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Chapter 2 The Principle of Symmetry

1. Symmetries and asymmetries in approaches to past science

The principle of symmetry became manifest in science studies with the advent of the strong programme in the Sociology of Scientific Knowledge (henceforth SSK) in the 1970s. Yet upon reflection we can see that every approach to past science relies on the assumption of some form of symmetry. To see why, we must first make a distinction between topics and resources. Topics are the things that stand in need of explanation. In this thesis the central topic is theory choice, or from a diachronic perspective, theory change. Supposing that, in the majority of cases, alternative explanations for a particular natural phenomenon or a particular research problem were in competition, the question historiography of science needs to answer is what made the balance tip in favour of one of these explanations, and consequently, how to evaluate the choices that were made?

With the topic stable, approaches to past science differ in terms of the resources they recognize to carry out the explanation. Any mode of explaining past science has variant and invariant aspects. What remains invariant is the symmetrical aspect of the explanation. Thinking about symmetry then is a way to think about the topic-resource interface in historical explanation. Decisions made with respect to these categories determine, so to speak, the working space of the historian of science, both in terms of research agenda and in terms of ways of approaching the main research questions.

The term symmetry is derived from the Greek word 'symmetros' which has the literal meaning 'to measure together', i.e. symmetry is some form of common measurement. It can be observed in three ways. The first is mirror symmetry, that is, reflection via an (imaginary) line. The second is rotational symmetry, that is, with respect to a fixed point of perspective. And the third is invariance under transformation: a system retains certain characteristics after transformation. In physics, symmetry is mostly defined as invariance under transformation. A clear example is the law of conservation of energy. No matter how many transformations occur in a closed system, the total amount of energy remains constant.90

90 For more background on the concept see Hon and Goldstein (2008) and Du Sautoy (2008).
Many approaches to the history of science work with a notion of symmetry as invariance under transformation. In Whig history of science, for example, all past science is considered as working up to the present. No matter how many transformations occurred in the past, the present is the stable element in Whiggish explanations of past science. Another form of invariance is the assumption that the structure of the world is stable. The history of theory replacement can then be interpreted as converging upon this structure. Nature itself rejects mistaken hypotheses, for example in response to experiments, and this prompts the search for alternative explanations. One can explain past science this way only on the assumption that the structure of the world is invariant.

Another important kind of invariance that approaches to past science rely on is given by the assumption of transcendent norms of rationality. In such approaches the proposed norms of rationality are context independent and hence are supposed to hold in every time and place. Progress in science is ultimately ensured not by historical actors, technological advancements or theoretical breakthroughs, but by norms of rationality that govern these.

When choices made in the past do not conform to the selected model of rationality one has to explain these choices with reference to specific factors operative in local contexts, often referred to in short as social factors. Persistent belief in what we now see as erroneous theories can for example be explained with reference to religious or philosophical doctrine, to authority relations, to cultural values or to social interests. The acceptance of one type of belief, correct belief, or at least belief in the right direction, is explained by rational factors and another type of belief, incorrect belief or belief hampering scientific development, is explained by social factors. These explanations of past science are said to be asymmetrical because not all beliefs are accounted for with reference to the same type of factors. Note however that there is a symmetrical aspect in the explanation, namely the assumption that the norms of rationality are historically invariant.

It is important to understand this way of explaining past science because it is against this form of asymmetrical explanation that sociologists and historians in the 1970s, and beyond, have reacted. There are approaches to past science that, at first appearance, come close to a fully symmetrical explanation of past science, because they allow for a considerable degree of contextualism. On closer inspection we can however see that these approaches, in one way or another, still rely on a transcendent norm of rationality. A first example is provided by Merton’s sociology of institutions. Merton proposed to interpret
science as an institution, functioning like other institutions in society. We can therefore apply the same sociological means to study the functioning of the institution of science, as we use in the study of the functioning of other institutions. However, in Merton’s theory social factors define only the context in which science is practiced, but not the content of science. For example, he argued that the Reformation created a climate that was very suitable for the development of modern science. Merton claimed that, had the whole of Europe remained Catholic, mankind would have arrived at the same scientific breakthroughs that have occurred in actual history, only at a much slower pace.

That this would be the case is due to a number of core values that define the character of science according to Merton, namely commun(al)ism, universalism, disinterestedness and organized scepticism. Living up to these values ensures the rationality of the scientific process. The pace of development is due to contextual factors, but these factors ultimately do not touch the content of scientific theories. Note that this approach may also imply an evaluative assessment of historical contexts, as some of these contexts are more favourable to scientific development than others.

Next to Merton’s sociology of knowledge we can also have a look at Lakatos’ methodology of scientific research programmes. In this methodology a research programme is interpreted as a collection of theories. A research programme can be progressive in two ways: theoretically and empirically. Theoretical progress is achieved by better predictions; empirical progress is given by the confirmation of novel predictions, which is a sign of increase in empirical adequacy. It is irrational not to choose for successor theories within the programme that promise theoretical and/or empirical progress. Lakatos’ methodology allows for temporary regressions or stagnations of the research programme, but these cannot last long. It is rational to pursue a progressive research programme, and within research programmes it is rational to choose progressive theories. When choices made in the past conform to these norms of rationality they belong to internal history, when not they must be explained with reference to social factors and belong to external history. A rational reconstruction of past science, i.e. its internal history, may deviate from the actual course of history and replace irrational choices by rational ones. We can see that in this approach assessments of rational behaviour are dependent on the chosen programme and on the particular theories in question, i.e. they are dependent on context, but evaluations of progress and regress through assessments of predictive accuracy and empirical adequacy are still the same for all research programmes, and hence also context-independent.
Finally, even Kuhn’s model of science as paradigm alternation, rightfully seen as more contextualist than Lakatos’, works with the idea of an internal realm and a surrounding context. What we call science can be called so only because it exhibits patterns of paradigm alternation via periods of normal science and revolutionary science. With this model Kuhn was able to capture two forms of criticism, institutionalized (during periods of normal science) and fundamental (during periods of revolutionary science). A flavour of meta-methodology is therefore kept alive in Kuhn’s model, as scientific development has to conform to this structure.

It is true that the constituents of paradigms are all taken from a particular historical context. Even what counts as a fundamental critique is informed by contextual parameters. But, already in Structure, Kuhn argued that a scientific community accepts paradigm shifts only when they promise an increase in problem-solving capacity. The fact that fundamental critique is directed at the previous paradigm ensures that the alternating paradigms are not wholly unrelated. Later in his career, Kuhn argued that inter-paradigmatic comparison is possible on a number of epistemic virtues such as simplicity, consistency and fruitfulness. This is not possible in symmetrical study of past science because both the preference for, and the very understanding of, these epistemic virtues are dependent on historical context. Hence they cannot play a fundamental role in the explanation of theory acceptance and rejection in science. It is therefore incorrect to interpret Kuhn’s philosophy of science as another variant of the social study of science, as is often done. The difference will stand out more clearly in comparison to the principle of symmetry of the strong programme.

2. The strong programme

In the previous section I argued that all approaches to past science rely on some form of symmetry. Yet only with SSK symmetry became a methodological principle. The key text is Bloor (1976), which starts with the observation that in

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91 This view is not the same as Laudan’s definition of scientific progress in terms of problem-solving capacity. For Kuhn, with every paradigm shift some problems become irrelevant and hence some problem-solving capacity is lost in the process. Laudan does not think along these lines, see below and chapters 4 and 6.

92 See also the debate between Laudan and Bloor on this issue below. Thinking about rationality in terms of the pursuit of epistemic virtues is essentially the right way of looking at it. But this idea can be interpreted in many ways. In chapter 7 I develop my own view on this matter.

93 Kuhn distanced himself from the symmetrical approaches to science coming ‘after’ him. One of the most constructive examples is Kuhn (1991).
approaches to science based on demarcation between internal and external factors it is the natural thing to arrive at correct theories. ‘Right’ actions then appear to carry their own motives, irrespective of historical context. Following Merton, Bloor argued that correct theories require as much explanation as incorrect ones.

“The sociology of knowledge came into being with the signal hypothesis that even truths were to be held socially accountable, were to be related to the historical society in which they emerged.”

Merton however did not draw the radical consequence from this, which is to move beyond demarcation between internal and external factors. His sociology of knowledge is a ‘weak’ programme. It does grant an important role to social factors in science but it does not turn social factors into the overriding determining type of factors *tout court*. When this happens, we speak of the strong programme in the sociology of scientific knowledge. It is with the postulation of the principle of symmetry that everything becomes ‘sociology’, which is why this principle represents such an important watershed in the study of past science.

The strong programme rests on four principles: causality, impartiality, reflexivity and symmetry.\(^95\) Scholars should focus on the causal processes that engender belief formation and theory acceptance in a community. In carrying out this naturalist programme, scholars should be impartial to the content of scientific theories. The whole approach must further be reflexively applied to SSK itself. As SSK makes the study of science a branch of sociology it is part of science. The reflexivity principle ensures that the principles of the strong programme are applied to itself and hence confirms its scientific status.

What gives the programme its most significant bite is the symmetry principle. Bloor formulated the principle as follows: “the same type of cause would explain say true and false belief.”\(^96\) This means that the same type of causal factors must

\(^{94}\) Merton (1973) p. 11.
\(^{95}\) Cf. Bloor (1976). In what follows we will mainly focus on the strong programme and the so-called Edinburgh school. Other members of this school were Barnes, Pickering and Shapin. The Bath school (Collins, Evans, Pinch) with its empirical programme of relativism (EPR) emerged around the same time, see Collins (1981a). Many other scholars such as Caneva, Wise, Kusch, etc., can also be associated with SSK. There are differences between approaches but these are not differences in principle, only in emphasis. EPR is for example more micro oriented and has a clear focus on the theme of expertise while the strong programme is more historically oriented, hence best fitting the present focus on historiography of science.

\(^{96}\) Bloor (1976) p. 5.
be used to explain all rejection and acceptance of claims to knowledge. Since social factors are taken to be decisive in all cases of theory choice, there is no special place for rationality anymore in the explication of past science. As Barnes put it:

“Needless to say all forms of relativism are anathema to rationalists, who insist that there is a crucial divide between rationally and irrationally held beliefs and that the incidence of the two different kind of beliefs must be explained in radically different ways.”97

If this difference disappears the qualification of what counts as rational has to be explained as well. This is what generally happens when a symmetry is posited: something from the set of resources shifts to the set of topics.98

With the assumption of the symmetry principle the natural question is: how do we get at outcomes in science at all? Full symmetry with respect to all properties of a system would amount to a standstill. Only the breaking of symmetry yields outcomes. The attractiveness of an approach to past science that assumes a lot of symmetry is that a priori assumptions about science are decreased to a minimum. Yet the more symmetry, the higher the demand for a satisfactory breaking mechanism becomes.

In SSK the breaking of symmetry is explained in an ingenious way. SSK insists on symmetry only on type level. On occurrence level, social factors manifest themselves in many different ways, for example, in relations of trust and authority, through disciplinary training or in processes of inclusion and exclusion. On macro-level we can think about political systems, the economy, societal structures, the role of cultural values, etc.99 In every situation of theory choice detailed analysis is required of all relevant factors, in terms of their causal efficacy on theory choice, and in terms of the interaction between them. On the level of actual occurrence there will thus be a different story to tell from case to case. The task of the historian of science is to investigate such particulars.

It is perhaps good to avoid a number of misinterpretations of the SSK approach to the study of science. First, it is not another form of externalism. As the internal-external distinction is given up, there is nothing to be external to anymore.100 Secondly, it is not the case that nature and the activities of

98 For other illustrations of this effect see the generalized symmetry principles discussed below.
99 Bloor (1981) p.203 suggests that where macro-social factors are not present the micro factors invariably take over.
100 Shapin (1992) addresses the community of historians of science on the consequences of the strong programme for their profession and makes this point very clear.
individual scientists play no role anymore in the course of science. The physical world does provide the phenomena that scientists study and individual scientists come up with theories explaining these phenomena. Although the phenomena set some limits on the possibilities for interpretation, a wide variety of interpretations is still possible.

However, individual interpretations do not yet amount to knowledge. What the strong programme is offering is a different way of looking at the justification of belief, and hence a different way of defining what counts as knowledge. If knowledge is defined traditionally, as justified true belief, then nature could play a role as truth provider and the justification for having a belief could be attached to individuals. In the strong programme however, knowledge is equated with *authorized* belief. An individual scientist can do research, come up with experimental results and theorize about these. But when it comes to sustaining such knowledge claims he or she has to enter the social sphere and engage in a debate with other people. Therefore, when it comes to the question of acceptance or rejection of theories, social factors outweigh all other factors.

There is thus a hierarchy of factors in science and the social ones are always dominant when it comes to theory choice.

Thirdly, SSK is not anti-science. It just offers a different view on what science is and how it functions, with the aim of deepening our understanding of knowledge formation processes. As it rests heavily on sociology, a scientific discipline, it aspires to be scientific itself. When it comes to the structure of nature Bloor has even claimed to be a realist. But he combines this ontological realism with epistemological perspectivism. Many perspectives can be projected on nature as “nature will always have to be filtered, simplified, selectively sampled and cleverly interpreted to bring it within our grasp.”

It is this perspectivism that leads to epistemological relativism because the strong programme does not accept independent criteria to assess whether one perspective is better than another. As Bloor has put it: “all cultures are equally near to nature.” Hence, all perspectives developed on nature should be understood in their own right. It is from this epistemological relativism that a

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101 “We take for granted that trees and rocks, as well as electrons and bacilli, have long been stable items amongst the furniture of the universe. They are just there providing a stable backdrop to the more volatile happenings on the human stage, where ideas change and theories come and go.” Bloor (1999) p.86. There is a fundamental difference with posthumanism in this respect, see section 5 below.


direct line to the localist and non-evaluative character of present-day historiography of science can be drawn.

As said in chapter 1, support for the strong programme comes from the argument from underdetermination. According to SSK, in scientific controversies there can be no conflict over empirical evidence. Either conflicting parties agree on the stack of empirical evidence or when not, this is because they recognize different standards of measurement, different instrument calibrations or different standards of interpretation of experimental results. Hence it is a conflict on standards and not on the evidence. As scientific controversies are eventually closed, theory choice must be settled by means other than empirical evidence. To solve this problem of underdetermination SSK draws the conclusion that we are forced to look at social factors in order to find out how controversies were settled. If this holds for all choices made in the past, the whole endeavour we call science is of a deeply social kind.

Other options to solve the problem of underdetermination are rejected on the grounds of circularity. We cannot use criteria of rationality, progress or success to tell the winners from the losers because these evaluations got attached to the winners only after they became victorious. It is circular to use resulting outcomes in the explanation of a process leading up to these results. Moreover, if it were possible to decide who is right and who is wrong by referring to standards of rationality or success, then there would not be a controversy in the first place. Pickering has put the point as follows:

“If one is interested in how a scientific world-view is constructed, reference to its finished form is circularly self-defeating; the explanation of a genuine decision cannot be found in a statement of what that decision was.”

Collins pointed out that the circularity argument is a logical consequence of accepting the principle of symmetry:

“The tenet of symmetry tells us something about the content of our explanations. The same types of explanation will be applied to all ‘qualities’ of scientific endeavour. Explanations of the true will be like explanations of the false, and similarly for the rational and irrational, and the successful and unsuccessful and, we may suppose, for the

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104 Clear examples are Pickering (1984) and Collins (2004). Note that if two parties agreed upon a difference in evidential support for their theories there would be no controversy worth mentioning.


apparently progressive and the degenerative. It follows that there are things that cannot form part of an explanation belonging to the radical programme. Knowledge cannot be explained by reference to what is true, rational, successful or progressive. If such categories were allowed into explanations then the explanation of, say true, knowledge would not be of the same type as the explanation of false knowledge.\textsuperscript{107}

Thus the principle of symmetry ensures both neutrality and the avoidance of circularity of explanation. The only way out of the underdetermination problem is to turn to social factors to account for theory choice.

What the objections against circularity basically amount to is saying that notions such as truth, rationality, objectivity and measure of success have no absolute character. The efficacy of these notions depends on historical context and must be socially accounted for. We have seen that traditional notions of right and wrong and the definition of knowledge must change accordingly. Scientific knowledge is never absolute. Knowledge claims must be judged according to the function they have in specific social circumstances and the way they attach to specific networks of belief. Perhaps a scientific theory can perform similar functions in another context but this is unlikely because there are too many specific factors in play. Therefore Bloor concludes:

“All knowledge is relative to the local situation of the thinkers who produce it: the ideas and conjectures that they are capable of producing; the problems that bother them; the interplay of assumption and criticism in their milieu; their purposes and aims; the experiences they have and the standards and meanings they apply.”\textsuperscript{108}

The turn to sociology involves a serious challenge to philosophy as the primary discipline to study science. SSK is deeply embedded in a tradition of social study of knowledge that started with Durkheim, \textit{The Elementary Forms of Religious Life} (1912). For Durkheim classifications of the world flow from the human need for organization. The evolutionary argument is that we need social organization because otherwise our species cannot survive. Science then must be seen as part of the social organizations man has created for this purpose. According to Durkheim, both philosophy and later science were born out of the most elementary form of human organization, which in his view is religion, which he interpreted as follows:

“Before all religion is a system of ideas with which individuals represent to themselves the society of which they are members, and the obscure but intimate relations they have with it.”\textsuperscript{109}

\textsuperscript{107} Collins (1981a) p. 217.
\textsuperscript{108} Bloor (1976) p.142.
\textsuperscript{109} Durkheim (1912) p.8.
Society for Durkheim was something *sui generis*, that is, it has characteristics that cannot be reduced to other things. Because of this, the notion of society or ‘the social’ can have explanatory force with respect to other things.

Bloor extrapolated these views towards the study of the formation of scientific knowledge and the structures in which claims to knowledge come about. Following Durkheim, Bloor saw society as the primary locus of human existence. It is the well from which religion, philosophy and science have sprung. ‘The social’ presents itself as a middle ground between full (or individual) subjectivism on the one hand and absolutism on the other. Moreover, this focus helps to sidestep a number of persistent philosophical problems, because science can be studied via the discipline of sociology.

Still it would be a mistake to think that SSK has fully disconnected the study of science from philosophy. The strong programme in SSK is inspired by the work of various philosophers, of which the later Wittgenstein is the most important. Bloor has emphasized that it is the conventional character of language, in which all social conventions are codified, that makes the “profound involvement of society a pervasive and inescapable feature of knowledge.” The conventional character of linguistic categories was captured by Wittgenstein in the concept of the language game and the idea that the meaning of linguistic expressions becomes manifest in the use of these expressions in particular situations.

People speaking a language can be seen as playing a game according to a set of rules. Which rules to follow is not a given necessity, it is a conventional matter. Such conventions are not dictated by individuals but come about in processes of social negotiation. Next to geographically separated nations, or historically separated localities, these ideas can also be applied to the study of particular societies. Social groups in one society differ because they play different language games. They use different sets of words and settle on different sets of rules. This perspective can be applied to opposing parties in a scientific controversy, when these parties represent different social groups.

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110 Bloor wrote two books on the philosophy of Wittgenstein. See Bloor (1983) and Bloor (1997).
111 Bloor (1981) p.211.
112 This is what happens in the by now classic exemplar of the strong programme applied to history of science: Shapin and Schaffer (1985), in which rationalism and empiricism are depicted as different ‘life forms’, which is another Wittgensteinian notion. I discuss this work in the next chapter in relation to the principle of charity.
According to the ‘meaning is use’ doctrine, words do not have fixed semantic content. Instead the meaning of words can become clear only when the role they play in the language game is specified. This requires a localist study of concepts, which is known as finitism. Finitism holds that no concept has a fixed meaning, i.e. meaning is finite with respect to a particular context. There are always circumstances, causes and potential problems that stand between previous applications of a concept and the next application of it. Thus in every situation, when circumstances shift, meaning is created afresh and this happens through social negotiation. For Bloor this holds equally for the empirical, mathematical and theoretical concepts used in science.

The views on language as a game, and on meaning as use, significantly undermine the logical positivist project of securing certain knowledge, because that project was based on the assumption of a neutral observation language and context-independent translation mechanisms to higher-order linguistic expressions. This does not fit with the conventionalism of Wittgenstein’s views on language. As a consequence it also questions the whole formalist tradition in modern philosophy of science that the logical positivist project sparked off. Interestingly, however, the linguistic turn in philosophy continues to have a deep influence. Even in the arguments of those who distance themselves from formalist approaches to science, the focus on language is still of central importance.

SSK may have claimed authority over philosophy in the explication of science. Yet, in spite of the rhetoric it has never been ‘philosophy out’ completely. As the strong programme is grounded on philosophical insights itself, only specific strands of philosophy are rejected while other philosophical views have come to replace these. I believe that any distinct approach to past science is based on a philosophical view on what science is and how it should be studied. It is therefore important to recognize the deeper philosophical views behind approaches to science, such as SSK, because historians of science, either consciously or subconsciously, inevitably come to operate with them.

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111 Finitism is close in outlook to the work of Quine and Rorty as well. In various places Bloor has subscribed to Quinean holism and to the coherentist view on justification associated with it, for example in Bloor (1981). Finitism bears many similarities to Rorty (1979). According to Rorty the task of epistemology was not to provide the ultimate foundations of knowledge but to study how knowledge operates in specific contexts.

114 See chapter 1, section 6.1.
3. ‘Refutations’ of the strong programme

The different perspective on knowledge SSK had to offer proved counterintuitive to many scholars in science studies. Consequently, it provoked harsh rejections and these led to fierce controversies. The fierceness is not surprising, if we consider that a struggle for authority in the science studies was at stake. Who was in the best position to offer interpretations and critiques of science? The philosopher, the sociologist, the historian or the scientist? This struggle was fought in a high moral tone over values such as fairness, honesty, respect and intellectual rigour.115 Even among symmetrists an ‘impartiality contest’ was fought. The most radical symmetrists presented themselves as heroic democrats and levellers, calling others ‘chicken’ or telling them to ‘go home’.116

In this section I argue that attempts to refute the strong programme have not succeeded. In order to demonstrate this I will have a close look at the Laudan-Bloor debate, the so-called Captives debate, and the ‘Pinnick’ controversy. The attacking strategy, aimed at complete refutation of the strong programme, is very often accompanied by simplifications and misrepresentations of the symmetrist position. This is for example the case in Tosh (2006) and Tosh (2007). Tosh misses the point that SSK offers an altogether different perspective on knowledge and the justification process. He also skips over the fundamental distinction between the principle of symmetry and generalized variants of the symmetry principle (see section 5 below).117 Blindness for subtle, but nonetheless essential, distinctions and nuances, is a consequence of the belligerent strategy.

This strategy should in my view be avoided.118 It is better to walk the distance with the strong programme and then ask whether the programme leads to undesirable restrictions on explaining past science. In section 4 I argue that this is indeed the case. The chapter from then on starts to work towards a treatment of determining factors in past science that is less restrictive, and hence more desirable, because it allows historians of science to investigate a wider variety of historical questions.

116 Some of these debates are collected in Pickering ed. (1992). See also Pels (1996).
117 This is also pointed out in a reply by Kochan (2010).
118 Misrepresentations have also affected the unfruitful science wars in the 1990s, see chapter 1.
An early attempt at refutation of the strong programme can be found in Laudan (1981a). Laudan delivered four main points of critique. First, he attacked the strong programme for relying on a priori assumptions. He saw no empirical proof for the dominant role granted to social factors in determining theory choice. Second, he accused Bloor of making a simple-minded distinction between teleologists on the one hand and (pure) empiricists on the other hand. Since both of these positions are obviously untenable, the strong programme came to appear as the only alternative. With a more fine-grained representation of positions in philosophy of science this would not have been possible. Third, Laudan argued that rationality is not a non-explanatory concept, as Bloor appeared to assume. Finally, Laudan conceded that symmetry works with respect to truth and success, but not with respect to rationality. Bloor had made the mistake to argue only against Lakatos’ project of rational reconstruction, thereby ignoring other interpretations of the rationality concept, such as Laudan’s own. Laudan acknowledges that some degree of relativism is unavoidable. Yet, what Laudan could not accept was the ‘omnibus’ relativism that is a consequence of the application of the principle of symmetry.

Bloor was not impressed by these charges. In his reply he referred to a number of case studies in order to show that the strong programme stood on firm empirical grounds. To the argument that he had performed ‘bad philosophy’ Bloor remained indifferent. For him every philosophy of science granting a special place to the notion of rationality, in one way or another, would have to succumb to the symmetry principle. The classification he had offered was not of primary relevance to this argument.

The problem with the rationality concept is not that it does not explain anything, but it is the self-explanatory character of the concept. According to Bloor, a preference for simplicity of explanation, for example, is context dependent in two ways. First, the very preference for this virtue over others is a contextual matter and second, what actually counts as being ‘simple’ is not invariant across history. For Bloor it is therefore mistaken to believe that

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119 The reply is Bloor (1981).
120 In chapter one we have pointed out the influence of SSK on historiography of science. In 1981 Bloor could refer in this respect to Forman (1971), Farley and Geison (1974), Shapin (1975), Turner (1974), Frankel (1976), Hanvood (1976), MacKenzie (1978), Barnes and Shapin eds., (1979), Wallis ed., (1979). Later important books were Pickering (1984), Shapin and Schaffer (1985), Collins (1985), Biagioli (1993), Shapin (1994) and Shapin (1996). These are historical studies only and it is only a selection. There is much more, including many sociological case studies.
rational standards do not require any further explanation. In his view the operation of rational standards is fully dependent on social context.\textsuperscript{121}

Finally to the argument that symmetry works with respect to truth and success, but not with respect to rationality, properly understood, Bloor replied that the circularity argument applies to any evaluative category. Against the charge that if this is indeed true, it will be impossible to tell anymore which cognitive features define science, Bloor simply replied that this is, in his view, indeed an empirical question!

In all fairness it should be said that Bloor seriously misconstrued Laudan’s position in various places. It is not the case that Laudan wanted to rule out social factors, it is also not the case that Laudan interpreted rationality as a ‘self-propelling phenomenon’, and Laudan is also not a proponent of the history of ideas.\textsuperscript{122} Laudan has in fact defended quite a moderate notion of progress based on the increase of problem-solving effectiveness.\textsuperscript{123} In this theory it is always rational to accept theories that solve the most problems. It is however a contextual matter which problems scientists select to work on, and how they weight solutions to problems, if these pull in different directions of theory choice.

Thus Laudan’s philosophy of science is highly context sensitive but it is true that he does make a distinction between social and rational factors and maintain that if irrational choices occur, they should be explained with reference to social factors only. Notwithstanding the misinterpretations of Laudan’s theory, Bloor could never go along with this. In my view there seems to be no definitive argument that forces a choice between Bloor’s descriptive naturalism and Laudan’s normative naturalism. It is more or less a matter of perspective which position deserves support.

Another example of an attempt at refutation of the strong programme can be found in Pinnick’s review of Shapin and Schaffer (1985), from which ensued a harsh discussion.\textsuperscript{124} Pinnick argued that \textit{Leviathan and the Air-Pump} was a clear example of bad historiography. The debate between Hobbes and Boyle is

\textsuperscript{121} Following Durkheim, Bloor did accept that a form of minimal rationality, required for survival, is present in any human society. But he hastened to add that such a concept is hardly interesting for the explication of theory choice in past science because it is not specified enough. For more discussion on this point see chapter 6 on naturalistic projects in philosophy of science.


\textsuperscript{123} Laudan (1977). Laudan’s approach to science falls under normative naturalism. For a discussion of normative naturalism see chapter 6.

\textsuperscript{124} Pinnick (1998). The debate between them was published in \textit{Social Studies of Science}.\textsuperscript{125}
presented as a central one in the 17th century. For her, no justification for the plausibility of this claim is however offered. According to Pinnick doubts about the experimental method were widespread and shared by everyone. The Hobbes-Boyle controversy is thus at best exemplary for the age. Shapin and Schaffer had also made selective use of source material. They deliberately included material that made Hobbes and Boyle look like complete adversaries and excluded material that made a more nuanced picture possible. According to Pinnick, the dichotomy between Hobbes and Boyle is an artefact of selective filtration of the historical evidence. Finally, she argued that the modern thesis concerning the impossibility of performing crucial experiments is read into Hobbes’ objections to empiricism, which she saw a clear example of the bad practice of anachronistic torturing of history.

In reply Shapin and Schaffer stated that they pointed out many similarities between Hobbes and Boyle. They argued that upon this record they construed a sophisticated account of the debate through which the differences between the two could be appreciated more clearly. Pinnick had failed to see this because she dogmatically wanted to see only similarities. In reply Pinnick accused Shapin and Schaffer of being dogmatic about the a priori thesis of social causation, which in her view has only the effect of distorting historical reality. No wonder that Shapin and Schaffer followed this with another harsh reply, but luckily the ‘debate’ ended here.

There is quite an interesting point involved in the discussion, namely how to weigh the various differences and similarities between contestants in past scientific controversies. Moreover, should we focus on one controversy in isolation or place it in a wider framework? Yet the tone in which the debate was carried out did not allow for a fruitful exchange on such crucial matters. This, in my view, is mainly due to Pinnick’s original intention of refuting SSK, by way of beating down one of its most profound examples from historiography of science.

In the so-called Captives debate Scott, Richards and Martin pointed out that “an epistemologically symmetrical analysis of a controversy is almost always more useful to the side with less scientific credibility or cognitive authority.”125 The requirement to be impartial actually contains a hidden value judgement, namely that all participants in any controversy are always equally credible. Symmetrists are therefore always ‘captured’ by the least credible parties. The impartiality principle is therefore incoherent in itself. In a short reaction Collins simply embraced this criticism and pointed out that he liked to side with ‘the

underdogs and the bad guys. He argued that this stance is necessary to take away the self-evident character of credibility and authority attached to scientific theories. This again strikes me as an issue that cannot be settled by argument.

The discussion on the strong programme is relevant to the arguments from presentism and underdetermination, given in chapter 1. These question the desirability of evaluative historiography. It is no wonder then that attempts at refutation of the strong programme have failed. It makes me think of a remark Popper once made:

“I regard conventionalism as a system, which is self-contained and defensible. Attempts to detect inconsistencies in it are not likely to succeed. Yet in spite of all this I find it quite unacceptable.”

If there is something to find unacceptable about the strong programme, and I believe it has some unwelcome consequences for the study of past science, which are discussed in the next section, this requires one to oppose it with a more desirable approach to past science, because it avoids the unwelcome consequences.

4. The strong programme: undesirable consequences for historiography of science

In this section I list the, in my view, undesirable consequences of the strong programme for historiography of science. Next to this I point out that, when going along with the strong programme, two inconsistencies occur that the programme cannot get rid of.

One of the most profound effects of the strong programme is that it restricts historical investigation to local contexts only. The more local and specific our view of knowledge becomes, the harder it is to see how it travels. And yet knowledge does travel: how should we account for that? This has been recognized as a problem of delocalization, which was formulated by Peter Galison as follows:

127 Yet there is something about the impartiality issue that is problematic for SSK as the principle of impartiality does appear to sit rather uncomfortably with the symmetry principle on meta-level (see below).
128 At least they have not stopped symmetrical study of past science at all, see Golinski (2005). Even the issue why scientists continue to perceive themselves as impartial truth-seekers has been addressed. In Mulkay and Gilbert (1982) it is for example argued this perception is the result of social constructive process of identity building.
129 Popper (1968) p.82.
“If the original production of scientific knowledge is so reflective of local conditions – whether they are craft techniques or religious views, material objects or forms of teamwork, how does delocalization take place?”

The problem of delocalization has also been addressed as the problem of construction of knowledge (Golinski) or as the problem of the movement of local knowledge (Secord). Earlier Rouse had something similar in mind with his problem of theoretical decontextualisation. SSK can refer to negotiation that takes place when one local context comes into contact with another (for example through military conflict, the expansion of an empire or travelling long distances). It is however unclear how to understand this interaction within the SSK framework. Should we see the zone of interaction as a new type of context? Or is it part of both the two originally distinct contexts? And if knowledge from one context turns out to be relevant in another context, why is this the case? Arguably, it has often happened that accepted knowledge in one context turned out to have profound effects in another context, while the content of this knowledge was not changed very much in the course of adaptation. If this is accepted we come close to saying that ideas can have a determining effect on social circumstances. At least SSK has to admit that the acceptance of one idea is not dependent on one specific set of social factors, as both the interacting contexts come to accept it, and they are not exactly similar in terms of social factors. It is difficult to explain such interactions within the SSK framework because they problematize the very notion of context. Yet SSK explanation of past science relies heavily on clearly defined contexts because only within such boundaries can the role of social factors in the determination of theory choice be adequately specified.

We can see the problematic effects this approach to past science has on the study of scientific controversies. First, SSK demands that a definite closure is reached at every ‘junction’ in the history of science in which a controversy was played out. Secondly, in order to make the story of competing interests work; the interests of the conflicting parties must be represented as full oppositions. Hence differences between contestants tend to be stressed more than similarities. According to Pels (1996) the symmetry postulate has the effect of

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133 The point that should have come across in Pinnick’s critique on Shapin and Schaffer. It is also made in Pels (1996) and Schickore (2009).
over-schematization and hence obscures and misplaces the more interesting similarities and differences between contestants in a controversy.

The word ‘closure’ is probably already misleading. Settlements of controversies often have a temporary character. Closure is often partial, letting other things rest. Later on they often stand open for revision. And conflicts can be fought over again when the incentive to do so arises. These intricacies become visible only when the horizon of the historian is widened. Yet the localism of the SSK approach blocks access to a diachronically wider perspective on past science.

Such a perspective would also put the problem of underdetermination in another light. Martin Rudwick, in his account of the Devonian controversy in the history of geology, convincingly argued that at some point in time, it was no longer possible to refuse to join the consensus over the Devonian system. The two dissenters that kept doing so violated norms of good scientific conduct. Allan Franklin presented the same argument in his account about the history of the idea of gravitational waves. According to him it was proved, beyond reasonable doubt that the original ‘detection’ of the waves by Weber was due to a misinterpretation of the experimental results. The ones who thought so checked and double-checked their findings, forwarded their results to others for critical examination, and tried various ways of interpreting the results (using differences in scale, methods of calculation, etc.). The fact that Weber did not do all this but still stubbornly kept maintaining his earlier results, must, according to Franklin, simply be qualified as irrational behaviour.

Against the interpretation of Rudwick however, Collins and Pinch maintained that the dissenters had a genuine position to defend. Equally, against Franklin, Collins maintained that Weber had the right to follow his own method of interpretation even if no one else did so anymore. I am inclined to side with Rudwick and Franklin on this issue. Their historical narratives consist of diachronic sequences of interrelated developmental steps. If we perceive past science as a collection of research programmes, gradually unfolding in the course of time, we do not need to demand full closure of controversies at every step in the development, and this opens up the possibility of dealing with the

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133 The point that should have come across in Pinnick’s critique on Shapin and Schaffer.
134 Rudwick (1985).
135 Franklin (1998a).
problem of underdetermination in another way and avoid the consequences SSK has drawn from it.\textsuperscript{138}

Also stemming from SSK’s localism is a ban on comparative historiography of science. With the strong programme it is, for example, difficult to account for the simultaneous occurrence of similar discoveries or claims to knowledge in distinct localities. The specific social circumstances in these situations cannot have been the same, so why do different social structures sustain the same claims to knowledge? Take for example the interesting case of Galileo and Descartes (together with Beeckman), who both arrived independently at a, by current standards, mistaken formula of free fall. Both initially thought that the speed of the object was proportional to the distance covered. Only Galileo managed to correct this into elapsed time.\textsuperscript{139}

Koyré ascribed the double occurrence of the same error to the reigning ‘thinking cap’ of impetus physics, which had influenced both Galileo and Descartes. In SSK this explanation would require substantiation in terms of widespread correspondence of social structures and cultural factors, shared by a group of European scholars. But if we go this way, a historical context is no longer geographically identifiable. Again the crucial notion of context, on which the strong programme depends, is problematized. Also, the question why Galileo managed to correct his earlier theory, becomes interesting in comparison to the others, who did not manage to do so. Tackling this issue in some part depends on a comparison between the two cases. Yet, comparative analysis of past science cannot be part of the strong programme as this programme insists on causal explanations. While it is true that comparative analysis can help to identify causal factors in history, the mode of analysis is not causal in itself.\textsuperscript{140}

Next to the undesirable restrictions that SSK puts on historical explanation it runs into inconsistencies if one wants to live up to all of its principles at the same time. As many authors have pointed out, the neutrality/impartiality principle does not sit very well with the symmetry postulate on a meta-level.\textsuperscript{141}

As the reflexivity principle says that the strong programme must be applied to itself, symmetrists must be neutral with respect to other approaches to past science. This cannot be defended, while at the same time claiming that the

\textsuperscript{138} In chapter 7 I develop this diachronic view on the history of science in more detail.\textsuperscript{139} Koyré (1978) discusses the episode at length.\textsuperscript{140} I have not even mentioned comparison between historically distinct localities. This is surely out of reach of SSK and thereby leaves a host of historical interesting questions unaddressed.\textsuperscript{141} For example Pels (1995), Tosh (2006) and Schickore (2009).
The strong programme is a better approach than other approaches to past science. Were symmetry the only principle to follow, this would not be much of a problem because it would allow for partisanship on a meta-level. However, in combination with the other two principles, impartiality and reflexivity, the strong programme cannot be coherently defended on the meta-level.

Another point of difficulty involves the question whether social factors are allowed in the explication of past science when the operation of these factors was not fully clear to the historical actors in question. There are many socio-cultural values, conventions, rules of conduct, etc., that are so self-evident to participants in a society that they are not consciously aware of how they influence their decision making. Why is it allowed to be presentist in this respect and use current sociological knowledge in the explication of past decision making, whereas this is not allowed for the best of our current insights from the natural sciences? There is something uneven about this. It is exactly at this point that approaches based on generalized principles of symmetry differ from the strong programme.

The localism of SSK leads to a number of problems for historiography of science. Among these are the study of the interaction between ‘distinct’ contexts, the over-schematic treatment of past controversies and the overly restrictive conclusions drawn from the problem of underdetermination. The lack of comparative ground makes it hard to account for simultaneous occurrences of theory choice in distinct contexts, and makes it equally hard to speak of qualitative improvement. Finally the assumptions of the programme are not consistent. The uneven balance in the use of present day knowledge, as the use of sociological knowledge is allowed but natural scientific knowledge is not, is unaccounted for. And the demand for neutrality cannot be maintained with the principles of symmetry and reflexivity at the meta-level. In order to remedy these problems scholars have generalized the initial principle of symmetry. In the next section I discuss how and in the section thereafter the main approach to past science based on generalized symmetry, namely posthumanism, is critically evaluated.

5. Generalized symmetry: posthumanism

A number of extensions of the original symmetry principle stand out in the literature. They all involve erasing boundaries between what were previously regarded as distinct categories. A clear example is giving up the boundary between science and technology. Pinch and Bijker, for example, argued that we should stop regarding technology as applied science, i.e. as spin-off of pure
science. According to them the relations between theoretical claims and technology are so intricate, that we cannot even make a strict distinction between facts and artefacts.\textsuperscript{142} Historians and sociologists have, in similar ways, questioned other distinctions such as mind and hand, theory and practice, and discovery and justification. Posing more symmetry is synonymous with erasing boundaries. It creates a less discriminated object of study for historians of science. Or, in other words, more things shift from the resource side and become topics of investigation.

Giving up these distinctions means moving beyond the original symmetry principle, because that principle was formulated only in relation to the acceptance of belief. Yet, in themselves the extensions of the principle do not challenge the mode of explaining past science of the strong programme. They can easily find a place in, and often strengthen, the programme of explaining the course of science with reference to socio-cultural factors.\textsuperscript{143}

A real shift in thinking came about with the so-called posthumanist approaches to science. The turn towards posthumanism has also influenced research in history of science deeply, and this continues to be so to the present day. For both these reasons posthumanism requires an elaborate discussion. Posthumanists think that SSK had made a step in the right direction. With the introduction of the principle of symmetry they undid science from its universality and its ‘holiness’, making an empirical study of science possible. Yet the mistake of SSK had been to put ‘the social’ in the place of the old universality ideal, as another mythical entity. Posthumanists argue that SSK works with an \textit{a priori} preference for social factors over other factors. Hence, despite the insistence on a principle of symmetry, a deep asymmetry between the natural and the social has remained in the strong programme. In order to make a truly empirical study of science possible the boundary between the social and the natural must be erased, that is, the principle of symmetry must be generalized.\textsuperscript{144}

For posthumanists the natural world and the social world grow up together. No social structure (possibly given by sociology) and no natural structure (possibly given by the natural sciences) can play a role in the explanation of past science, because these structures stand in need of explanation themselves.

\textsuperscript{142} Pinch and Bijker (1984).
\textsuperscript{143} This also holds for the programme of reflexivity that was developed to combat the problem of coherence on the meta-level. See Woolgar (1988) and Ashmore (1988).
\textsuperscript{144} The first use of the term ‘generalized principle of symmetry’ in this sense is probably (Callon 1986). An important section on the generalization of the principle by one of its main proponents is Latour (1993) pp. 94-96.
Structures in the world are the result of an interaction process of agents (also called actants or actors), which can be both human and non-human. In SSK humans occupy central stage because of the dominance of social factors, which is a human category. With the principle of symmetry generalized, non-human agents acquire an important role too as one of the determining factors in science, hence the term ‘posthumanism’.

In order to understand this approach to science I first focus on the theory of one of its best-known proponents: Bruno Latour. Before the world is classified in social structures and natural structures, including the institution we call science and the knowledge claims that are defended in it, ‘things’ have not taken shape yet. Terminology is lacking here, but as something needs to be there in order to interact, Latour prefers to speak of quasi-objects. All the quasi-objects together make up everything there is in the world. They enter into processes of interaction. Latour speaks of actors who are constantly mediating with other actors in order to achieve networks of alliances. Hence he called his theory Actor Network Theory (henceforth ANT).

Processes of mediation repeat themselves continuously. In the process more stable structures, or networks of associations, gradually emerge. These also include our present-day classifications of the natural and the social. Such classifications can however be upheld only because they are sustained by supporting networks. Networks can acquire relative stability over time. Yet, nothing in the networks is permanent as every stable situation can be destabilized through further processes of mediation. Historicity is all-pervading in ANT. Nothing can escape the torrent of history. This also holds for all analytical categories. Real historicization of science, and hence a fully empirical approach, must divorce itself from any form of a-temporality.

Latour’s most famous case study is on Louis Pasteur and his theory of microbes. According to Latour, Pasteur’s theory beat its competitors (mainly Koch) because Pasteur was a shrewd negotiator who managed to create a strong network of alliances among fellow scientists, politicians and the entities in nature, which we now refer to as microbes, but which did not exist before the process of network building started.

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145 In Latour (1987) a programme for the study of science consisting of 7 rules of method and 6 principles is presented. Rules of method 3 and 4 together form the generalized symmetry principle. Note that corresponding ideas can be found in the work of Callon and Stengers.

146 Latour (1999), chapter 4 ‘The Historicity of Things’.

147 Latour (1988), Latour (1987) contains a number of other case studies such as the double helix theory of DNA.
Once relatively stable networks are in place, the process of interaction with actors acquires qualitatively different features. Actors will have to mediate in relation to the existing networks and networks of alliances can compete with each other, which introduces a new level of competition. Further, Latour argues that on the network level characteristics emerge that cannot be found on the actor level. For example, he asserts that the larger the network becomes the stronger the formal ‘nucleus’ needs to be to keep the network together. Mathematical or logical formalizations can play this role. In Latour’s view these are not a measure of truth but represent a demand of strength.

Next to this Latour argued that scientific theories or scientific instruments must be in finished form in order to move through networks. In this context he introduced analytical notions such as ‘immutable mobiles’ and ‘black-boxing’. There are quite a number of belligerent terms in Latour’s vocabulary. He basically sees the selection of scientific theories as a survival of the fittest between competitive networks. The strongest network of alliances will win over its rivals in a scientific controversy. After the closure of a conflict the winning theory is ‘black-boxed’. People tend to forget the contingent process that is behind the establishment of the theory. The theory becomes immutable (it is taken as a fact) and because of this it becomes mobile and helps to bind a network together.

ANT leads to a clear research agenda for the historian of science. Foremost, he or she must follow the interactions of actors and simply describe these, like an anthropological participant observer. Because this is not fully possible for historical study, the historian must rely on the inscriptions actors have left behind and study how these have been used in processes of negotiation. Note that symmetry breaking is explained in ANT only through the concept of mediation. Actants are always the cause of scientific outcomes (type level) but their specific interactions differ from case to case (occurrence level). The fact that actants mediate means that they are capable of exerting power and offer resistance to pressure. A more profound analysis of the concept of agency, beyond this capacity for mediation, is not required according to Latour. He needs to work with a broad definition of agency in order to include all actors, both human and non-human.

While following the actors, historians of science must of course also study how networks are formed, grow (win over competition) and decline (lose to competition). Changes in science are synonymous with changes in networks.

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148 This is exactly what is done in Latour and Woolgar (1979). Their approach bears similarities to Geertz’s ‘thick description’.
The degree of acceptance of a claim to knowledge is similar to the strength of the network that supports this claim. Historians of science can however re-open the black boxes by providing a detailed study of the interactions that have occurred that in the end resulted in the accepted theories. This gives the most detailed access to the contingent aspects of knowledge formation. It can also help to reveal important aspects of history that have become forgotten after black-boxing has occurred.

With the blurring of a clear distinction between what is natural and what is human, and with its focus on networks, Latour’s ANT is part of one of the strongest currents in present-day philosophy of science, and beyond. Network thinking is nowadays very common in many areas, quite possibly owing to the end of the Cold War, through which the world is no longer split up in distinct compartments, and to technological developments that have integrated the world such as the internet, GPS and mobile communication. The focus on networks involves a shift to a relationalist view of reality. For Latour the process of establishing relations is fundamental because things exist only via lists of associations. In this respect works from the continental tradition of philosophy appear to become relevant in the traditionally analytically dominated field of philosophy of science.

Above we have seen that Latour used the notion of quasi-objects in order to refer to the something that is there to enter in processes of interaction, out of which natural and social structures emerge. This however is not just a matter of classification. Posthumanist philosophers have also addressed the issue of ontology, with the idea that natural objects and artefacts merge together in new types of objects. It is by no means clear how to capture these processes and what to call these new types of objects. If we take posthumanists’ ideas seriously

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149 In Latour (1993) p.145 an explicit connection between the new study of science and the fall of the Berlin Wall is drawn. This however overstretches the point. Collins (1985) had already published a network perspective on science, although it is true that this differs from posthumanism on crucial points. For a view on history in terms of networks (webs) see McNeill and McNeill (2003). Barabasi (2002) contains interesting material with respect to network analysis in all areas of society. Other publications can easily be cited as well.

150 Gilles Deleuze is an important source of inspiration in this respect see Braidotti (2011), (2013). Note however that in the analytical tradition some take relationalism as the ultimate constituents of the universe. Muller (2013) for example states that entities are discernible not by properties but primarily by relations. This is also defended in variants of structural realism. See French and Ladyman (2003), Ladyman and Ross (2007), Esfeld and Lam (2008).

this means that answering the central problem in this thesis in some ways requires a departure from analytical philosophy and a venture into continental philosophy of science and feminist epistemology.

Returning to posthumanist models for the study of science, I would like to compare Latour’s ANT with Pickering’s idea of the mangle of practice. For Pickering all determining factors of history enter together in a mangle. In a process of interaction things come about, including institutional academic structures and scientific theories. Ultimately only agents can be responsible for changes. Pickering studies the interaction of agents via the concepts of resistance and accommodation. Like Latour he advocates an empirical strategy of following the actors. We should describe what they do, ‘in the thick of things’. Knowledge for Pickering is neither a construction, because reality plays an important role, nor a revelation of reality, because reality is active and not passively waiting to be discovered. He speaks of ontological transformations, hybrids, mediation and emergence. His model of science is very dynamic as no equilibrium ever is a perfect adaptation to the environment and will always be challenged to change. Pickering therefore purposefully uses evolutionary concepts to articulate his posthumanist views on science.

There are a lot of resemblances between Latour and Pickering. Latour’s ANT can also be read as an evolutionary theory, with its constant trials of strength. They both centre on actors and allow a determining role for human and non-human actors in the course of science. Differences between Latour and Pickering are therefore differences in emphasis, not in principle. Latour has developed a number of analytical notions, which can be used as tools of description, when it comes to the study of networks, which are missing in Pickering. Pickering however pays more attention to the notion of agency.

Where Latour does not make a clear distinction between humans and non-humans, Pickering defends the view that only humans possess intentionality. The agency of non-humans mostly manifests itself as resistance to human intentionality. There is thus a clear asymmetry between humans and non-humans in Pickering’s model as different mediating powers are ascribed to them. This asymmetry can however also be detected in Latour when he states

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are collected in Dolphijn and Van der Tuin (2012). Daston and Galison (2007) suggest that we may be on the brink of a new sense of objectivity, which they tentatively call the nanoarefact, inspired by developments in nanoscience. The nanoarefact is about presentation (actively interfering in nature) instead of representation. Perhaps we also need ontologies on other levels such as an ontology of processes in order to capture the hybridization of natural entities and human artefacts.

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152 See also chapter 6 on evolutionary approaches to science.
that humans are to be seen as the ‘weavers of morphisms’. The freedom they possess is the capacity to sort combinations of hybrids. Non-humans do not possess this freedom, at least not in a comparable degree. Finally Pickering is inclined to lean towards the mystical aspects of the mangle metaphor and to perceive everything as a great flow of being, without any (essential) distinctions. With Latour this is much less the case.

Posthumanism met with a delayed reception in history of science, but is currently very much en vogue. This is not due to deep philosophical reflection but the result of an increasing feeling of discontent with the prevalent localism in the field. An important keynote lecture at a meeting of the History of Science Society in Halifax in 2004 by Jim Secord can be seen as the kick-off of all kinds of research projects in the circulation of knowledge. The aim of these projects is to study how knowledge travels from one locality to another. Interlocal contact is often represented as connections in networks. Historians study how actors were connected in these networks and how they interacted with one another. For this they make use of ‘trans’ words like transfer, translation, transition, transaction, transcription, transformation, etc., (but not transcedence!). In their explications historians also frequently make use of notions introduced by Latour, such as ‘inscriptions’ or ‘immutable mobiles’.

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154 In later works Latour appeared to call for a departure from ANT towards even more symmetry. In Latour (1998) he wrote: “There are four things that do not work with actor-network theory; the word actor, the word network, the word theory and the hyphen! Four nails in the coffin.” We should just start thinking in a flux or a flow in which no distinctions can be made at all. However, in other places Latour clearly sticks to ANT, see for example his 2005 book titled Re-assembling the Social. An Introduction to Actor-Network Theory. If Latour has changed his position at all, I believe this change has not been significant. In any case the pure flow thinking is just too mystical to be of help for the study of past science.

155 See section 4.

156 Of the early examples Latour’s Pasteurization of France (1988) has already been mentioned. In Pickering and Guzik eds. (2009) a number of historical case studies based on the mangle concept can be found. Another important book is Porter (1996). Relevant publications after Secord’s lecture are Raj (2007), Roberts, Dear and Schaffer (2007), Cook (2007), Davids (2008), Raj et al. eds., (2009), Dupré and Lüthi eds. (2011), Roberts ed. (2011). One of the messages of these works is that circulation of scientific knowledge is indissoluble from economic traffic, processes of nation building, colonization, etc. The dominant focus is often on material culture following the slogan that ‘books, not -isms pass hands’. Many conferences on the history of science are organized with a focus on circulation of knowledge: for example the 4th international congress of the European Society for the History of Science (2010) took as its theme ‘The Circulation of Science and Technology’ and the 4th Woudschoten conference of the History of Science in the Low Countries (2011) opted for ‘Locations of Knowledge’.
Even though the focus of research and the use of posthumanist terminology are often not accompanied with a defence of generalized symmetry, such historical studies do rely, at least methodologically, on such a principle. It can be expected that the theme of circulation of knowledge, the analysis of past science in terms of networks and the ontological issues involving the ‘new’ materialism will continue to dominate the agenda in history and philosophy of science in the near future. Therefore it is important to reflect on the symmetry principle for historiography of science today.

The posthumanist approach to science offers a solution to two problems with the strong programme. First of all the network concept offers a way out of the problem of localism. With the network concept, the troublesome concept of context can be avoided. Posthumanists need not be clear on what exactly counts as a locality and what not. What is local, extra-local, or even global simply depends on how large the network is. When two distinct networks come to interact with one another a new network is, or new networks are, formed. Since posthumanism does not need the relative stability of social factors in the explanation of past science it is in a much better position to deal with such interactions.

Secondly, the social is undone from its mythical authority. Social factors remain important in the explication of past science but as one factor among others. On the one hand this has been achieved by further naturalizing the study of past science, that is the insistence on causal explanations of theory acceptance and rejection has become even stronger than in the strong programme. On the other hand we now have no access to sociological theory anymore as a resource of explanation. The number of resources has been decreased even further, compared to the strong programme. This has been a conscious aim with an empiricist research programme in mind, which Latour has put as follows: “The whole challenge of the exercise is to generate a maximum of differences by a minimum of means.”

Because the process of further naturalization has been a gradual one, commentators discussing the ‘social’ approach to past science often fail to distinguish clearly between the original claims of the strong programme and posthumanist approaches to science. For a fair debate on approaches to past science, posthumanism must be seen as a real shift in thinking about science,

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not just methodologically but also metaphysically. As said above, the SSK approach represents a form of perspectivism. Bloor, for example, combines ontological realism with epistemological relativism. The world allows for many different perspectives, which can be called systems of classification, systems of belief or conceptual schemes. All these perspectives are engendered by humans and in the last instance natural factors do not play a decisive role in the acceptance of them. According to Bloor all perspectives are equally distant from nature.

The strange thing of perspectivism is that the world is there somewhere, as it really is, but we are never able to reach the actual state of affairs. The world allows for the projection of a large number of perspectives but there is an unbridgeable distance between all those webs of belief and the world they are supposed to represent. Latour has, in my view rightly, pointed out that something strange is going on here. He argues that with perspectivism we have lost direct contact with the world and one needs to move beyond it in order to regain this contact.

Interestingly a similar way of reasoning can be found in Davidson’s famous attack on the ‘third dogma’ of empiricism. In this paper Davidson supported Quine’s earlier attack on the two dogmas of empiricism, namely reductionism and the analytic-synthetic distinction, but argued that Quine had mistakenly left a third dogma untouched. This was the dogma of the dualism between conceptual schemes and empirical content of sentences. Like Latour, Davidson argued that if we leave this dualism behind we can come to a theory of science that works with an idea of direct mediation between our conceptual schemes and the world, thereby regaining contact with that world. The similarity in views has been noted by Kremer:

“The quickest way of expressing this commonality is to say that philosophers as diverse as William James and Friedrich Nietzsche, Donald Davidson and Jacques Derrida, Hilary Putnam and Bruno Latour, John Dewey and Michael Foucault—and Richard Rorty, of course—are anti-dualists. They are trying to replace the world pictures constructed with the aid of metaphysical dualisms inherited from the Greeks (essence and accident; substance and property; appearance and reality, etc.) with a picture of a flux of continually changing relations.”

159 It is for example significant that Latour and Woolgar changed the title of their 1979 book *Laboratory Life: The Social Construction of Scientific Facts* into *Laboratory Life: The Construction of Scientific Facts* for the second edition. Dropping the word ‘social’ was done purposefully to avoid association with SSK.

160 Davidson (1973).

The commonality between Latour and Davidson is important for the current project, as it also visible in the discussion of the principle of charity in the next chapter.

Beyond dualism there is so to speak a single ontology of events. These are best captured via the notion of relations. It is only through establishing relations in a continuing process of mediation that structures in the world come about. To consider nature as being rich enough to allow for many different classifications, is already wrong from the posthumanist perspective because nature is not stable, instead its structures are shaped in continuous processes of interaction.

Latour has therefore described this position as relativist relativism:

“The relativist relativist, more modest but more empirical, points out what instruments and what chains serve to create asymmetries and equalities, hierarchies and differences.” 162

The more apt term, that he also uses, and that fits better with the work of other scholars, is simply relationalism. 163 The theory is dynamic and favours notions that express openness such as hybridization, emergence, adaptation, accommodation and meditation. Stability of networks is only temporary, as they are in constant flux. In contradistinction to SSK, posthumanism therefore represents a relational ontology of becoming. This is one of the main reasons why it leads to a different approach to past science, and should be dealt with accordingly.

6. Posthumanism: attractions and difficulties

Posthumanism has a number of attractions over the strong programme. The relationalist stance has made it possible to be undogmatic about determining factors in science. This allows for more flexibility with respect to both natural and social factors. The notion of local context is re-interpreted through the notion of the network. This removes conceptual difficulties for extra-contextual studies of past science. Finally the treatment of closure of controversies is less strict, as all forms of stability have only a temporary character in posthumanism.

Nonetheless, I believe that there are also serious difficulties with the posthumanist programme. A first problem is the confusion of levels of

161 Latour (1999a) p.161 speaks of relationism, but relationalism is a more current term.
analysis.\textsuperscript{164} Sometimes the social and natural structures that have emerged are seen as products of our classificatory schemes. This has occurred in interaction with natural entities, such as ‘microbes’, but it is we who eventually introduce the concept of microbes to speak about the natural world. This is different from speaking about objects directly, relating to the issue how we should perceive objects that are hybrids of natural objects and cultural artefacts. A clear example of this confusion is Latour’s third methodological rule from \textit{Science in Action} (1987):

“Since the settlement of a controversy is the cause of Nature’s representation, not its consequence, we can never use this consequence, Nature, to explain how and why a controversy has been settled.”

This rule contains a shift between theories of nature (representation) and nature itself in just one sentence.

Latour also does not distinguish consistently between the perspective of the scientist and the meta-perspective of the sociologist or historian of science. The latter should investigate what scientists do, or have done, and generally refrain from getting involved in the direct investigation of nature. Pels (1995) suggests that Latour sometimes blurs this distinction because of a tendency to equate science with nature and politics with society. The study of science and politics can then also involve a direct study of nature and society. All this leads to confused mixtures of ontology, epistemology, sociology and ethics.

Reasons for this confusion can also be sought in the decrease of conceptual resources that the generalization of the symmetry principle has brought. Past delineating concepts such as knowledge, science, nature and society, have lost their explanatory value. The new conceptual alternatives are not always sufficiently clear, mangles and quasi-objects for example are vague notions, and the characteristics of agents and networks are not specified in much detail.

Networks can in fact function as both explanans and explanandum. They are a topic of investigation for the historian of science but can also function as a resource in explaining how knowledge claims have become accepted or rejected.

Another problem with giving up resources for the explanation of past science is the ensuing lack of selective criteria. Every agent and all determining factors may become relevant to the study of past science. What about the meals that are served in the institutes in which scientists work? How do they travel from home to work? What about their family background? Etc. Once you start speculating

\textsuperscript{164} See also Pels (1995), Sokal (1998) and Bloor (1999).
in this manner everything seems to become relevant in ‘the great dance of agency’. If everything is potentially relevant to the study of past science the aim must be to literally describe all past interactions that have occurred between humans and non-humans. Not only is this simply impossible, it would also be rather pointless because it would amount to an exact repetition of the past without offering an analysis of the historical development of science, which is what the goal of historiography of science must be. Of course description of historical contexts is a vital component of such analyses but it cannot be the overriding purpose of the whole enterprise.

The problem is that there are no clear criteria to select what is relevant and what to leave out of our historical accounts. Why couldn't we simply choose an approach to past science based on demarcation? For example, why not simply select the Mertonian values? If the majority of historians of science can be made to agree with this choice, then the network of historians of science has accepted that this is the best way to approach past science. On what grounds could the posthumanist criticize this?

What has become unclear in erasing almost all analytical boundaries is why we should involve in a strategy of following the actors. What purpose does that serve? In SSK, a clear concern with modern society stood at the basis of their programme. Modern science in their view could be beneficial to society but in order to control its negative effects, science had to be brought down from its universal pedestal. Can one in posthumanism still address the question why modern science is such a successful enterprise? Can one answer the question why knowledge travels from one locality to another? It seems that with the posthumanist approach we can answer only the ‘how’ questions and describe how things have come about.

What is won in posthumanism in descriptive scope is lost in analytical scope. It is striking that posthumanists often fall back on SSK-style explanations in terms of conflicts of interests, processes of negotiation, trials of strength, the effect of power relations and processes of standardization to address the ‘why’ questions. It then becomes unclear indeed how much the generalization of the symmetry principle has brought for the study of past science. Moreover the other problems that we have listed with SSK, such as the lack of comparative ground and the demand for full closure of controversies, remain equally troublesome in the posthumanist approach.

Last but not least, there is still a problem with the inclusion of rational factors in the set of determining factors in past science. Posthumanists are not inclined
to grant these factors an important role. Especially striking is Latour’s advice in *Science in Action*. Rule 7 says:

“Before attributing any special quality to the mind or to the method of people, let us examine first the many ways through which inscriptions are gathered, combined, tied together and sent back. Only if there is something unexplained once the networks have been studied shall we start to speak of cognitive factors.”

The implication is that this does not occur. I believe that the exclusion of cognitive factors has to do with the fear that including them automatically leads to the attribution of ‘special qualities of the mind.’ And hence this would lead us all the way back to approaches of past science based on a demarcation between rational and social factors. This is why, as soon as cognitive factors are mentioned, symmetrists immediately shout ‘halt!’ They suffer from what I would like to call a demarcation reflex. Unfortunately this reflex restricts assessments of past science to quantitative terms, namely in terms of the length, depth and degree of interconnectedness of networks. It blocks access to real evaluations of past science, which are of a qualitative kind.

So far we have discussed the two major approaches to past science based on two principles of symmetry. We have found that the application of these approaches to the study of past science is problematic. Generalization of the symmetry principle solved a number of the earlier problems we had with SSK (but not all), but created new problems by itself. Still, I am attracted to symmetrical study of past science because I think that an approach based on demarcation will inevitably lead to the application of rational norms when these should *not* be applied (see also chapter 4). We must however find a way to include cognitive factors in our set of determining factors.

At the end of this chapter a tentative proposal for symmetrical study of science, including a new idea to account for symmetry breaking, is presented with these goals in mind. With this proposal I believe that most of the problems with symmetry and generalized symmetry we have discussed above, can be answered. On the other hand the proposal still needs to be related to a platform for the study of past science that enables comparative evaluation, if it is to work as a basis for evaluative historiography (see chapters 6 and 7).
7. A proposal: new relationalism

Approaches based on demarcation use different sets of factors to explain correct and incorrect claims to knowledge. In more schematic formulation we thus have something like (1):

(1) Type factor A explains acceptance/ rejection of x, type factor B explains acceptance/ rejection of y, where x and y are scientific theories and x belongs to the right (course of) science while y does not.

Both approaches based on symmetry and approaches based on generalized symmetry can be captured with the same formal structure, which reads something like (2):

(2) Type factor A explains both x and y, where (a1, a2,.., an) explains x and (a1, a2,.., an) explains y. (a1, a2,.., an) stands for possible instantiations of type A. The correctness of x and y is not relevant to the explanation.

All knowledge claims must be explained with reference to the same type of factors. Symmetry breaking is explained with reference to differences in instantiation of these factors. In the strong programme the type of factors that do the explanation are social factors. Instantiations of these explain all instances of theory acceptance and theory rejection. In the case of posthumanism agency is the determining factor. On occurrence level the interactions of agents again differ from case to case. The deciding type of factor has changed, but the mode of explanation has not, as both approaches make use of a hierarchy of factors. In both approaches one type of factor is dominant over the others.

In my proposal the leading idea is that the determining factors in science such as natural, social, psychological, rational, and personal factors, as well as cognitive resources and technological possibilities, require no predetermined hierarchy. Their relative efficacy is an empirical matter that must be accounted for by means of historical research. The other idea is that theory choice can be accounted for by looking at changes in the combinations of the factors. The new relationalist explanation of past science must look like (3):

(3) Type factors A, B,.., Z explain both x and y. Differences in acceptance are explained by different relations among instantiations a1-an, b1-bn, ..., z1-zn of the factor types A-Z.
It is in fact not exactly symmetry that is defended in this new relationalist approach but heterogeneity, as the acceptance and rejection of scientific claims to knowledge do not have to be explained with reference to the same type of factors in all cases. Yet before research all type of factors could possibly have played a role in the determination of theory choice. Only detailed historical investigation can reveal which factors played a role in a particular instance of theory choice and what the dominance relations between these factors were.

The idea to explain changes in the historical process with reference to changing relations between relatively constant factors resembles an approach of the German historian, Nipperdey. Society according to Nipperdey consists of many processes that all have their own mode of development. It is through the intersection of these processes that a historical context can be carved out.\(^{165}\) According to Nipperdey it was possible to establish relatively constant elements of human life but that different relations between these elements occurred from time to time. It is through these differences that an account of the special characteristics of historical periods can be given.

In Nipperdey’s model the same type of elements may occur in historical contexts but the combinations between these elements are always different. This idea is not yet present in (1), as it was introduced in the science studies by SSK, with the distinction between a constant type of factors and differing instantiations of the type variable. I believe this is still a fruitful idea. It is maintained in (3) and will also serve the purpose of setting up a platform for historiography of science in chapter 7.

At least two other major approaches to past science have been formulated that are both based on the recognition of a number of constant elements. These approaches account for differences between historical contexts in terms of differences in the combination between the constant elements, like a chemist whose different molecules are built from the same set of elements of the periodic system. A clear example of such an approach is offered by John Pickstone.\(^{166}\) Pickstone thinks we can identify a number of fairly constant ‘ways of knowing’ such as world reading, calculation, analysis and experimentation. Science in any period of time is always a compound of these ways of knowing. In one historical period one way of knowing can become dominant over the others. The period from roughly 1780 to 1850, for example, is interpreted by Pickstone as an ‘age of analysis’, as in a lot of fields of study scholars were busy

\(^{165}\) Nipperdey (1976).

\(^{166}\) Pickstone (2000) is the key publication. More recent articles are Pickstone (2011a) and Pickstone (2011b).
with the collection of data and ordering these data according to particular systems of classification. This does not imply that other ways of knowing were not present in that period, but only that analysis was the dominant way of approaching things.

This model must be clearly distinguished from the more static style approach put forward by Crombie.167 Crombie argued that several research styles alternated in the history of science. He distinguished between deductive, experimental, analytical-hypothetical, taxonomic, statistical, and evolutionary styles. So far this resembles Pickstone’s model. Crombie however offers a choice between styles in distinct periods, whereas Pickstone argues that, although styles indeed alternate, the other styles continue to be present too. The alternation is a matter of dominance relations and not of wholesale replacement. Pickstone’s model therefore is more dynamic as it allows for many possible combinations between the ways of knowing or styles of reasoning respectively.

Another ‘chemical’ approach to the study of science was recently defended by Hoyningen-Huene.168 According to Hoyningen-Huene science differs from other human activities by being more systematic. The difference is however gradual and must be studied on nine distinct levels. These levels are loosely defined and it depends on the concrete field of research how the specific forms of systematicity have actually taken shape. Again, in this approach a number of constant elements are indicated on type level. Distinctions follow by investigating instantiations and combinations between these elements on occurrence level.

The symmetry breaking proposed in (3) is especially designed to account for theory choice. This is not the case in the ‘Ways of Knowing’ model, which can capture changes in science only on a very general level. In the example of the ‘age of analysis’ the different modes of analysis have to be made domain specific to function as criteria for the acceptance and rejection of theories. As it stands however, the five ways of knowing are too generally formulated to play this role in historical explanation. Moreover it appears that Pickstone’s model has to be supplied with another mode of explanation that can account for the causes of the changes in the relations between the five ways of knowing.

In (3) above the factors A-Z are selected because they are the determining factors in theory choice. Compared to either SSK or posthumanism more analytical guidance is demanded at type level as the list of factors that are possibly relevant to theory choice is much longer and requires specification,

albeit in minimal terms. *A priori* guidance of historical research on type level must be present, but loose at the same time. Only at occurrence level, that is, in concrete historical circumstances, do the determining factors acquire their explicit meaning and efficacy. One advantageous prospect of the model is that the door is opened to rational factors again *without* the need to posit a line of demarcation. How exactly to interpret this set of factors, and how rational factors would facilitate evaluative historiography in this framework, are still desiderata that need to be met in the chapters to come. If we give up on a demarcation between internal and external realms of science, as I think we should do, the question is whether this pre-empts all space for evaluations of past science. The challenge is to show that to posit a context of justification does not depend on a demarcation between internal and external factors.

In this chapter I hope to have demonstrated that symmetrists have not ‘gone crazy’ but that instead symmetrical approaches to past science rest on fairly cogent arguments that require serious attention. Although I have uttered dissatisfaction with the main symmetrical approaches to past science, a number of their key ideas, such as the type-occurrence distinction of SSK or the relationalism of posthumanism, should, in my opinion, be embraced and find a place in an approach to past science that aims to move beyond them, in order to allow for qualitative assessments of past science.