign, which we know so little about anyway. Nevertheless, it is probably correct. The general comparison between middle- and late-Roman sites can be taken as a valid figure for the late-3rd century because it should approximately equal the number of sites which continued to exist, even if the inhabitants were newcomers. Some unusual and perhaps late-Roman native wares have occasionally been found at such sites,102 but the opposite is also true (late-Roman imports in transrhenish sites such as nos. 1 and 7); and it is quite certain that there are no settlements in the river area with only hand-made late-Roman or Merovingian pottery that has not been recognized as such.103

The palynological data are, of course, a reliable independent third indication for the decreased total population of the area. The evidence (see fig. 137) is impossible to express in percentages with their false sense of exactness, but they do indicate precisely the same thing. We can safely assume that a reduction of the population to approximately one-fourth of its former level is a realistic estimate, and that does not exclude the possibility that there were intervals when the actual population was higher or lower.

Apart from the number of settlements, there is also the question of the size of settlements. As outlined in chapter 8, there is no real evidence in the river area for such large nucleated villages as are to be found even within 10 km north of the Rhine. But the clustering of sites, at a regional level around Nijmegen and locally around defensible settlements, does at least suggest that more people than before were living in the average settlement. In other words, clustering at the regional and subregional level is taken as an indication for clustering at the settlement level as well. It is possible that this con-

100 Note that this conforms to the overview of the situation given by Bogaers 1968, although part of the evidence and most of the conclusions are no longer acceptable (cf. below).
102 Cf. chapter 6, 180.
103 The addition to the list of late-Roman sites of those without wheel-turned late-Roman wares but with identifiable (middle- or late-Roman) transrhenish hand-made pottery would probably increase the survival rate of sites, but only by a few points.
tributed to the extremely low values for culture indicators in the pollen diagrams reached in the mid-5th century. Other factors may have been important, notably the departure of many Salii around that time, but there is no good reason why no-one else would have moved in after so many more or less futile attempts in the previous periods. It is therefore possible that the low values are also the result of increased nucleation of the population and its fields in the Late-Roman Period, causing a further growth and density of the river forest with the minimum values of the 5th century as a result.\textsuperscript{104} The trend was reversed again by a very gradual dispersal of settlement that became noticeable in the 7th century.

The regional centre, the former town of Nijmegen (399), had ceased to exist in the late-3rd century, although from a regional analytical perspective one might also say that the \textit{civitas} capital was reduced in size by the transfer from site 399 (Waterkwartier) to 403 (Valkhof). The new regional centre on the Valkhof hill and along the Waal at its foot has only partially survived post-Roman erosion by the river. Its population, especially somewhat later in the 4th century, probably did not exceed some 1000 souls, and if the calculations from the two cemeteries are relevant not more than 700.\textsuperscript{105} At that time, although conclusive evidence is lacking, a second large settlement of 300–500 people (Wijchen?, Castra Herculis?, Quadrriburgium?) is within the range of reasonable assumptions, as are others further down the line (Cuijk, Rossum). Although this line of reasoning quickly leads to pure speculation, there is every reason to believe that a late-4th-century rank-size curve (see fig. 132) would be not more than slightly concave, perhaps even slightly convex.

It can also be said that whatever the real curve will prove to be, it will be close to a lognormal distribution and thus conform to the rank-size rule.\textsuperscript{106} This is normally connected to an integrated and economically mature settlement system, and to a certain degree it may be interpreted in this way: the size of the largest settlement is in proportion to the surrounding region (its productive capacity) and the centre does not drastically interfere in horizontal relations between lower-order settlements. On the other hand, it must be realized that this situation was only very partly the result of an autonomous development. Ulpia Noviomagus may indeed have been declining already in the century before AD 275,\textsuperscript{107} but the final blow was dealt only at about that time with the partial elimination of the town’s red tape: the religious, administrative, military and other central institutions, and the people living off them. The new centre must have kept such a super-structure, but the deadwood, the artificially maintained part of it, should have largely disappeared in the 4th century. In Diocletian’s reign, the situation may already have been comparable. There is a chance that initially more people stayed in Nijmegen, but that is not very plausible. It is difficult to see where, between AD 276 and 292, sufficient food would have come from.

And there is no evidence for a sudden increase in arable land around Nijmegen in the late-3rd century, let alone for a cemetery which should have left some traces if there were still, say, 2000 or 3000 people living in the town. Such burials are exceedingly scarce anyway, and when current views on the chronological position of graves in the river area are maintained, virtually nobody died and was buried in a regular fashion between AD 270 and 300. Obviously, the chronologies of some cemeteries, in Nijmegen notably the end of site 398 (Hees) or the beginning of sites 405 (Mariëenburg) and 410 (Hugo de Grootstraat), or both, will need some revision. It should be possible to do this by reversing the normal procedure, that is, by assigning all graves for which there are no compelling reasons to date them pre-AD 270 or post-AD 300 to precisely that period (= Qualburg I). In view of the relatively short duration of the period, that could result in an acceptable number of late-3rd-century graves. An alternative or additional possibility is that especially in this period there may have been a relatively high incidence of very irregular disposal-of-the-dead practices.

The occasionally discovered 3rd-century mass graves are an example,\textsuperscript{108} but these may in reality reflect only small-scale disasters with enough survivors at least to bury the dead. A victorious horde of bloodthirsty barbarians, to state the case explicitly, may have been far less scrupulous, and the literary evidence is surely enough to envisage in some years masses of corpses floating down the Rhine, and thereby effectively out of the archaeological record.

\textsuperscript{104} See also the argumentation on p. 426 above.
\textsuperscript{105} Bloemers 1983, 193–5; above, p. 317.
\textsuperscript{106} Above, p. 418–9 and fig. 132.
\textsuperscript{107} Above, p. 418.
\textsuperscript{108} A.o. Destelbergen (De Laet a.o. 1969) and Gellep (e.g., Bechert 1982, 256).
Constantine

The limited evidence for Diocletian fortifications in the river area is compensated for by the quite apparent traces of the efforts under Constantine and his sons to consolidate the situation. The coins from the large ditch around the Valkhof (403) provide a possible date of around AD 330 for its construction,109 and the find of a stamp of the legio XXII, without the title C(constantiniana) v(ictrix), fits well into this.110 The general coin evidence, with a drastic increase after AD 306 (see figs. 1.41-2), also supports the notion of a Constantinian rebuilding program in Nijmegen, as do the relatively numerous burials from the first half of the 4th century. On the basis of the coin evidence, in addition to some other finds, it is very likely that the former frontier fort at Maurik (Mannaricum?) was also reoccupied at approximately the same time and perhaps the data from Rossum also point to Constantinian activity.111 The earth-and-timber fort of Cuijk (499) is reported to have been built 'under Constantinus I.'112 Other sites that should be noted in this context are the presumably still inhabited site 460 (Qualburg) and the evidently continuously garrisoned burgus at Heumensoord (391). In the hinterland, in addition to the information supplied by Von Petrikovits' overview, mention should be made of recent discoveries at Maastricht which have provided (dendrochronological) evidence for a Constantinian date of the fortification.113 From these data it appears that the Diocletian restoration of order at the frontier was finally implemented only under Constantine by the building of a true defence-in-depth system in Germania II. The earliest activity in this context may have been the construction, between AD 312 and 315, of the fortification Divitia, across the Rhine from Köln and linked with a bridge to the town.114 Most if not all new defences north of there seem to have been constructed in later years. It looks as if these measures may have been taken as a result of the trouble in these parts after Constantine's accession to the throne. That is indeed a possibility, but no such direct link need be presupposed, and not only because the archaeologically visible measures would in fact be a rather belated response. As mentioned above, it was Constantine who reorganized the army by the formation of central field armies; this is generally supposed to have taken place in about AD 312 but certainly before AD 325. This implied the inclusion of former border troops (limitanei) into the cohortes. Although at that time the frontiers were far from being depleted of garrisons,115 this is a decisive change of strategy. Even if initially perhaps the result of an immediate need (to defeat Maxentius or for other campaigns)116 it was never reversed. The further measures of refortifying strongholds on the borders, improving the infrastructure,117 and protecting it by road-forts are the logical and necessary complement of the reorganization. As the material from Maurik shows, the forward line of the defensive system did indeed extend up to the Rhine. In this case, the true nature of the Constantinian occupation remains obscure because the site - as so many others in the river area - was eroded by the river. It may well have been only a small burgus of the road-fortlet type, a watchtower, or a protected landing-place.118 The primary function may have been to keep an eye on the ever troublesome Chamavi, but it could also be related to the protection of the waterway. In both cases, we should expect the future discovery of more and perhaps similar sites along the Rhine, and in fact indications for this have emerged. Apart from the enigmatic Brittenburg on the coast,119 there is the possibility of Constantinian construction in Valkenburg. In a recent paper, Groenman-van Waateringe has presented detailed and incontrovertible evidence for her earlier voiced opinion

109 Cf. Haalebos 1976, 205; the coins from the later excavations are not yet available.
111 Haalebos 1976, 208-9. For Rossum the data are too limited to be confident about any dating or interpretation (cf. op. cit., 204).
112 Bogaers 1967, 111.
113 Von Petrikovits 1971. For Maastricht, see Panhuysen 1984, 64: 'the provisional results of the analysis point to a construction date in the last years of Constantine's reign'. Wood from rammed-in oak foundation posts under the late-Roman wall.
114 Von Petrikovits op. cit., 182.
116 For a concise overview, see Von Petrikovits 1978, 181-3.
117 In the Netherlands illustrated by the two Constantinian milestone fragments from Eygelshoven. See Brunsting 1946, 34-5 and AfB. 10.
118 The typology according to Von Petrikovits 1971, cf. respectively figs. 27, 28, and 24, and comparable to such installations as Hüchelhoven-Heidenburg, Asperden, or something like the ferry-terminal at Engers.
119 It is discussed below, p. 455.
that there is a post-Period 6 phase in the fort. Apart from a few late-Roman sherds, no datable finds have turned up so far, but wood samples have been analysed from a drain and from the construction of the horrea (used for repairs) as well as from a post beneath the south wall of the principia. One re-used piece of wood yielded a dendrochronological date of AD 223 ± 20, two others of AD 316 ± 10 and AD 365 ± 40, and the foundation post '4th century'.

These data are not conclusive evidence for a Constantinian date of the new Valkenburg Period 7 fort, although they prove that one of the oaks used was felled no later than AD 326. It is quite conceivable that repairs and new constructions were only made under Valentinianus. The few sherds are inconclusive in this respect and coins are lacking, but the most important aspect stands undisputed. The limes fort in Valkenburg was used again in the 4th century, with at least two horrea and a repaired principia within its perimeter.

In addition to Valkenburg, further down the line of former limes forts, mention should be made of coins from the period Constantinus I - Magnenius from Woerden, Vleuten-De Meern, Utrecht, Vechten, and in Wijk bij Duurstede (i.e., from Rijswijk-G. or Maurik ?), although in all cases only a few specimens. The same is true for the coastal sites at Aardenburg and Domburg and, in somewhat larger numbers, from Westerschouwen. Admittedly, these finds prove nothing and when more data are available, as in Utrecht, a later-4th or early-5th century date is more likely, as is also the case for places such as Rhenen, Meinerswijk, or Huissen. But they should certainly not be rejected and in view of what we know today it is more than likely that Constantinian activity will be demonstrated at one or more of these sites or in hitherto unsuspected places.

On the other hand, it is perhaps useful to stress that this suggestion should not be interpreted as a proposal to consider a Constantinian restoration of the former limes. A limes in the context of a forward defence line is totally incompatible both with the available evidence and the basic strategy of a defence-in-depth system. Within the context of such a system there is no line but, ideally, a broad zone honeycombed with fortified sites and a mobile force in the hinterland. In such a context and in the conditions of the delta, whole series of forts and fortlets along the Meuse, Waal, and Rhine are theoretically possible, if Constantine and successors did not have to worry about time, costs or manpower – but they almost certainly did. The refurbishing of some old limes forts for the first line of strongholds is an obvious labour- and cost-effective measure. But that does not mean that whole series of forts are to be expected.

Coinage

In addition to what was said so far, there is some evidence for military sites in the eastern river area hitherto left undiscussed. Most noteworthy are site 239 (Ewijk) on the Waal and the nearby site 244 (Beuningen). The former used to be a luxurious stone-built villa before AD 270, the latter must have been a simpler settlement (no traces of stone buildings), and they have in recent years yielded an astonishing number of late-Roman coins (figs. 141-2). Earlier coinage is not lacking, but its relative importance is minimal (fig. 141) and is merely further evidence for the long occupation history of both settlements. The coin diagrams of both settlements are very similar, indicating no activity under Diocletian, a sudden increase under Constantine, and again under Valentinian.

For a normal settlement, the quantity of coins itself is almost incomprehensible, and as argued in chapter 8 it cannot be explained away by the use of metal detectors: in the pre-detector age the absolute total of coins recovered would have been much lower but their relative sig-

120 Groenman-van Waateringej, 1977 and in press. I am very grateful to the author for providing the manuscript before its unduly delayed publication.

121 On this aspect, see below, p. 449.

122 Further literature in Bogaers 1967, 107, note 37.

123 Boersma 1967.

124 See below, p. 454.

125 The same is true for other unfortunate suggestions which have found their way into the literature, from the long-abandoned idea of a limes Belgicus (cf. De Boone 1954, 64 f.) to the more recent introduction of a Waal limes.

126 But see chapter 8, 309-12.

127 See chapter 8, 279 and 315.

128 I am grateful to W. Tuyn (AWN-Nijmegen) for providing the data. Most coins were determined by Dr R. Reynen and W. de Jong (Nijmegen), some by Dr J.P.A. van der Vin (Den Haag) and Dr N. Roymans (Amsterdam). The data in fig. 141 can only be regarded as provisional and could need slight modification or further additions.
Fig. 141 Coin series from sites in the central and eastern river area. White: gold and silver coins, black: bronze coins. Maurik, Rossum, Cuijk, and Valkhof after Haalebos 1976, fig. 3. The Valkhof coins (late-Roman only) do not include the finds from recent excavations. Data for Asperden after Geißen in Hinz/Hömbreg 1968, 207-12.
Fig. 142 Relative proportions of late-Roman coins from sites in the central and eastern river area. The provisional impression of the very numerous coins from recent ROB excavations at site 403 is that these will not drastically alter the relative proportions as presented here.

Significance in relation to other sites not less. Another explanation is therefore required and the thoroughly searched surface of sites 239 and 244 permits a first conclusion which could be very relevant with respect to other findspots. That is the relative proportion of late-Roman to all coins, which is 75% and 80%. For the excavation at Cuijk it is also 75%, but for the river finds from Maurik and Rossum only 16% and 8%. When taken at face value, the latter figures could be used to depreciate the relative importance of these sites in late-Roman times. But when, in view of the find circumstances, they are considered a valid indication for late-Roman occupation (as they are by all authors on the subject), then the data from the excavation and the detector-searched sites indicate that the small change of the 4th century is underrepresented, although not as strongly as would appear at first sight.

That is because a direct comparison implicitly assumes that both groups of sites were involved in the same pattern of coin circulation throughout their history, and that is an assumption which cannot be maintained. Before the Late-Roman Period, sites 239 and 244 were civil settlements, Cuijk was a secondary centre and for a short time a fort, the same is perhaps true for Rossum which may also have been a fort much longer, and Maurik was certainly a military site. Coins are strongly associated with soldiers and it is an unquestionable fact that they are found in much larger quantities in or near forts than elsewhere. Therefore, it is likely that late-Roman coins are less strongly underrepresented in Rossum and certainly in Maurik than might appear.

This argumentation could also be turned around by concluding that in Ewijk, Beuningen, and Cuijk the late-Roman coins are to some degree overrepresented. Because their chance of being included in the collections studied is in general less and in the samples studied perhaps equal to that of earlier coins, the only plausible interpretation is that this overrepresentation must be explained by a change in the function of the site, because there are enough other continuously occupied settlements where such a phenomenon cannot be observed. It is not likely that such very large quantities of late-Roman coinage are to be expected at all late-Roman sites, even though it would be logical if there were somewhat more late coins around than early ones. That is because there was much more small change. Perhaps it is exaggerated that the

129 The total number of coins recorded is 336 for site 239 and 123 for site 244. With only a few exceptions all 'indeterminable' coins are late-Roman (75 and 28 specimens) and as far as attributable to IVA vs. IVB the proportion is about 2:8. Undoubtedly most of these (small, worn down, fragmented) coins are the ones most likely not to have been found without a detector. The relative proportions (fig. 142) are therefore also comparable to those from the other sites.

130 This could have further implications for some of the sites with a few late-Roman coins mentioned above, but on most the quantities are so small that it is not useful to speculate too much about this. Unless, of course, one is prepared to accept that many coins are a characteristic of all late-Roman sites.

131 See note 130.

132 For this argument, see Reece 1973 with respect to the monetary situation in Britain compared to that in the western empire as a whole.
Zwammerdam is 114, for Vechten 836, and for Maurik 292. Representative collections are aes-iv with some aes-ni. Compared to Asses. By contrast, most of the late-Roman coins in fig. field-drawing 137 See 1977, 3.

136 Glasbergen 1967, 63. By comparison, the figure for Zwammerdam is 114, for Vechten 836, and for Maurik 292 (see Haalebos 1976; 1977).

137 See 1977, esp. 230–1 and the field-drawing (fig. 3).

Later forts must be sought. The process of ‘removing the topsoil’ may thus have been responsible for the effective removal of late-Roman artefacts. The accidental discovery of two late-Roman terra sigillata sherds from this upper stratum may illustrate the situation, but even then it remains curious why the first excavation planes above the Roman level, with much early-(??) medieval material, did not yield somewhat more late-Roman finds. When the available data are combined there is, however, a possible interpretation. For the moment, we may accept that most 4th-century artefacts have indeed not been recovered, but only on the condition that the total amount of such finds was fairly limited. As for features, we do have definite evidence for 4th-century constructions. But we do not have evidence for a heavily fortified 4th-century stronghold: at best, the defences of the Period 6 fort were still used and no new ditch appears to have been dug. A combination of the evidence suggests a possible solution, and one which was offered earlier by Glasbergen in his 1967 summary: ‘Two rim-fragments of a fourth-century bowl Chenet 320 suggest a short visit of Roman forces at Valkenburg in the 4th century (during the expeditions of Julianus in the late fifties?)’. The visit must have been less ephemeral than Glasbergen believed, but it was not of long duration either. For the moment, we can perhaps best see it as a temporary reoccupation to establish a grain depot in the context of one or another campaign, of the kind that Julianus erected along the Meuse by repairing three forts. This also removes one other difficulty, namely, that the only kind of more permanent Roman fort which could legitimately be expected in such an exposed position is the usual self-contained stronghold with massive walls and a wide ditch. Although a defence-in-depth system, let alone a *limes* line, in the area behind the coast is almost unthinkable because we know that there was not much worth protecting, there is no reason to reject such artefacts for such an assumption.

137 See e.g. the description of the 1962 excavation by Glasbergen 1967, 16–20. Earlier campaigns under Van Giffen may have been less scrupulous, but in later excavations (1980) special attention was paid (under unfavourable circumstances, cf. Groenman-van Waateringe, in press) to possible 4th-century finds.

139 Further reinterpretations may perhaps prove this wrong, but at present it is unacceptable that Valkenburg was converted into a true 4th-century fort: there are far too few artefacts for such an assumption.


141 Amm. Marc. XVII 9, 1–2.
strongholds in principle. The literary evidence quite explicitly shows that the Rhine was used to transport grain at least in IVB, and there is not one good reason why we should read Waal there. And in contrast to our very early sources the later ones were well aware of the difference between Waal and Rhine.\footnote{142} Protected landing stages are entirely conceivable in this context, as well as a transshipment base at the mouth of the river (Brittenburg?). But Valkenburg 7 was not one of them or only very temporarily, and its possible Constantinian date could, if true, be one further indication. The classical sources suggest that the really large-scale efforts on the Rhine did not take place until Julianus and later.

\section*{Batavia Salica}

Shortly after AD 350, the Frankish invasions reached massive proportions in the wake of Magnentius’ usurpation. It is only logical that this should have had a disastrous effect on the river area. The Betuwe may have been subjected to some kind of invasion, followed by a treaty with Constans, at an even earlier date,\footnote{143} but this time the defence-in-depth system could not function, apparently for lack of troops. The disaster was now complete, as we are told, and the numerous hoards,\footnote{144} in our area for example in Kessel on the Niers (site 467),\footnote{145} may illustrate this. Moreover, there are good reasons to assume that among the towns conquered by the Franks was Nijmegen. The coin evidence from the large ditch shows that it must have been filled in shortly after AD 350,\footnote{146} as well as the two outer ditches.\footnote{147} When Nijmegen fell, it followed that the surrounding fortifications of the Constantinian system would share the same fate, and from what we know this is precisely what happened. For the best known of these forts, Cuijk, it has been concluded on good grounds that the Constantinian installations were replaced by a new stone fort, perhaps under Valentinianus I and Valens,\footnote{148} or earlier under Julianus and Constantius II.\footnote{149} A connection with the events in the early 350s is therefore quite likely, and the same applies to the installation at Maurik, where the coin series breaks off sharply with the issues of Magnentius and Decentius.\footnote{150} A similar conclusion can be drawn for sites 239 and 244, and for Rossum (cf. figs. 141–2). One other coin series, that from Westerschouwen on the coast at the mouth of the Scheldt,\footnote{151} can also be interpreted in this way; at other sites the number of coins is certainly too small for further inferences.

There is thus every reason to accept the conclusion that with all its strongholds and refuges gone, society in the river area as it had continued to function within a sometimes more, sometimes less coherent form ceased to exist. What happened to the population we can only speculate about, and necessarily in a very negative way. Even though we lack excavations of rural settlements which might illustrate what happened, there are a few indirect clues which can be used as evidence for a wholesale replacement of population groups, in addition to the explicit historical evidence. One element thereof has not yet been mentioned, namely, the absence of the \textit{ducatus} Germania II from the \textit{Notitia Dignitatum}.\footnote{152} This may be accidental, but that is not very likely. After all, both northern \textit{civitates} of Germania II are absent in the \textit{Notitia Galliarum} (VIII). It is generally accepted that they disappeared after the mid-4th century,\footnote{153} their former territories being inhabited by Frankish federates, who were of course responsible for the defence.\footnote{154} Had former conditions been restored, this situation would presumably have been rather different.

Archaeological indications for the change can to some degree be deduced from the burial evidence, meagre as it is.\footnote{155} The cemeteries which show continued use into the 4th century (sites 292, 413, 418, and 419) do so only up to the middle of the century. \textit{Elsewhere} (sites 12, 16, perhaps 321) cemeteries start only after the mid-4th century (sites 292, 413, 418, and 419) do so only up to the middle of the century. Nijmegen is the exception, because there the new cemeteries begin in the late-3rd century. That is understandable in view of the move from the former town to the Valkhof site, but both sites 405 and 410 remained in

\footnotesize
\begin{enumerate}
\item[142] Cf., e.g., note 58. Which does not mean that the Waal or other rivers were not used.
\item[143] See above, p. 436.
\item[144] Von Petrikovits 1978, 187.
\item[145] See above, p. 319. A second but dubious hoard perhaps around Wageningen-Renkum (cf. ch. 8, note 325).
\item[146] Cf. Haalebos 1976, 205. The new excavations at site 403 may provide further evidence in this respect.
\item[147] Above, p. 306–7.
\item[148] Bogaers 1966, 68–70; 1967, 111; see also figs. 141–2.
\item[149] Haalebos 1976, 205–6; see also Bogaers 1966, 70.
\item[150] Haalebos 1976, 208.
\item[151] Boersma 1967, 69 and 95–6, but also 72.
\item[152] Occ. 5.
\item[153] E.g., Von Petrikovits 1978, 217; see chapter 8, 312.
\item[154] Cf., e.g., Böhme 1974, 201.
\item[155] See above, paragraph 8.5.3.
\end{enumerate}
use after c. AD 350, although the provisional analysis of the Hugo de Grootstraat cemetery (site 410) shows that the later graves constitute a clearly discernable group. The evidence is not nearly good enough, but we may at least say that discontinuity in burial sites in the mid-4th century is a possibility, in addition to the quite obvious changes in burial rites.

The last observation need not be extensively discussed here, as it has been fully documented in the comprehensive and highly illuminating study by Böhme. The characteristic Germanic objects in male and female burials dominate only after the early-4th century and have been associated with a Germanic population group intimately associated with a military function. The graves offer an interesting amalgam of burial practices (acculturation?), in the adoption of inhumation on the one hand and the introduction of weapons and other personal equipment on the other. The detailed analysis of typochronological developments has also shown that there must have been very close contact with the transrhenish population up to the North Sea coast until the early-5th century, when uniform developments ceased. Yet another indication for the change in the river area may be constituted by the pottery evidence. Perhaps this is caused by the somewhat obscure position of late-3rd- and early-4th-century pottery, but it is nevertheless remarkable that where we are sure of dealing with late-4th and 5th-century wares these are suddenly found again all over the inhabited Betuwe sites. They are also present at contemporary sites south of the Waal, and it could be that they indicate a reoccupation of the only very sparsely settled region between Waal and Rhine by the new immigrants. Such pottery is also available from settlements north of the Rhine and has been found as far as the North Sea coast, but it decreases rapidly in numbers north of the river.

The above development is, of course, only understandable under the conditions of relative safety which were established after the campaigns of the caesar Julianus and the following restoration and further development of the defence-in-depth system under Valentinianus.

The archaeological data, limited as they are, indeed confirm such an enterprise and also some of the claims considered exaggerated by several more recent studies. The above-mentioned late-Roman pottery and its distribution already show that Roman force was restored up to the Rhine and slightly beyond. The view that Germania II was reduced in size in the north and reached only to the Waal can no longer be upheld. On the contrary, following the earlier but perhaps less fully implemented Constantinian program, it might be argued that now Germania II, for the first time after the fall of the limes, was completely and effectively extended to its former border again, and precisely at a time when the river area was entirely occupied by Franks.

The defensive system: a first approach (fig. 143)

The new fortification program may have involved two stages, with Julianus’ measures being followed by the Valentinian enterprise a decade later, but such a fine distinction can only be maintained with sophisticated data. These we do not have, apart from the never wholly certain identifications of sites with historically attested places, which sometimes give a precise date. When reviewing the possibilities in this respect the three forts lying in a straight line along the Meuse, which were restored by Julianus in AD 358 to serve as storage depots, are the first to come to mind. One of these may have been Cuijk, repairs perhaps being limited to restoring the timber work of the earth-and-timber wall, as observed during the excavation. As far as the other two fortifications are concerned, their location depends on the direction of Ammianus’ straight line. If north-south, the next site could have been Lottum, a probable 2nd-3rd-century road station where late-Roman pottery and coins have been found, and the last one Blerick. There are other sites further south, but these three have the advantage of being not too far away. On the other hand, Ammianus reports that the corn of the Chamavi was not yet ripe and Julianus had to deal with a mutiny of hungry soldiers, so it could be that the refortified depots were indeed too far south. In that case two further...
Fig. 143 Model of the 4th-century defence-in-depth system of Germania Secunda and part of Belgica Secunda. In view of the numerous uncertainties and unknowns, no attempt has been made to distinguish between the different (Diocletian, Constantinian, and Valentinian) stages of development of the system. As a model, the system as drawn here should be valid for the last two of these stages; as a representation of facts and some assumptions, it is largely valid only for the Valentinian system. To avoid a complicated legend, different but functionally comparable sites have been indicated by the same symbol.

Legend: 1 coastal dunes; 2 marine clay deposits and peat; 3 fluvial deposits; 4 Pleistocene deposits; 5 fortified towns; 6 less or more certain military fortifications; 7 less or more certain road forts (fortified settlements and *burgi* along land- or water-routes); 8 other late-Roman urban settlements. Sites: 1 Oudenburg, 2 Domburg, 3 Westerschouwen, 4 Oostvoorne, 5 Brittenburg, 6 Valkenburg, 7 Veulzen-De Meern, 8 Utrecht, 9 Maurik, 10 Rhenen, 11 Rossum, 12 Kessel, 13 Wijchen, 14 Ewijk, 15 Driel, 16 Meinerswijk, 17 Huissen, 18 Nijmegen, 19 Heumensoord, 20 Cuijk, 21 Asperden, 22 Qualburg, 23 Altkal- 
kar, 24 Xanten, 25 Rheinberg, 26 Lottum, 27 Blerick, 28 Moers-Asberg, 29 Krefeld-Gellep, 30 Neuss, 31 Haus Bürgel, 32 Bonn, 33 Remagen, 34 Junkerath, 35 Zülpich, 36 Rövenich, 37 Brühl, 38 Hückelhoven, 39 Jülich, 40 Heerlen, 41 Huls-berg, 42 Maastricht, 43 Stokkem, 44 Heel, 45 Amay, 46 Ver- 
voz, 47 Oreye, 48 Braives, 49 Taviers, 50 Namur, 51 Pente- 
vilie, 52 Liberchies, 53 Morlanwelz, 54 Waudrez, 55 Givry, 56 
Famars, 57 Kortrijk.

sites should be mentioned, namely, Catualium and Fe 
resne, both indicated on the *Tabula Peutingeriana*. Ca 
tualium is probably Heel and Feresne could be the 
eroded late-Roman installation at Stokkem (Belgium).165 
Finally, there is also the Constantinian fortification of 
Maastricht but that is not likely to have been one of the 
sites mentioned by Ammianus.

When, however, the line of fortifications ran east-west, 
Cuijk was almost certainly one of them. There is much 
to say for such an east-west location, because for a cam 
paign against the Chamavi it makes tactically much more 
sense to have three supply depots as a broad base-line for 
the operations than to have them lined up deep into the 
hinterland. The shortage of food may have been due to 
a variety of causes and not necessarily to distance. When 
the possibilities are reviewed for the other two depots, 
there is at least one site which could qualify: in Kessel, 
not far from Rossum. As in Stokkem, washed down wall 
fragments of what was probably a late-Roman fortifica 
tion have been discovered, with appropriate pottery and 
coins (a.o. from IVB).166 Could it be that this is the long-
sought site of *Ad Duodecimum* from the *Tabula Peut-
ingeria?*167 The third supply base in this case is as yet 
unknown, unless Rossum could qualify. This site was 
reoccupied under Valentinianus.

For the forts on the Rhine restored by Julianus we have 
the quite likely identifications of sites 460 as Quadribur-
gium and site 126 as Castra Herculis. For the remaining 
strongholds of the late-4th-century system, no very pre-
cise datings are possible. It included, in the eastern river 
area, the two *burgi* at sites 391 (Heumensoord) and 466 
(Asperden), site 239 in Ewijk, and perhaps site 315 (Wij-
chen). In the hinterland, no other potential strongholds 
can be identified, although the continuing occupation, 
let alone the Germanic soldiers’ graves, in Nijmegen is 
more than enough evidence that at some point the site 
was also restored. Valentinianus himself may even have 
been there on 20 September 368, as we may infer from a 
law signature.168

Back to the Rhine. Ammianus Marcellinus, although in-
clined to write favourably about Julianus, was a respon-
sible historian, and as an army officer who had served in 
Gaul and wrote about events during his own time he de-
serves to be taken very seriously. When he writes that 
Valentinianus built and repaired forts all along the 
Rhine as far as the ocean,169 we are not at liberty to read 
‘Waal’ without solid proof. Such proof is lacking, de-
spite the fact that on the Waal some strongholds can be 
expected and are indeed present. In addition to the habi-
tation evidence in the Betuwe and Castra Herculis at 
Meinerswijk, there are other sites to be considered. Be-
tween Meinerswijk and Qualburg there may be a fort at 

165 On Heel, see *ER III*, for Stokkem, Mertens, in Bogaers/ Rüger 1974, 153.
166 See *NKNOB* 1977, 189 and Haalebos, in press.
167 The *tabula* locates the site at 18 *leugae* from Nijmegen, 
which could fit. Even if the name does mean that the place was 
located ‘at the twelfth league-stone’ as current assumptions 
maintain, that need not mean that it was 12 leagues from Nij-
megen. Unfortunately, this causes problems with the identifi-
cation of Rossum = Ginnes at v (some say vt) *leugae* from Ad 
Duodecimum, but we can easily declare this corrupt and read 
11, which could be nearly correct. But then the site is still on 
the wrong side of the present-day Meuse. See also chapter 3, 
70, notes 218 as well as 217.
169 Amm. Marc. XXVIII 2, 1.
Huissen (site 135), and downstream from Meinerswijk is Driel (site 123).\footnote{On both sites, see chapter 8, 309. The absence of much coinage in both cases follows from the find circumstances.} Next there is Rhenen (site 12), where all we have is a cemetery with Germanic soldiers, but Böhme’s interpretation that they manned some kind of military post on the high Pleistocene ridge there is plausible.\footnote{See chapter 8, 317-8.}

Further downstream, Maurik may have been reoccupied again by Julianus or under Valentinianus. There are too few coins to be certain, but then the very small latest Roman aes-\textit{iv} would be the first kind of coin to be underrepresented in view of the find circumstances there. The finds also include a late-4th-century hairpin and parts of crossbow fibulae.\footnote{The finds are only briefly mentioned (Haalebos 1976, 209) and include pottery. At least the hairpin must be assumed to date from the late-4th or early-5th centuries (Böhme 1974, 35 f.).} No statements can be made as yet about Rijswijk (g).\footnote{Van Es 1984, 280-1.} The late-Roman finds ‘from Wijk bij Duurstede’ may originate from this site, but that is wholly uncertain. Likewise, Vechten in late-Roman times must also be considered doubtful. The minimal amount of coins and pottery, if anything, would suggest early rather than late-4th-century activities.\footnote{The one post-Magnentius coin is one of Honorius, but it is a solidus.} Utrecht, however, has yielded more useful evidence. Van Giffen ascribed phases \textit{iv a-c} to constructions under Julianus, but apparently this need not be true.\footnote{See Bogaers 1967, 107, note 37, which does not give any conclusive evidence against the earlier interpretation. Professor H. Brunsting, however, is also in doubt (pers. comm. 1980).} Nevertheless, there are some coins up to Valentinianus and there is a not inconsiderable collection of late-4th and early-5th-century pottery, in part from a significant stratigraphical position.\footnote{I am grateful to the town archaeologist, T.J. Hoekstra and collaborators, with whom the finds were examined in November 1983. They contain several Alzei forms including also the (type 3) cooking-pots with thickened, cordoned rim discussed in chapter 6, 168-70. Moreover, part of these finds was found under the collapsed wall of the fort. There is also Merovingian material among the sherd collection.} In addition, recent discoveries include three graves from a hitherto unknown cemetery with the earliest datable excavated burial dating to the second half of the 5th or the early-6th centuries.\footnote{De Groot/Hoekstra a.o., \textit{NKNOB} 1982, 173-6.} At the same time, these burials provide a \textit{terminus ante quem} for an as yet undatable, artificially raised stratum overlying an earlier, late-2nd-century raised layer. For the moment, even if Van Giffen’s arguments are questionable, there is thus every reason not to reject his conclusions.

At the next former \textit{limes} fort, in Vleuten-De Meern, relevant finds are apparently meagre, but the excavations have yielded two massive foundations which postdate the late-3rd-century destruction of the fort.\footnote{See Jongkees/Isings 1963, 8-11, 17, 38, 98, and Plate 1.} The excavators suggest these should date to the 4th century, an observation which becomes even more relevant in the context of the site, where post-AD 100 layers are severely disturbed and at least 1-1.5 m (all upper strata) were dug off in the past. The limited excavations provided no further details and all we have from Vleuten is apparently one coin of Magnentius. This prohibits further conclusions beyond those of the excavators. Of the three

\begin{figure}[h]
\centering
\includegraphics[width=\textwidth]{fig144.png}
\caption{Plan of the Brittenburg as depicted by Abraham Ortelius in AD 1568.}
\end{figure}
possible dates for the building activities, IIIa, IVa, or IVb, the last two are the more likely.

In Woerden, where all upper levels were also removed, presumably in about AD 1700, and where not only Merovingian but even late-Medieval pottery is very scarce,\textsuperscript{179} stray finds include coins from Valens and Theodosius in addition to the Constantinian specimens.\textsuperscript{180} For Bodegraven and Zwammerdam evidence for late-Roman activities is lacking.\textsuperscript{181} For Alphen all we have despite many finds in recent years is an old reference about 'coins up to Honorius', not necessarily invalid but not very informative either, and for Leiden-Roomburg all we have is inconclusive literary evidence.\textsuperscript{182} Apart from the already discussed evidence for Valkenburg, no 4th-century finds are known from the remaining sites towards the coast. There is, however, the enigmatic Brittenburg (fig. 144), as a possible late-Roman fortification the subject of much discussion recently revived again.\textsuperscript{183}

The walls with half-round projecting towers are now considered the remains of a 3rd-century town wall of Lugdunum, of a 4th-century fort, and even of an early-Medieval castle. None of these proposals can be preferred on the grounds that it provides a better context for the two curious double-towers at the corners. But all findings, especially those building material and the evidence on the foundation method,\textsuperscript{184} indicate that the ruins must have been those of a Roman installation. The limited reliable finds from the site cover the period from about AD 100–250, those from the 1982 excavations have been dated to AD 160–240. The evidence allows the conclusion that there was a \textit{limes} fort here, presumably occupied by the \textit{cohors I Raetorum civium Romanorum p.f.}, and also a naval base of the Classis Germanica (20\% of all tile-stamps). The numerous military tile-stamps (35 from 1982 plus 23 older ones) cannot be used as evidence against a civilian town there, but the only advantage such an interpretation has over a designation as 4th-century fortification is that contemporary finds are available. The significance of that fact is limited, because the 1982 excavations were at a considerable distance from the actual Brittenburg, at the outer perimeter of the \textit{vicus}, and the lack of late-Roman finds still surviving after their discovery in the 16th or 17th centuries is of course entirely undecisive.

For the moment there are thus no compelling reasons to reject the Brittenburg as the heavily fortified late-Roman stronghold of c. 75 x 75 m it seems to be, its central \textit{horrea} indicating a function as a (grain) storage depot. This is compatible with the Valentinian defensive system as discussed below; in a way it is even a necessary element thereof, as a transshipment base as well as to block access to the Rhine.\textsuperscript{185}

In addition to the Brittenburg, there might be other late coastal sites relevant to the present theme, especially at strategically important sites comparable to the mouth of the Rhine at Katwijk. Bogaers has recently discussed the evidence for the Helinium, Tacitus' \textit{os immensum} and the mouth of Waal, Meuse, and probably a branch of the Scheldt.\textsuperscript{186} The dispersed evidence is apparently sufficient to assume one or more military sites, a naval base and (or in combination with?) a fort where cavalry was stationed, perhaps to be localized at Oostvoorne where in the 18th century foundations were seen. Although the evidence for late-Roman activity is thin, the suggestion of a late stronghold is indeed acceptable and for very much the same reasons as the Brittenburg. For the mouth of the Scheldt, the coins from Westerschouwen on the northern bank and from Domburg on the southern bank should be mentioned.\textsuperscript{187} The vague indications for 4th-century activity in Aardenburg and Brugge are probably irrelevant,\textsuperscript{188} the more so because the main road into the hinterland from there was effectively protected by the fortification at Oudenburg.\textsuperscript{189}

\textsuperscript{179} Bogaers/Haalebos 1983, 309.
\textsuperscript{180} See ER III, 133.
\textsuperscript{181} Haalebos 1977, 203, 216, mentions one possible 4th-century coin from the \textit{principia} at Zwammerdam.
\textsuperscript{182} Alphen: cf. Bogaers 1967, 107, note 37; if Roomburg = Matilo, then the site is mentioned by the Anonymus Ravennas (IV, 24), cf. fig. 140. But this cannot be used as evidence in the present context.
\textsuperscript{184} See Dijkstra/Ketelaar 1965 for an overview of relevant drawings.
\textsuperscript{185} In this respect, the so-called 'tower of Kalla', a round tower of which nothing is known except that it was further into the sea than the Brittenburg, is perhaps relevant. Together with the two walls running from the fort into the sea one might entertain the notion of harbour works.
\textsuperscript{186} Bogaers 1974.
\textsuperscript{187} Boersma 1967.
\textsuperscript{188} See Willems 1983a, 123, with further references.
\textsuperscript{189} See Mertens/Van Impe 1971 for a discussion of the fort as well as the cemeteries.
The available information mentioned so far has been combined in fig. 143, together with other published sites. With respect to several of the indicated sites, their indication on the map rests on uncertain or real but very limited evidence. That this is due to complicating factors such as the destruction of late-Roman levels in some cases or the erosion by rivers and by the sea in others, is true. Also true is that more research is needed at several tangible sites, and that a combination of conclusive and inconclusive evidence cemented by wishful thinking should not lead to presenting fiction as fact. Therefore, fig. 143 as a whole can be regarded only as a model of the situation, but one which is based on hard evidence and on the analysis of that evidence, to which the uncertain elements have been added because they agree with that analysis. They are also based on the assumption that accidental or surface finds, or those from limited excavations, are normally indicative of much more contemporaneous artefacts after more research. For the eastern river area, the find-histories of numerous sites confirm that assumption. When the context is different, such as with large excavations, the interpretation must be different also. Zwammerdam, with one (uncertain) 4th-century coin, cannot be considered in the present discussion. Valkenburg, however, shows that even a mere few sherds may yet be very significant indeed, although the scarcity of finds prohibits inferences about a prolonged reoccupation of the fort. As far as the river area is concerned, the imported wheelturned pottery shows that the region was relatively densely settled in the east with people up to the Rhine having direct contact with the hinterland. In the central part, research has not yet been completed, but a first published distribution map shows that settlements with similar evidence for late-Roman habitation are present although in lower numbers. In the coastal area, such sites are virtually unknown. Although the effects of the marine transgression phase must have been important, some areas were obviously still habitable and we may conclude only that we are dealing with a probably very sparse habitation, the people involved not being in any regular communication with the hinterland.

When the fortifications are added to this picture, there is good evidence for protection of the frontier zone in the east and somewhat less in the central river area. For the moment, there is every reason to assume that the area east of the line Utrecht (or Vleuten)–Rossum was protected by a defence-in-depth system, and it is hardly a coincidence that this is the region where the late-Roman pottery is found. Entrance to the area over water was controlled by front-line strongholds: from the Flevo lake, if the IJssel could be used, by Meinerswijk–Castra Herculis (and Huissen?) and for the Vecht by Utrecht–Traqiectum; from the coast over the Lower Rhine by Utrecht (or Vleuten?, or Woerden?), over the Lek, then presumably a functioning river, perhaps by Rijswijk–G. Wijk bij Duurstede or Maurik; and from the Helinium over the Waal by Rossum and over the Meuse by Kessel.

The latter two sites may also have served to block a road into the hinterland to Tongeren, which quite definitely was a function of the Cuijk fort. The necessary intermediate posts also seem to be present, with Rhenen on the Rhine and Nijmegen on the Waal as the best examples, to which can be added Ewijk on the Waal, less certainty Wijchen on the Meuse and Driel on the Rhine, and quite uncertainly Vechten. For land routes, Heumensoord and Asperden are of course very important intermediate stations. In spite of the limitations of the evidence it is thus quite clear that the entire river area was under Roman control in the later 4th century, and it is also obvious why this should be so because we know that the area was a vital link in the transport of British grain. If this had been transported only over the Waal, the control of Batavia (i.e., the area between Rhine and Waal) was necessary for securing the waterway. But our sources do not say so; they tell us instead that even the foreland beyond the Rhine was, at least from time to time, under Roman control.

Transport over the Lower Rhine may, occasionally, have been rather perilous. But there is no reason to assume that the Germanic soldiers manning the strongholds were just sitting there waiting for the next attack. In general they must have known reasonably well what was going on in the north. Also, it may have been advantageous to use the Lower Rhine. Even aside from the ingen 1983) have not provided any indications except for the long-known coins from Voorburg–Forum Hadriani.

191 See chapters 3, 71–2 and 4, 90–1.
192 Van Tent 1984, 259, fig. 78.
193 Recent detailed inventarisations of sizeable areas, such as Midden-Delfland and Den Haag (see Bult 1983; Van Heer-
psychological impact of demonstrating power, refurbishing former forts was easier than building new ones. In addition, the Lower Rhine was a relatively small and safe river. The Waal was larger and, perhaps very important, it had its mouth in the Helinium. The effect of the Duinkerke II transgression and periodical storm surges must have made the *immensum* as even more immense, with numerous creeks and gullies, constantly shifting shoals and sandbars, and thus not a very attractive area to pass through. There is, however, no reason to assume this was not done and, in order to be able to control passage through the area, a fortification there must be considered a strategical necessity.  

To return to the Rhine: in view of the recent discoveries, Valkenburg can now be used as definite evidence for the fact that the last stretch of the Lower Rhine was indeed considered to be of interest. Even if itself only relatively shortly occupied in the context of one or perhaps more campaigns as a depot, it demonstrates the army’s concern with the river. Valkenburg thereby places the other finds, even though in many cases we cannot yet be sure about their true meaning, in a logical context. And that includes the Brittenburg, which can be profitably seen as a stronghold blocking the entrance through the mouth of the Rhine in case of seaborne raids, and as a transshipment base.

Social and economic developments
Estimating a population size for the late-4th century is a hopeless task. We must be content with the observation that as far as archaeological evidence for occupation is concerned, it was probably larger than at the beginning of the century. If we consider that the Salii probably manned most if not all of the strongholds and in addition provided an *auxilium* for the field army, that could support such an interpretation. If there remained some organized administration, its centre should still have been located in Nijmegen and the fact that Valentinianus himself may have been there could be relevant. We know at least that habitation continued at the Valkhof and below it on the bank of the Waal, and from the burial sites it is evident that Germanic soldiers and their wives lived in Nijmegen. Conceivably they were the retinue (later called *antrustiones*) of a (the major?) king of the Salii residing at site 403. The only other site which could have been of administrative importance is Wijchen (315), but all we have from there are surface and dredged-up finds. The defensive system and the region’s importance for the Rhinelanl and allowances the conclusion that at least the broad framework of a Roman administration may have existed, but there was also an effective internal socio-political organization.

This can be deduced from the earliest codification of the Lex Salica, the *pactus legis Salicae*, formulated between AD 507 and 557 presumably under Childeric’s son Chloroweg (that is, before 511).  

To a certain degree, river area finds confirm this. The Germanic soldiers’ graves and those of their wives – as analysed by Böhme – fit into the higher echelons of the historically attested social strata. Böhme sees their appearance as a new and original development of the recent immigrants, conscious of their important position in Gaul. In the graves there is a very rapid development towards rather uniform grave goods and stereotype combinations which may be profitably seen as marking social status. The distribution of characteristic grave goods such as abundantly decorated belt-fittings all along the Rhine-Danube frontier are an indication for the development of a new, and this time Germanic, ‘military caste’ along the frontier, archaeologically even more clearly visible than that of the late-2nd and early-3rd centuries.

Again, fibulae were also a distinctive element, and not only in female dress. The role of crossbow fibulae as insignia has already been mentioned, and in our area it is paralleled by the related *Stütsarmfibel*, with a marked concentration in the river area and also in the northern coastal area between Elbe and Weser. An extraordinary find in this context is the gold specimen from Wijj-
chen (fig. 89), and in ascribing it to a high-ranking Germanic official we should not be far off the mark. In paragraph 8.5.4, the gold torcs such as those of the ‘Velp-type’ (see fig. 145) have also been interpreted as probable status symbols of very high-ranking individuals. Their concentration in the eastern river area and specimens to the north (not surprisingly that part where the Salii, and perhaps the Tubantes, came from) and east of the Rhine is yet another indication of the close ties with transRheno-Belgic groups. Perhaps their occurrence in hoards can be connected to the Salii’s great move southwards. The gold hoards quite obviously belonged to chieftains, a very large one such as the first hoard from Velp may qualify as the thesaurus of one of the reges criti-nos. It is possible that, contemporary to or in the wake of (if not indeed in coordination with) the Salii under Chlogio, other groups set into motion, at first of course only the warriors and their leaders who are not likely to have taken their own thesaurus with them. The hoards can thus be seen as instances when the chieftain and others who may have known its location did not survive the campaign. The precise date of burial is difficult to ascertain, and in many cases ‘about AD 400’ may be proposed, but ‘VA’ would do just as well, because that is the phase of the great gold hoards. There is a difference between hoards with torcs and those consisting only of solidi. But in terms of wealth it is remarkable that of the very large hoards, containing 0.5 kg of gold or more, all but one (Dortmund) have been found on the Rhine, a second category of 0.1–0.5 kg is virtually limited to transRheno-Belgic hoards, and only the hoards of the third category of less than 0.1 kg occur on both sides of the Rhine. This can be the combined effect of Frankish chieftains’ treasures being buried beyond the Rhine and the fairly effective retrieval of gold by the Roman state behind it. But it may not be a coincidence that four very large hoards were found on the Rhine, two near Xanten and two in the eastern river area.

It may be a symptom of the growing prosperity and economic activity in the late-4th century, centred on the Rhine area and affecting the regions within and beyond the frontier. For example, the river area could be the source of some of the late-Roman or earliest Medieval metalware, such as the Stützarmfibulae and long hairpins. Other indications are the well-known terra nigra-like cups Chenet 342 (fig. 37), which have a distribution between Elbe and Seine comparable to the metalware studied by Böhme, with the densest concentrations on the North Sea coast, the Lippe/Ruhr area, and the eastern river area. In principle, these cups may be considered a ‘Frankish’ development and the product of native form and Roman technique; and there is some evidence for production in the Lower Rhine area.

In this respect attention should also be drawn to the tentative identification of a group of pottery labelled lower Rhenish (or Frankish) grey sigillata derivatives that was discussed at length in chapter 6. Whether the label is correct remains to be seen, but in any case the group as such is again an original development which from many points of view (fabric, decoration, chronology) stands between ‘late-Roman’ and ‘Merovingian’.

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200 See also Haalebos, in press.
201 There are also typologically identical bronze specimens. See Waterbolk/Glasbergen 1955 for discussion and distribution map.
202 See above, 440 and note 83. In view of the amounts of money paid or received in various contexts as mentioned in the Historia Francorum, the true significance of even the largest hoards is very difficult to evaluate.
203 The same was later the usual practice for the Frankish kings; the thesaurus was kept in the king’s residence and stayed there when he went away on campaigns or on other business: see Weidemann 1982, 19–20. It only moved when the residence changed.
204 For an overview, see Bloemers 1983, 198–9, to which should be added the (unquantifiable, but probably worth over 500 solidi) first hoard of Velp (cf. chapter 8, note 390) and the hoard of Xanten-Menzelen of at least 207 solidi (cf. chapter 8, note 338).
205 See further paragraph 8.5.4.
206 The two Velp hoards, sites 28 and 30. The significance of the location of these two hoards is uncertain. Perhaps they can be connected to Meinerswijk-Castra Herculis, but it is entirely conceivable that archaeology has so far failed to locate a very important late-Roman site (stronghold?) on the hills of present-day Velp (or Arnhem). However, a hoard is not a cemetery and we cannot simply postulate such a site as in the case of Rhenen (where there is also a hoard in addition to the cemetery!).
207 See Böhme, 1974, maps 9 and 10; Haalebos, in press. For specimens, see above, figs. 89 and 56, respectively.
208 See the map in Bloemers 1983, fig. 8.27 and chapter 6, 164–5 with eleven new findspots for the river area. The same conclusions already in Van Es 1967, 550–2.
209 Chapter 6, 165, note 106: site 7, Bennekom.
Fig. 145 Gold torcs from Rhenen (site 9). Two faceted torcs of the Velp type and part of a third torc (with hinges), decorated with five glass paste stones. (Photo Centraal Museum, Utrecht).
All these aspects, from pottery and metalware to the new burial rites and the relative prosperity can be understood as a process of mutual acculturation between Franks and Gallo-Romans leading to new and original developments, in particular in the regions at the interface of both groups along the imperial frontier. The increasingly important role of Franks as high officials of the Roman state can be seen as a result of this development. It is difficult to identify with certainty a core area which could be considered the nursery of Frankish society. There may have been several nuclei along the Rhine in Lower Germany. The Salii's quite obvious pre-eminence soon afterwards, shown by the fact that their highest elite, the Merovingians, became the Frankish royal house and their law, the Lex Salica, became the Frankish law, could be a result of historical accident. It could be attributed to the administrative and military capacity of such men as Chlogio, Childeric, and, of course, Chlodoweg. But it is not fruitful to speculate on this.

The end of the developments from our point of view came when Chlogio moved south, when the centre of gravity shifted further into northern Gaul. This can be seen as Frankish society hatching out of its nursery, and further decisive developments, whether put into a catalogue of historical facts or treated as a further process of socio-political and economic development, took place elsewhere. The brief but crucial period which has been termed Batavia Salica, perhaps not entirely correctly, had ended by the mid-5th century. And so had, even before then, all real power of Rome.

12.2.3 The River Area in the Merovingian Period

The collapse of the Roman state, a growing power vacuum in Gaul, and Frankish force are of course elements which determined 5th-century developments. It must also be realized that by controlling the area in southern Belgium and northern France the Franks controlled an area of great agricultural potential. When current assumptions are valid, the Roman occupation may have had a disastrous effect on the fertility of the Pleistocene sandy soils, in contrast to the calcareous soils of Britain and the continental limestone/loess area, which are less easily exhausted. The Holocene river area deposits can hardly have been exhausted, but there another factor is very relevant, namely, the position of the area from a supra-regional point of view. The end of Britain as a Roman province in the early-5th century meant that at that time at the latest the strategical importance of the Dutch river area for transports was also terminated. And that must have drastically reduced the fuel that fed the local economy. In this respect, it is perhaps worth mentioning that coins are not an important indication. The earliest 4th-century coins are very scarce indeed in the river area, but bronze coins were issued by the state in order to retrieve gold, not to facilitate economic exchange. The very unequal distribution of bronze coinage during the entire Roman period is a consequence of this. The cessation of Roman control implied the cessation of official payments in gold and thus of the necessity to provide bronze. The fact that so few later bronze coins are found and that no replacements were issued in the form of imitations, confirms the limited function here as elsewhere. The same phenomenon may, in fact, explain the relatively huge number of coins from the House of Theodosius found on some sites (fig. 142). That so many are found must be because they were the dominant issues in the pool of circulation at the time when small bronze change became obsolete.

Although unrelated to the diminished importance of the river area as a trade corridor, the breakdown of the late-Roman monetary cycle meant the termination of yet another, although more general, economic stimulus. Relevant for the river area are also the changes in the fluvial system which must have started in the 4th century and were increasingly important in the 5th century, when many formerly navigable branches began to silt-up. With an independent Frankish kingdom in the Rhineland around Köln, Alamanni and other groups further upstream, and the departure of many people and most of the elite with their retinues to the south, the Dutch river area in the late-5th and 6th centuries must have become a backwater in all but the literal sense. The maximum extension of river forests in this period (fig. 137) confirms this picture, and so does archaeology; not so much by positive indications but rather by the lack of them. The dating of pottery, let alone that of the sherds found at the surface of settlement sites, is not very precise. As outlined in paragraph 8.6, there is continuity of occupation for the region as a whole and for some sites in particular, but such continuity is certain only in a few instances. As far as the evidence goes, the...
indications are that the 106 ‘Merovingian’ settlements are the result of population growth into the 7th and early-8th centuries, but from what minimum level it is impossible to estimate.

The archaeologically visible economic activity at least is datable to a relatively late phase. Gold coins may have been minted in Nijmegen in the late-6th century and the production of wheel-turned pottery at site 420 (Ubbergen) can be dated to the late-7th and early-8th centuries. That some people were still able to hide (or occasionally perhaps lose) gold coins throughout the 5th and 6th centuries\textsuperscript{215} is an illustration of the continued relations with the south, but even there the minimum is reached in the late-5th – early-6th centuries.

This is most likely to be the phase with the lowest population density and a largely autarkic economy. Not surprisingly, human interference in the landscape was down to the Bronze Age level (fig. 137), and economic life may not have differed widely in the two periods, although we are not at liberty to extend this to social life as well. That the Scandinavian raiders of the early-6th century were not interested in the river area but at best only in the region from Nijmegen to the east\textsuperscript{216} thus agrees with expectations.

What could be more surprising is the apparent continuity of settlements, which is striking compared to evidence from other regions further upstream.\textsuperscript{217} A different location of settlements from that in the Roman Period, which is the rule elsewhere, is the exception in the river area. Nor is there an overall relocation of settlements after AD 350, when the major change of population must have taken place. The explanation for this abnormal situation is, however, given by the above conclusion that in, say, AD 500, a minimum population level was reached and only relatively few sites were inhabited. This implies that the possible continuity on so many sites visible in fig. 23 (chapter 4) has quite correctly not been interpreted as such and that the interruptions in the bars indicating the chronology should in most cases indeed be taken as real discontinuity.

That so many late-Iron Age and Roman Period settlements were apparently reoccupied after one to four centuries is in fact logical. Whereas in the Rhineland and the Pleistocene sand areas a clear preference of new Frankish settlers for valley-floors and streams can be observed, this was already and necessarily true for the river area in Roman times and earlier. Site location here is prescribed by geological and hydrological conditions and in every period settlers chose the best-located places which were thus necessarily the same in almost all instances. The only significant change in the habitation of the river area with regard to site location took place after World War II and most strongly in the past two decades, when it extended into the flood-basins. But such a flagrant negation of natural conditions was never possible before.

\textsuperscript{215} See paragraph 8.6.3, 328-9.

\textsuperscript{216} See above, 440-1 and notes 87-9.

\textsuperscript{217} A recent overview by Müller-Wille/Oldenstein 1981, esp. 280 f.
SAMENVATTING (DUTCH SUMMARY)

Het verschijnen van dit werk betekent de afronding van een studie waarbij voor de derde keer binnen 150 jaar de inheemse en Romeinse bewoning in het rivierengebied aan een samenhangend onderzoek onderworpen is. De eerste keer was al in 1838–9, toen dominee O.G. Helderling zijn Wandelingen ter opsporing van Bataafsche en Romeinsche oudheden, legenden, enz. publiceerde: in feite een van de eerste min of meer systematische archeologische veldverkenningen in Nederland. Ruim honderd jaar later volgde een serie artikelen waarin P.J.R. Modderman, naar aanleiding van de al tijdens de oorlogsgjaren van ’40–’45 begonnen bodemkartering, een overzicht gaf van de vroegere bewoning in relatie tot het landschap. Deze studies, en al wat er in deze en in de vorige eeuw in met name het oosten van het Nederlandse rivierengebied aan opgravingen werd verricht, hebben aangetoond dat de streek in de Romeinse tijd niet alleen dicht bewoond was maar ook dat de sporen van deze bewoning in het algemeen goed bewaard zijn gebleven. Ze bleken bovendien relatief eenvoudig op te sporen. Deze wetenschap vormde in feite de reden voor het nu afgeronde onderzoek, al was de aanleiding een andere. Die werd gevormd door de sinds 1951 vrijwel ononderbroken voortgaande ROB opgravingen in Nijmegen. In de zeventiger jaren werden deze breder van opzet en verschoof het accent van een vooral op Romeinse militaire forten en kampe-menten gerichte belangstelling naar een onderzoekstrategie waarin juist de nadruk kwam te liggen op de omringende, civiele bewoning en de veranderingen daarin in de loop van de tijd. Dit deed de noodzaak gevoelen om de gegevens uit Nijmegen, in de Romeinse tijd het militair en bestuurlijke centrum van de civitas, het departement of stamgebied van de Bataven, te plaatsen tegen de achtergrond van dat gebied. Uiteraard was dit mede het resultaat van nieuwe ontwikkelingen in het vak archeologie sinds het eind van de zestiger jaren die, onder meer via regionaal onderzoek, leidden tot pogingen om een compleet beeld van het verleden te krijgen en met archeologische middelen uitspraken te doen over, bijvoorbeeld, sociale en economische patronen en structuren. In dit verband is het vermeldenswaard dat de huidige studie en de beide eerdere heel duidelijk de ontwikkeling van de archeologie als wetenschap illustreer. Heldring is een typische, zij het zeker vooruitstrevende representant van de 19e-eeuwse oudheidkunde. Het werk van Modderman is een wetenschappelijk onderzoek dat past in de typisch Nederlandse traditie waarvoor tussen de beide wereldoorlogen door Van Giffen de basis werd gelegd. Het is een gedegen studie van vindplaatsen en hun verspreiding, het aanwezige vondstmateriaal en vooral ook de relatie tussen bewoning en landschap, toentertijd een belangrijk nieuw onderzoeksaspekt en uiteraard nog steeds van essentieel belang. De nu gereedgekomen studie poogt daaraan het aspect van de relatie met het ‘sociale’ landschap toe te voegen, en dat is tevens de reden waarom het oostelijk rivierengebied in de Romeinse tijd een in de Nederlandse verhoudingen zeer geschikt onderzoeksterrein vormt. Om de eenvoudigste uitspraken te kunnen doen over maatschappelijke structuren en processen zoals de Romanisering van de inheemse bevolking, zijn al zeer veel en zeer verschillende gegevens nodig. Het vele al gedane onderzoek in de streek rond Nijmegen maakt deze dus tot een relatief aantrekkelijk onderzoeksgebied vanuit een meer theoretisch perspectief. Relatief, want uiteraard blijft er ook hier veel te wensen over en zal er in de toekomst nog heel wat gegraven en onderzocht moeten worden. Een heel ander voordeel bij een archeologisch onderzoek dat poogt de materiële gegevens ook te gebruiken voor conclusies omtrent immateriële zaken is de aanwezigheid van althans enige historische informatie. Daarin zijn immers, met de nodige voorzichtigheid, heel wat aanwijzingen te vinden die in combinatie met inzichten uit de sociologie en antropologie veel extra aanknopingspunten leveren. Bij al dit soort overwegingen blijft natuurlijk onverlet dat de basis gevormd moet worden door zo veel mogelijk relevante archeologische en andere gegevens; ‘feiten’, zo men wil. Deze gegevens en hun directe interpretatie in de zin van chronologische positie, functie, bodemkundige betekenis en dergelijke zijn het onderwerp van deel 1 (hoofdstuk 1-6). Hoofdstuk 2 is gewijd aan een aantal historische gegevens in verband met de periodizering: de ondervordering van het totale bestudeerde tijdstraject in een aantal analytisch relevante fasen. Naast de Vroeg-(12 v.Chr. - ± 50 na Chr.), Midden- (± 50 - 270 na Chr.) en Laat-Romeinse tijd (± 270 - 425 na Chr.) zijn dat ook de Late IJzertijd vanaf ± 250 v.Chr., en de Merovingische periode tot ± 750 na Chr. Immers, wanneer het doel is meer te begrijpen van de toestanden en ontwikkelingen gedurende de Romeinse tijd dan is het noodzakelijk ook enig begrip te hebben van de situatie voor de komst van de eerste legioenen en te weten wat het effect was van het weer los raken uit het verband van de Romeinse staat.
Hoofdstuk 3 geeft vervolgens een uitvoerige studie van het landschap en de reconstructie daarvan in de Romeinse tijd. De chronologie van de nederzettingen en andere vindplaatsen, en een aantal gegevens over elke plek, zijn ondergebracht in de hoofdstukken 4 en 5. Hoofdstuk 6, tenslotte, is gewijd aan een soms meer soms minder gedetailleerde bespreking van een aantal vondstcategorieën die op de in totaal 549 behandelde sites (plaatsen van menselijke activiteit) zijn aangetroffen. Hoofdstuk 7 vormt de verbinding van deel 2 met de eerder hoofdstukken, en geeft tevens een nadere uitwerking van de reconstructie van het landschap in de Romeinse tijd, de daarbij behorende flora en fauna en de exploitatiemogelijkheden voor de mens. Hoofdstukken 4-6 worden nader uitgewerkt in 8, dat is gewijd aan de typologie van de sites. Daarin wordt, op basis van opgravingen en vondsten in en rond het rivierengebied en vergelijking met gegevens van elders, weergegeven welke soorten van nederzettingen, grafvelden, en andere plaatsen van menselijke activiteit onderscheiden kunnen worden. Bovendien wordt nagegaan wat de onderlinge verschillen kunnen betekenen, met name tussen de nederzettingen, en ten dele ook welke ontwikkeling daarin valt waar te nemen van de Late Ijzertijd tot de Vroege Middeleeuwen. Deze materie wordt, tenslotte, verder uitgewerkt in de concluderende hoofdstukken 10-12. Het tussenliggende hoofdstuk 9 is de publicatie van een kleine opgraving in Arnhem-Meinerswijk die in 1979 werd uitgevoerd om twee hoofdredenen: het onderzoeken van de aard van deze site, die een Romeins castellum bleek te zijn, en daarmee het testen van de procedure om op grond van toevalsvondsten het karakter van de bewoning te bepalen. In dit geval leverde dat een succes op en zoals besproken in paragraaf 8.4.3 bestaat er een gerede kans dat dit in de toekomst ook voor andere nederzettingsvormen kan worden toegepast. De laatste drie hoofdstukken vormen, zoals gezegd, het voorlopige sluitstuk van het onderzoek. Voorlopig, omdat er veel in staat wat in de komende jaren zal moeten worden onderzocht en getoetst aan nieuwe gegevens. Dat kan gelukkig ook, omdat het project 'Oostelijk Rivierengebied en Maasdal' van de ROB juist ten doel heeft door opgravingen en veldkarteringen niet alleen Nijmegen maar ook haar achterland in de Romeinse tijd beter te leren kennen en begrijpen. De hier gepresenteerde conclusies en hypothesen kunnen daartoe een zinvolle leidraad zijn, om gericht te kunnen werken, en ze worden daarbij zelf verder onderbouwd en natuurlijk ook veranderd. Hoewel ze vóór hoofdstuk 10 maar af en toe ter sprake komen is het onderzoek in feite gericht op Romeinen en Bataven en, in meer algemene termen, op allochtonen en autochtonen en hun interactie in een ruimtelijk en chronologisch beperkt gebied. In hoofdstuk 10 wordt daarvoor de basis gelegd en worden de ontwikkelingen onderzocht in de Late IJzertijd tot het moment, in 12 v.Chr., waarop de eerste grote Romeinse troepenmacht daadwerkelijk in het rivierengebied arriveert. De bevolking komt daarbij naar voren als een vrij egalitar georganiseerde tribale samenleving met een sociaal-economische structuur die, blijkens een combinatie van archeologische, historische, antropologische en ecologische gegevens, was gecenterd rond een waarschijnlijk nogal extensieve vorm van veeteelt, overigens zonder dat er aanwijzingen zijn voor een nomadische levenswijze. Caesar heeft op deze groepen nooit goed wat kunnen krijgen, als dat al de bedoeling was, maar onder Octavianus, de latere keizer Augustus, is men begonnen ook dit perifere gebied enigszins te controleren. Dit gebeurde door het sturen van met Rome geallieerde stammen. Voor het rivierengebied was dat een, waarschijnlijk uit Noord-Hessen afkomstige, groep Chatten die zich Bataven noemden of zo gingen heten toen ze in het gebied waren. Dit weten we overigens alleen uit de historische overlevering, want de archeologische aanwijzingen zijn nogal vaag. Ze betreffen echter wel een strategisch belangrijk areaal midden in het rivierengebied waar Maas en Waal dicht bijeens komen en de Brabantse riviertjes uitmonden in de Maas. Na een theoretische beschouwing over imperialism en kolonialisme en de specifiek Romeinse vorm daarvan komt vervolgens, in hoofdstuk 11, de eerste fase van werkelijke interactie tussen Bataven en Romeinen aan de orde. Vanaf 12 v.Chr. werden Bataven, vanwege hun krijgshaftige karakter, steeds ingeschat als irreguliere hulp troepen (tumultuarii) bij de verschillende campagnes om het vrije Germanië tot aan de Elbe te veroveren. Ook leverden ze, misschien al vanaf 38 v.Chr., tot aan de dood van Nero in 68 na Chr. het overgrote deel van de persoonlijke lifwacht van Romeinse keizers. De onregelmatige krijgsdienst paste overigens goed bij hun sociaal-economische structuur die er, met de extensieve veehouderij, traditioneel op was ingesteld dat grote groepen mannen tijdens het hoogtepunt van het akkerbouwseizoen afwezig konden zijn. Deze situatie, hoewel ze wederzijds niet echt onbevredigend zal zijn geweest, leidde vermoedelijk niet tot werkelijk diepgaande contacten en veranderingen, althans

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weest aan wat, op archeologische gronden, een zich
delta van belangrijke rivieren, vooral te danken zijn gele-

gkeersgeografisch gunstige ligging van het gebied in de

den lieten besturen. Vaak door koopli-

civitas tonen bijvoorbeeld aan, en dat is een grote uit-

niet indien de penetratie van de Romeinse materiële cul-

tuur op inheemse sites daarvoor als maatstaf genomen

werd. De onderlinge verhouding veranderde echter toen,
donder keizer Claudius, de veroveringen meer op

Britannia werden gericht. Bataafse *tumultuarii* werden

toen tot reguliere hulptroepen (*auxilia*) ongevormd en
duizenden mannen, op een totale bevolking van tegen de

40.000, waren plotsklaps niet tijdelijk maar permanent

huis. Daar kwam nog bij dat de Rijn als grens werd

ingericht, met een keten van forten, en dat een Romeinse

bestuursstructuur steeds verder moet zijn doorgevoerd,

met een vrij kunstmatige hoofdstad in Nijmegen. De

daarop volgende ontwikkeling van het traditionele in-

heemse samenlevingspatroon bleek een vruchtbare voed-

ingbodem voor een inheemse revolte, de Bataafse op-

stand van 69-70.

De onderdrukking daarvan leidde tot een verdere uit-

bouw van de Romeinse militaire en administratieve in-

frastructuur in het gebied onder de Flavische keizers:

een groot investeringsprogramma dat in het laatste kwart

van de 1e eeuw zo’n 1% van het totale inkomen van het

hele Romeinse rijk opgeslokt kan hebben. Die
tijd, nu met alle eigen troepen ver weg en onder Italiaan-

se officieren, een legioen (het *roe*) in Nijmegen en

vreemde hulptroepen, met name Thraciërs, in de grens-

forten, kan voor de inheemse bevolking niet bepaald plau-

zierig zijn geweest. Archeologisch zijn er dan ook geen

aanwijzingen dat ze profiteerde van alle investeringen.

Dat veranderde pas in de 2e eeuw, toen de ‘Romeinse’

input weliswaar verminderde maar er enerzijds een ook

archeologisch aantoonbare opwaartse spiraal moet zijn

ontstaan en anderzijds de nodige sociale veranderingen.

Er kwam een eind aan een speciale en naar alle waar-

schijnlijkheid voor extensieve veeteelt typerende neder-

zettingsvorm. Ook in het rivierengebied begon in die

zelfde tijd een nieuw soort nederzetting te floreren: de

villa, ongetwijfeld in samenhang met de na 70 nieuw ge-
bouwde hoofdstad, Ulpia Noviomagus Batavorum. Er

zijn echter goede aanwijzingen dat de hele militair/admin-

istratieve structuur alleen dankzij externe impulsen kon

functioneren en Nijmegen is noch als stad noch als

regionaal centrum ooit echt volwaardig geworden.

Inscripties tonen bijvoorbeeld aan, en dat is een grote uit-

zondering, dat de Bataven hun civitas vaak door koopli-
den lieten besturen.

De relatie tot rivierengebied moet dan ook, behalve aan de ver-

keersgeografisch gunstige ligging van het gebied in de
daelt van belangrijke rivieren, vooral te danken zijn ge-

weest aan wat, op archeologische gronden, een zich
met nauwe relaties met stammen ver in het noorden tot in het gebied tussen Weser en Eems. Vanwege de noodzakelijke graantransporten van Britannia naar het Rijnland was controle van het rivierengebied ook van groot belang. Er zijn dan ook allerlei aanwijzingen dat zich, misschien gestimuleerd door deze transport-route, een bloeiende samenleving ontwikkelde met een vermenging van Germaanse en Romeinse elementen.

De definitieve ineenstorting van het west-Romeinse rijk, die kort na het begin van de 5e eeuw aanving, veranderde daarin niets. De Salii breidden hun invloed juist verder zuidwaarts uit en kwamen later zelfs bovendrijven als de belangrijkste groep in het Merovingische koninkrijk. Misschien ten dele daardoor, en zeker door het verdwijnen van haar verkeersgeografisch gunstige positie in de nieuwe situatie, werd het rivierengebied in de 5e eeuw een onbelangrijk achterland. Er zijn verschillende soorten gegevens waaruit blijkt dat de streek nog maar dun bevolkt was, en de natuurlijke vegetatie kon zich herstellen tot een niveau dat sinds de Bronstijd niet meer bereikt was. Pas vanaf de late 6e eeuw ging de ontwikkeling weer opwaarts en werd het rivierengebied opnieuw belangrijk en onderging een integratieproces in het Merovingische rijk. Daarmee begon in feite een nieuwe cyclus van ontwikkelingen die, bijvoorbeeld, leidde tot de opkomst van het Karolingische Dorestad als internationaal handelscentrum; deze episode valt evenwel buiten het perspectief van het hier samengevatte onderzoek.

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