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Science Studies as Naturalized Philosophy

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Science Studies as Naturalized Philosophy

by

Finn Collin
University of Copenhagen, Denmark

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Finn Collin
University of Copenhagen
Dept. of Media, Cognition and Communication
Njalsgade 80
2300 Copenhagen
Denmark
collin@hum.ku.dk

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Preface

In the mid-1990s I had the good fortune to be a member of a research group at the University of Copenhagen that also comprised Heine Andersen, Bo Jacobsen, Søren Barlebo and Jørgen Zachariassen. The topic of investigation was the standards of scientific excellence embraced by Danish social science communities, and the effort was sponsored by the Danish Research Council for the Social Sciences. In this forum, my budding interest in STS received a crucial stimulus, and I owe my colleagues in the research project a deep debt of gratitude for our fruitful and enjoyable discussions. Since then, I have been struggling to find my bearings in the complex debates within STS and to put together my own contribution to this discussion. It took a research grant from the Danish Research Council for the Humanities, starting in 2008, to provide me with the undisturbed time needed to finish this project. I am grateful to the Research Council for providing me with ideal working conditions and to my two collaborators in the project, Jan Faye and David Budtz Pedersen, for their helpful comments upon my drafts. Steve Fuller, Vidar Enebakk, Klemens Kappel and Thomas Basbøll also read the manuscript, or parts of it, and I am truly grateful for their comments. All the above mentioned individuals, alas, remain unpersuaded by my argument on one point or another, and it behoves me to state that I alone am responsible for the views expressed in the following text. The manuscript has also benefited from comments from an anonymous referee for Springer Publishers.

This book has been a long time coming, and I am grateful to the publishers for their generosity in repeatedly extending the deadline for submission of the manuscript. I also want to thank the Department of Media, Cognition and Communication at the University of Copenhagen for granting me the privilege of a sabbatical that provided perfect working conditions. Gratitude is also extended to the Department of Philosophy of the University of Berkeley where I spent at period during the fall semester of 2008 as a Visiting Scholar and took the final steps towards finishing the text. A great thanks is owed to Lorilea Jaderborg for removing infelicities in my English.

Sage Publications kindly granted me permission to reuse material from an article, "Bunge and Hacking on Constructivism", published in *Philosophy of the Social Sciences*, vol. 31, no. 3, 2001, while Edward Elgar Publishers gave me permission to reuse material that first appeared in Hans Siggaard Jensen *et al.*, eds, *The*

Evolution of Scientific Knowledge, Edward Elgar 2003. My contribution to that volume was entitled "Evolutionary, constructivist and reflexive models of science", and the present work comprises pp. 64–69 of that article.

Most of all, I want to thank my wife Ingrid for her boundless patience with my odd working hours, and my frequent absentmindedness, during the long time of gestation of the text before you. This book is dedicated to her.

Introduction

1. The topic of this book is Science Studies considered as naturalized philosophy. I use "Science Studies" – or alternatively, "Science and Technology Studies" (STS) – as a broad term intended to cover a cluster of rather heterogeneous efforts to study the phenomenon of science by empirical, scientific means that have emerged within Western academia since the start of the 1970s. The cluster mainly comprises sociological and anthropological approaches, but also includes psychological investigations. Adopting the perspective of naturalization means highlighting those aspects of Science Studies that constitute an attempt to replace, or at least to augment, traditional philosophical approaches to science with empirical ones, or to answer traditional philosophical questions by empirical means.

The naturalization of the philosophy of science is, on the face of it, a narrowly technical issue unlikely to generate any great interest outside of professional academic circles. Yet two circumstances invest it with a significance transcending this narrow perspective. First, it is a recent, local manifestation of a long-term trend in Western thought that has contributed to defining the social and cultural conditions under which we live today. That trend has to do with the ever-increasing prestige of scientific ways of understanding man and the universe and the gradual withering away of rival ways of thinking, be they religious, magical or metaphysical. Hence, considerable historical and cultural interest attaches to this phenomenon. Secondly, the particular contribution to this general trend that I shall examine in this book at one stage generated a heated and much-publicized confrontation between Science Studies, on one side, and philosophers and natural scientists, on the other. This conflict reached an intensity that has earned it the epithet of "the Science Wars" and has attracted broader attention to itself in the general public. While this war has been fought with science as its object of contention, scientists have not entered the fray to any great extent, with a few high-profile exceptions. The main belligerents have been philosophers and representatives of Science Studies.

My analysis of the confrontation between STS'ers and mainstream analytic philosophers of science is thus placed in a historical framework that represents STS as generated by the coalescence of two intellectual trends: one long-term and internal to the development of philosophy – that is, the drift towards naturalization that is really coeval with philosophy itself – and the other short-term and externalistic – that is, certain critical attitudes towards the societal role of natural science.

The former commands greater interest, for the reason just indicated, and the first chapter of this book is largely dedicated to examining it, while less attention is given to the latter, more ephemeral, trend that largely reflects incidental, albeit profoundly defining, events of the 20th century, including two world wars. The point of intersection between these two trends is at the same time the point of origin of my narrative. Historically, this is the emergence of the so-called Edinburgh School. Two full chapters are dedicated to telling its story, which is a tale of the pernicious interaction between the School's purely scientific objectives and its broader ideological agenda which forced it into an increasingly hostile confrontation, and at the same time entanglement, with philosophy. This inner tension in the Strong Programme stimulated an intense hybridization among its descendants within STS, leading them to move in many different directions which I trace through such authors as Harry Collins, Bruno Latour, Andrew Pickering and, finally, Steve Fuller. In the concluding chapter, I sum up the entire story and ask what lessons have been learned with regard to future efforts to enrich the philosophy of science by empirical means – that is, the project to naturalize the philosophy of science.

2. The relationship between Science Studies and philosophy is a complex and delicate one. Peaceful coexistence between the two would have been possible if Science Studies had restricted themselves to examining the *actual* conduct of science, leaving philosophy to develop models of its *right* conduct. Such a division of labour, reflecting the distinction between fact and norm, could be sustained even if it were agreed that a description of the way science is actually done has implications for its proper conduct; if, for instance, it were held that the only notion of scientific rationality available to us is one elicited from the actual conduct of scientists. A demarcation between the two undertakings could still be upheld,at least from the side of empirical science studies, for the demonstration of a possible spill-over from fact to norm would itself be a philosophical move that could not be made within a purely scientific (i.e. descriptive-explanatory) perspective upon science. Thus, even if support for proposed models of scientific rationality, or challenges to established models of this kind, could be mounted on the basis of empirical findings about science, the empirical studies of science could still adopt a studiously neutral stance on such questions, leaving them to philosophers to worry about.

True, if the traditional philosophical view is correct, there would be limitations to the scope of sociological explanations of science. If valid science is the product of a brute confrontation with reality in the form of experiments, mediated by scientific methods that are themselves timeless, rational verities, as the story is sometimes told in simplistic versions intended mainly for undergraduates, the sociology of science is largely restricted to dealing with invalid science – or with the context of discovery, rather then the context of justification. This is indeed the conclusion drawn by the father of the sociology of science, Robert Merton. But even if sociologists accepted this picture, they might not want to get entangled in debates with philosophers about precisely where sociology comes up hard against these limitations. Empirical students of science might want to probe these limits from the inside, as it were, i.e. empirically. They would explain science in the appropriate scientific categories wherever they could get purchase and leave the rest to other approaches, such as those employed by philosophers.

Modern Science Studies have elected not to abide by these restrictions, however, and to revolt against Mertonian sociology. They have struggled energetically to shed the restrictions that philosophers put upon the reach of empirical methods in the study of science. In so doing, they have allied themselves with the long-term slide of Western thought towards naturalism: the idea that empirical, scientific investigation holds the key to all genuine knowledge about the world in which we live, and that other styles of thought, such as philosophy, represent stages in the development of mankind that we have now overcome.

That Science Studies represent an attempt to naturalize philosophy is no news; it is indeed a very explicit tenet in the Edinburgh Strong Programme, which will form the point of departure of our investigation, and was bequeathed to later descendants of that programme. Hence the objective of this essay is not to establish this fact, but to do three things that are subsequent to its recognition. The first is to place this naturalization in a historical context. One circumstance in particular will be in focus: From the perspective of Science Studies, the naturalization of philosophy was never the primary goal, but was rather a side effect, or a step along the way towards the ultimate goal, which is that of achieving a better understanding of science and its role in society.[1] This effort had a tacit normative aspect to it from the start, reflecting the sentiment that science has achieved a false dominance in society and that a more modest role should be found for it. The focus on science is the reason why the most visible manifestation of the rivalry between philosophy and STS was named the "Science Wars", not the "Philosophy Wars". Philosophy was mainly dragged into the battle because it had made itself the staunchest and most vocal defender of the view of science that was being attacked. In my presentation, I highlight the ideological concerns that caused so much turmoil around the naturalization of the philosophy of science, by contrasting this development with the peaceful naturalization of epistemology that happened at about the same time.

The second objective of this book is to subject this naturalizing effort to critical scrutiny. At issue is not a general criticism of naturalism qua naturalism, but rather a criticism of the particular forms, in themselves quite diverse, that this effort has taken in recent Science Studies. Still, a common theme emerges out of these particular points of criticism: In invoking social science to explain natural science and resolve the philosophical problems inherent in it, Science Studies come up against the problem that social science is itself controversial in its methods. To tackle those problems, it is inevitable that recourse is had to philosophical considerations; this is indeed what has happened. As a matter of fact, the practitioners of Science Studies have got into fierce debates among themselves about the proper way to conduct the study of science – it would not be entirely amiss to call them the "Science Studies Wars". (They are made up of separate skirmishes, such the Barry–Harry debates between Barry Barnes and Harry Collins, the Chicken debate involving Harry Collins, Steven Yearley, Bruno Latour, Michel Callon and Steve Woolgar, the Bloor–Latour debates, and lately even the Latour–Fuller discussions.) These controversies are of an unmistakably philosophical nature and have been every bit as inconclusive as those we experience in standard philosophical discussions. This is ironic, since one of the aims of Science Studies was precisely to bring to an end the interminable philosophical debates over the nature of science – with logical positivists pitted against Popperians pitted against Feyerabendians – by means of sober empirical investigation of how science is actually conducted. Yet, in fact, this outcome was inevitable, given the unresolved methodological issues still bedevilling social science.

The third aim of this book is to indicate ways of overcoming the barrenness of current Science Studies and move towards a more constructive role for the empirical study of science within a naturalized philosophy of science. The discipline has been hampered by a drift towards ever more radical stances. I shall argue that, on the contrary, what is needed is a mainstreaming effort. This might take the shape of an approximation towards the stance adopted in current work in Social Epistemology as conducted by such authors as Alvin Goldman and others. This work also represents an effort in naturalization and the guiding idea is that of *reliabilism*, that is, the idea that the validity of man's methods for gaining knowledge cannot be established by a priori means, but only by empirical, a posteriori investigation. The empirical material generated by Science Studies fits into this project very well, since it shows the actual mechanisms – social, instrumental and others – which give science its reliability and robustness as a revealer of truth.

3. The present work is written by a philosopher and no doubt bears all the marks of its lineage. Thus, although I frame my discussion in a partly historical format, my main interest is certain systematic issues that are key to the viability of a naturalistic approach to the philosophy of science. These have to do with such issues as explanation, relativity and reflexivity. Thus, the investigation is largely undertaken from an "internalist" point of view, although it is placed within a historical framework of an externalistic kind, chronicling the emergence of a concern about the role of science in society in the postwar decades. In employing a distinction between internalistic and externalistic stances, I have not already adopted a methodological position hostile to Science Studies, which dispute the tenability of this very distinction. What offends STS is a particular way of effecting this division, which presupposes that the intrinsic perspective is specifically "rational", while this fails to hold for "extrinsic" concerns. No such presupposition is involved here at the outset, although one of the conclusions of our investigation will be that an exclusively externalist construal of science will generate reflexivity problems for the scientific study of science itself. In the present context, the internal/external distinction merely reflects an intuitively clear division between arguments appealing to features of the subject matter of a particular discipline, its theories and standard methods, as opposed to arguments referring to features of the societal context in which it is embedded. Thus, this division conforms to my aim of not begging any issues against Science Studies in my critical investigation of their scientific practices.

My primary concern in conducting this study is a philosophical one, that is, to determine what STS can contribute to the philosophy of science, in a constructive vein. Frank admission of this rather limited objective will, I hope, serve to mitigate possible misgivings that I have been overly selective in my choice of people to discuss. This narrowness of scope, which would clearly be inexcusable if the purpose had been to tell the full story of Science Studies, is mainly motivated by lack of space. Still I do not think that this narrowness vitiates the soundness of my conclusions; my systematic points could have been sustained even if it had been possible to include in my account such figures as Karin Knorr-Cetina, Steve Woolgar, Michael Lynch and others. My particular perspective upon science studies also implies that I shall focus on the theoretical reflections within Science Studies, leaving largely unaddressed the fast-accumulating body of empirical studies which, for all their intrinsic interest, remain peripheral to my purposes. They will only be introduced for occasional exemplification of the general points.

Contents

Chapter 1
The Naturalization of Philosophy

1. Naturalism, in the broadest sense of the word, is the view that the sum total of reality is coextensive with nature and that, as a consequence, human knowledge has no object beyond the natural realm. There is no higher, transcendent sphere; in particular, the human mind (or "soul") does not itself inhabit any such higher realm, nor does any aspect of man's activities or thoughts bring him in contact with such higher spheres. It follows that all phenomena are to be investigated by the same methods that are brought to bear upon the humbler parts of the natural world. Naturalism thus canvasses a *monistic* claim both with respect to ontology and methodology, to the effect that the world is a uniform realm that must be explored with one and the same set of methods.

Naturalization is the movement of thought towards naturalism. A movement of this kind has been a pervasive characteristic of European thought for two and a half millennia, a slow but inexorable drift away from an original dualist mode in European philosophy, and indeed of European thought in general, towards an ever more stringent naturalistic monism. The original dualism was a fusion of a number of partly overlapping contrasts dominant in Greek thought; themes that no doubt perpetuated religious or mystic modes of thought highlighting the contrast between body and spirit. One such opposition is that between *matter* and *mind*; another one pertains to the duality of the *material* and the *ideal*. This opposition was also known by other names, such as that between the material and the *rational*; it coincides roughly with the opposition between the *factual* and the *normative*, between the *contingent* and the *necessary,* and between the *transient* and the *eternal*. Just as these contrasts have manifested themselves within a broad range of intellectual fields, so their gradual elimination has had broad repercussions. For instance, it is instantiated in the gradual weakening of religious ways of understanding man and the world in Western thought. Here, however, we shall focus solely on developments within philosophy.

2. At the high point in Greek philosophy, i.e., in the philosophy of Plato, the demarcation between all these roughly coincident sets of opposites was drawn with exceptional starkness. It was primarily defined in *ontological* terms, with the rational, necessary and normative constituting a special, privileged realm of being composed of eternal and immutable essences: It is the world of "ideas". He who

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thinks rationally – and only he who thinks rationally thinks *truly* – does so by virtue of having established cognitive *rapport* with this reality. The association between rationality and the world of ideas is established by the fact that the paradigm examples of ideas in Plato were mathematical or logical entities, the nature of which may be captured in eternal and self-evident truths. Such truths are the primary exemplars of cognitive rationality. Plato's aim in positing a realm of ideas was mainly to account for the certainty and immutability of mathematical and logical truths, virtues indicating that their objects do not have their home in the transient sensory sphere, but inhabit a nobler realm. In Plato, the material world, the sphere of contingency, can only be the object of fleeting opinion (*doxa*). All these related aspects of Platonic thought are captured in the famous parable of the Cave in the *Republic*.

But already at this high point of Greek philosophy, the first retreat was made from this extreme dualist position. Turning away from the ontological extravaganza of the Platonic picture, Aristotle brought essences down from their transcendent realm and lodged them in objects of the empirical world as their immanent "natures". Along with this came a significant shift in epistemology: the strict separation of the empirical, irrational and unknowable on the one hand and the ideal, rational and knowable on the other was dissolved, since observation and induction (in the special Aristotelian version of *epagoge* from the *Posterior Analytics*) were now recognized as adequate tools for the cognition of essential truths. The deflationary trend within ontology continued as the intellectual inheritance of Greek philosophy was perpetuated during the Middle Ages. Nominalism and the rejection of universals gradually emerged victorious in the medieval debates over essentialism.

3. With the scientific revolutions in the 16th and 17th centuries, naturalism underwent a crucial metamorphosis, at the same time gaining increased momentum. From now on, naturalization was no longer driven by ontological but rather by epistemic considerations. The breakthrough of natural science gave proof that man possesses the cognitive tools with which to penetrate the secrets of empirical nature. The material world is not the realm of the irrational, the shifting and transitory character of which renders it impenetrable to the human mind; rather, those endless transformations can be shown to exhibit a mathematical and hence rationally comprehensible order. Knowledge is not got by turning away from the empirical world to contemplate another, immutable realm, but from interaction with the empirical world through observation and experiment. Science now becomes the main vehicle of naturalization; indeed, it is necessary to distinguish henceforth between two versions of naturalism, one of which is the broad notion examined before, while the other is *scientization*, i.e. the incursion of empirical science into areas of knowing that were previously the preserve of theology and philosophy. The point of making this distinction is, of course, the existence of schools of thought that combine naturalism in the broad sense with anti-scientistic leanings.

The classic early example of naturalization is astronomy, a study which had originally been pursued by means of speculative, non-empirical methods and a priori principles with roots in the ancient dualisms of European thought: The immutable heavens are set starkly against transient, terrestrial reality, the former being governed by eternal, rational principles embodying ideals of an aesthetic as well as a moral nature. They display a macrocosmic "harmony of the spheres", which is to be replicated in the microcosm of man's mind through mimetic assimilation. The heavens represent the rational, the eternal, the knowable and the ideal. This cluster of ideas, originating in Greek thought, was easily appropriated by the Christian church for its own purposes. By the end of the 17th century, however, the heavens had become an object of empirical science, and appeals to their rational, mathematical perfection had lost all force, as had invocations of Christian scripture and the picture of the heavens contained in it.

The profound transformation brought about by the scientific revolution was reflected within philosophy itself in a turning away from ontology and towards epistemology. The central task of philosophy was henceforth to explain how such a momentous intellectual achievement was possible. The task proved a difficult one, even though the object of research, scientific practice, was after all right before the researchers' eyes; indeed, some of those who philosophized about the nature of scientific knowledge had themselves contributed to its success. Still, the intimate fusion of mathematics and observation that characterized the newly emerging science proved quite resistant to philosophical unravelling. The first philosophical school that tried its hand at the task, the rationalists, failed to appreciate the significance of the empirical component, falsely assimilating the nature of theoretical science to axiomatized mathematics.

The British empiricists of the 18th century were better at handling the epistemic challenge posed by the advent of natural science, as the role of empirical evidence was at long last given its due – to the extent, indeed, of underestimating the importance of mathematical methods. To an empiricist, the proper method of thinking, even in science, consists in sticking closely to the given in experience and to the conclusions that may be inferred with certainty from that basis. Such conclusions turn out to be a rather slender lot. According to David Hume (cf. *Treatise of Human Nature*, Hume 1729/1888), human thought extends legitimately only to matters of fact and to "relations of ideas". Whatever lies beyond this is denounced as forever uncertain, or even meaningless. Large scale metaphysical speculation is dismissed as empty of content. Thus, we get an epistemological monism that further strengthens the ontological monism that represented the first departure from the original dualism of European thought.

The empiricist criticism of rationalism was quickly pushed to extremes, in particular in Hume's sceptical empiricism. Hume extends his critique from the realm of metaphysical and theological speculation to the theoretical elements of science itself, notably in his analysis of causality. Hume puts forth the first constructivist thesis in the history of philosophy, although in a psychological form, not a social one. He argued that the entire causal framework of description that we apply to reality is a fiction and a projection, or at least is so if, by the term "cause", we understand something involving an aspect of necessity. To the extent that they possess an objective aspect, causes and effects exist in the form of sequences of events, or, differently put, in the form of regularities in our experience. According to Hume, we have no rational grounds for projecting these regularities beyond our past experience.

Kant's epistemology as set forth in *Critique of Pure Reason* (Kant 1781) represents an important albeit mainly retrograde step in this historical development. Intent on forging a compromise between rationalism and empiricism, Kant granted the empiricist premise that meaningful human thought reaches only as far as our sensory evidence lends it support, leaving no room for dogmatic metaphysis. This is not to say, however, that the only source of knowledge of the empirical world resides in sensory evidence. For, being defined and delimited by our perceptual experience, the world of phenomena conforms to the fundamental categories of our thought. Hence, certain features of empirical reality may be recognized a priori and enjoy a necessary status. This goes for certain characteristics of time and space and for the overall causal order of our experience. On the ontological side, Kant hangs on to the idea of a transempirical, noumenal realm, but only in the sense of a limiting notion: An unknown "X" about which nothing can be said.

During the rest of the 19th century, the naturalizing tendency would face opposition from various descendants of Kantian philosophy, with numerous compromise positions emerging. On the epistemic side, naturalization is represented most consistently by John Stuart Mill, who tried to show that even mathematical truths eventually reflect empirical generalizations (Mill 1843). Mill also espoused ontological monism with his doctrine that the world is to be construed as the sum total of sensations, most of them, however, being only *possible* sensations. An ontological monism of the same ilk is Ernst Mach's theory of "elements" (Mach 1959); both may be seen as sophisticated (and secularized) versions of Berkeleyanism. An interesting compromise position between empiricism and Kantian rationalism is Whewell's philosophy, which has been referred to as "inductive rationalism" (Whewell 1840). It agrees with Kant that human thought is informed by concepts and principles that are not derived from experience, but are somehow imposed upon it; these are not timeless or a-historical notions, however, but rather ideas that evolve and receive clarification in step with the development of science. An alternative, more sociologized, version of the idea of a "historical a priori" was developed by Émile Durkheim, who argued that the necessity and universality of the categories of human thought that the Kantian tradition interpreted as signs of a transcendental status were instead a symptom of the social, supra-individual origins of those cognitive structures (Durkheim 1915, pp. 13–20).[2]

4. The naturalizing trend continued in 20th century thought, both inside and outside of philosophy. In philosophy, the signal event was the rise of logical positivism and the "linguistic turn" it brought in its train.

But before we examine this, we have to address briefly the notorious split in 20th century thought between analytic and Continental philosophy and the bearing it has on our current topic. We may capture part of the difference between the two by saying that while both are naturalistic in the broader sense introduced at the beginning of this chapter, Continental philosophy is largely anti-scientistic; in particular, continental philosophy is sceptical with respect to all sorts of reductive efforts inspired by natural science, both with respect to its methods and its ontological implications.

Some of the roots of continental philosophy lie in a movement that, at least in certain respects, was explicitly anti-naturalistic in the sense of anti-scientistic; I refer to *phenomenology*. Husserl's anti-scientism resided in his resistance to the reduction of the normative, idealized disciplines – such as logic, mathematics and, indeed, philosophy – to the empirical science of psychology (Husserl 1973). Mill's reduction of mathematics to psychology offered the primary provocation for his thinking. Husserl was even partly anti-naturalist in the broader, ontological sense, since he postulated a transempirical world of essences, to serve as the object of a special cognitive mode, *Wesensschau* (Husserl 1980, 1982, 1989).

This ontological profligacy was not endorsed by Husserl's successors, such as Heidegger and Sartre. Still, they too insist on the difference between their depiction of human reality and a purely scientific, empirical study; they are staunchly anti-naturalist in the sense of being anti-scientistic. Through most Continental philosophy runs the theme that man will forever elude capture in the restricted categories offered by the empirical sciences. This, by the way, would later bring them at loggerheads with an emerging anti-humanistic trend in French thought, inspired by linguistics and hence a representative of "scientism". This was *structuralism*, represented in its strictest form by Claude Levi-Strauss. But, characteristically, this line was soon to be opposed, from within as it were, by Roland Barthes, Julia Kristeva and others, who insisted on the fluid character of linguistic and cognitive structures. The same transformation happened to Marxism in the French tradition: The "scientistic" (but not natural-scientistic) structural Marxism of Louis Althusser (cf. Althusser & Balibar 1965) was opposed by lines of thought that stressed the fluid, constructed character of social reality and of man as an inhabitant of it.

Thus, as we pursue the issue of naturalism into 20th century thought, we shall largely be dealing with developments within analytic philosophy and leave Continental philosophy on the side. We shall examine how Science Studies grew out of early 20th century analytical philosophy as one of its two major roots, in an effort to supersede this philosophy in a way that would finally bring the latter's inherent naturalism to full consummation. The task was to purge philosophy of its residual non-naturalistic elements. Still, as Science Studies strove to take the final steps towards a fully naturalized view of thinking, and of reality, it made frequent use of insights from Continental philosophy.[3]

5. As we turn to logical positivism of the early 20th century, we find a species of naturalism that dismisses statements purporting to deal with a trans-empirical reality as not so much mistaken or otiose as simply and strictly meaningless. The positivists conducted an aggressive naturalizing campaign with respect both to ontology and epistemology, wielding the verification principle as their weapon. Only the empirical world exists, while purported claims about trans-empirical reality are eliminated by means of a "logical analysis of language" (Carnap 1931). Correspondingly, there is no a priori mode of cognition, for want of a rational realm to serve as its object,

nor are there any transcendental, non-contingent features of empirical reality for it to deal with.

To the positivists, this doctrine received crucial vindication in the developments in theoretical physics at the beginning of the 20th century: Einstein's theory of relativity showed that even space and time have no privileged ontological or epistemic status, thereby eliminating a residual Kantian a priorism that was still influential among philosophers and even physicists.

Another instructive example of the positivists' push towards naturalization is mathematics. This discipline was traditionally construed by philosophers as dealing with a special non-empirical realm. The positivists' ontological and epistemological monism is consistent with the special status granted to the truths of mathematics, as this was not due to the latter having a special reality as their object, but rather to their having no object at all: They are purely analytic statements, merely reflecting the way that we have defined our terms.

The same applies to Carnap's conception of logic. However, it took some time for this conception to mature. In its early stages, Carnap's understanding of logic was heavily influenced by the logical absolutism of the logical atomists, in particular the early Wittgenstein of the *Tractatus*. Here, logic is a unique thing and the utterances that cannot be translated into this privileged medium are consigned to meaninglessness (Carnap 1931). But by the time he wrote *Logical Syntax of Language*, Carnap's outlook had changed dramatically. Now, there is no "logic" in the singular; instead, there are as many logics as you care to construct. Carnap now espoused a *Principle of Tolerance*: "*It is not our business to set up prohibitions, but to arrive at conventions ... In logic, there are no morals*. Everyone is at liberty to build up his own logic, i.e. his own form of language, as he wishes" (Carnap 1937, § 17; italics in original). Logic is no longer seen as limning the transcendental scaffolding of the empirical universe, but instead as exploring something as common and ordinary as a set of human conventions.

Still, there is a minimal but crucial remnant of a priorism inherent in logical positivism (as defined, in particular, by Carnap). The positivists celebrated science, in particular natural science, and energetically championed a "Scientific Conception of the World" (Hahn et al. 1929). Everything in the empirical world must be studied exclusively through the methods of science, at the expense of any a priorist undertakings. However, ironically, one important exception was tacitly made to this principle, that is, with respect to science itself. In presenting their picture of science, the positivists come perilously close to a priori speculation. Their models of science, revealingly dubbed "rational reconstructions", could hardly be passed off as simple descriptions of actual science, nor on the other hand are they to be understood as pure prescriptions; rather, they are a priori articulations of the ideal essence of science, of which actual science is only an imperfect realization.

The prescriptive, non-naturalistic aspect of Carnap's philosophy of science is strongly in evidence on two points, that is, in his sketch of a constitutive system of scientific concepts in the *Aufbau* (Carnap 1928), and in his development of a rigorous epistemic logic built upon the probability calculus in *Logical Foundations of Probability* (Carnap 1950) and later works on inductive logic. The first project aims at defining the concepts of science in such a way as to guarantee their purity of metaphysical slag; the latter, at safeguarding the epistemic credentials of science. (Of course, the two projects are not independent.) Mach had argued that there are remnants of ontological obscurantism in science, for instance in the atomic theory of matter; these are murky metaphysical pockets that need to be cleaned out. In the *Aufbau*, Carnap adopts the less radical strategy of retaining the traditional physical notions, but only on condition that they can be rendered metaphysically sanitary. The way to do this is by reducing them to basic notions that are securely observational and hence metaphysically sound.

The other, epistemic project is meant to safeguard inductive reasoning. Both projects are attempts to articulate the essence of science as a systematic structure of knowledge. The reconstruction shows how scientific knowledge is really knowledge, that is, it is justified, and is superior in systematicity as compared to everyday knowledge.

Carnap held that it is the task of philosophy – or better, the successor discipline of "logical syntax" – to devise artificial languages in which to conduct science. He left it as a "practical" question, a matter of "expediency" or "fruitfulness", which of these languages is best suited for the purpose (Carnap 1937, especially Part V, 1950). This notion of "pragmatic" or "practical" questions was always left conveniently vague in Carnap. There was little urge to make the obvious further step of invoking systematic empirical investigation, undertaken for instance by sociologists of science, to determine these pragmatic merits. In Carnap's architecture, this possibility apparently fell between the cracks of the bifurcation of systematic cognitive pursuits into a purely formal part – "logical syntax" – and a material part, consisting of the familiar sciences. This picture left little room for an empirical "meta-science" investigating the "fruitfulness" of different methodological approaches within the object-level sciences. As it happens, ideas along the latter lines were found in inchoate form in the works of Neurath, who from the start had distanced himself from Carnap's language-centered, formalist approach. However, with the early death of Neurath in 1945, these ideas would never reach fruition, and the future of logical positivism would largely be defined by Carnap and those who shared his formalist predilections, such as Hempel.[4]

Carnap's non-naturalist philosophy of science forms part of a non-naturalist residue at the very heart of logical positivism, pertaining to the status of philosophy itself. To this core belong such ideas as the verification criterion of meaning, and the fundamental commitment to empiricism. But even here, there are formulations in Carnap that take a step in the direction of naturalism. Carnap would occasionally describe both the verification criterion and the empiricist stance as *proposals*, the merits of which are, presumably, to be determined in terms of "pragmatic" considerations (Carnap 1936–37). What remains to be done to turn this into a full-fledged naturalistic stance is, again, to assess the "pragmatic" merits by means of a rigorous sociological investigation.

6. We have just touched on a basic irony in logical positivism: Logical positivists wanted do away with philosophy, as traditionally conceived as an a priori,

non-empirical enterprise, and replace it with a naturalistic, *scientific* investigation of reality. But their efforts to achieve this were based upon a conception of science that was itself non-naturalistic and non-empirical. Some of these defects were to be addressed by two post-positivist thinkers who both have been highly influential in 20th century analytic philosophy, namely, Ludwig Wittgenstein and Willard Van Orman Quine.

The starting point of these philosophers' engagement with logical positivist dogma, however, was not the positivists' philosophy of science, but rather their philosophy of language. Wittgenstein's version of naturalism antedates Quine's, but here we shall first look at Quine's rather more developed, and more familiar, views to serve as a foil for the examination of Wittgenstein's version, which will be postponed until the next chapter.

An important spring of Quine's naturalism resides in a central problem in Carnap's philosophy of language, epitomized in the attempt to draw a distinction between analytic and synthetic sentences (Quine 1951). In conformity with the general conventionalism of his late philosophy, Carnap tries to effect this distinction by invoking the notion of conventional, explicit definition, or of conventional "meaning rules". To Quine, there are serious problems with this project, close kin to the worries about making contractual agreement or other sorts of explicit consent the foundational notion of a theory of political order. Historically, such contracting episodes are non-existent and, as a matter of fact, in order to engage in explicit definition of terms, or in drafting explicit contracts, people already must have a language at their disposal. Hence, philosophy of language must resort to the idea of "implicit" definition of terms, just as political philosophy had recourse to the idea of "tacit consent" to the social contract (Quine 1969c). But Quine despairs of giving a suitable sense to this notion.

Quine does not contest the existence of explicit convention, as a derived phenomenon, whether in language or in social life in general: There is such a thing as a government adopting an official name for a newly-instituted agency or office, such as the U.S. Department of Defence, the Federal Drug Administration, or the UNESCO; thereby, the meaning of that term has been laid down by convention. But now a problem of compliance arises. What if everyone adopts a nickname for the institution, such as "the Pentagon" for the Department of Defence, and never uses the official name? And what if even those who behave in conformity with the rule turn out to be unable to formulate it, or are even unaware of its existence, but just mimic what everyone else does? In what sense can the existence of a convention "in the books" explain and elucidate a practice that is causally independent of those stipulations?

Quine had further, more technical, objections to Carnap's proposal, which we need not enter into here. What concerns us is Quine's replacement of Carnap's theory by a naturalistic account, closely modeled upon Skinnerian behaviourism (Quine 1960). Language use is basically construed as a system of operant conditional responses. In the process of learning language, the child's spontaneous emissions of word-like sounds are selectively rewarded by the parents when the baby is in the vicinity of relevant objects; thus his verbal output is moulded into a socially acceptable shape. When he is visually confronted with a rabbit, the baby's verbal emissions are applauded in proportion to their approximation of the vocable "rabbit". In this way, the baby learns to emit the appropriate word when confronted with a particular creature. However, to speak properly, we should not say that the term is cued to the presence of some object, but rather to the presence of certain sensory representations of that object, or, even more scientifically correct, to certain stimulations of the speaker's sensory surfaces, caused by the animal.

Quine is primarily interested in an account of speech that identifies, as faithfully as possible, the sensory cues that elicit verbal behaviour. In the end, precise identification will turn out to call for purely physicalistic redescription of those cues. Semantics is ultimately a matter of physical analysis of those irradiations of sensory surfaces that control man's verbal behaviour. Thus Quine adopts a causal semantics, with *stimulus meaning* as its core notion. Stimulus meaning of a term T is defined by the ordered set of those sensory stimuli that will elicit a positive verdict concerning the applicability of T to a given segment of reality and those stimuli that will elicit a negative verdict.

Most words in language are not only cued directly to sensory stimulation, but also indirectly. Thus, utterance of the word "rabbit" is not only prompted by stimulation that suggests a familiar long-eared animal of modest size, but also by observations of the rabbit-fly that is often found in the vicinity of rabbits. Observation of this insect will not immediately elicit the rabbit-response, but rather the rabbit-fly response; still, it will increase the likelihood of emission of the former, especially in situations of uncertain and fleeting rabbit sightings. Thus, the emission of the rabbit-response has been cued to the emission of the fly response. Other terms are tied to reality by such indirect links only; this goes in particular for scientific and other abstract terms. The links are provided by theories, whether everyday ones or strictly scientific. Utterances of the word "proton" are not directly cued to the presence of protons, which are too small to be picked up by the senses, but are instead cued, for instance, to the presence of fine vapour trails in a metal box and to the linguistic response appropriate to it. The proton response is only elicited by virtue of a complex interconnection between the sentences of a theory that explains the workings of the box, known as a "bubble chamber", combined with an elaborate theory of the ways of protons, including their propensity to ionize a gaseous medium through which they pass so as to cause the production of vapour.

According to Quine, this understanding of language shows that certain key elements of the traditional picture are without foundation. In particular, it lends no support to the distinction between analytic and synthetic sentences so dear to logical positivists. On Carnap's conception, analytic sentences follow from the "meaning postulates" of the language, while synthetic sentences make up the rest of meaningful discourse. But, according to Quine, we cannot, in the description of our linguistic practices, draw a useful distinction between sentential interconnections that reflect "meanings" (definitions, "meaning postulates") and those that represent mere empirical but not meaning-bestowing ties, prominently regular co-occurrence of the phenomena referred to. Given the way that terms are typically learnt by ostension and the way that language functions as a going concern, such a duality in the description of language lacks any solid basis. If rabbits invariably, in our experience, have been found to have both long ears and to be accompanied by the rabbit fly, it makes little sense to insist that the former is a defining feature, while the latter is not. Our linguistic practice with the term "rabbit" does not allow us to draw such a distinction.

Nevertheless, we may draw a distinction that captures some of the epistemic features attributed to analytic and synthetic sentences, respectively. The latter, we think, are subject to empirical refutation, while the former are immune in this respect. Thus, we may simply distinguish between sentences that are given up in the face of unanticipated sensory evidence (irradiations of sensory surfaces) and sentences that the speaker will assent to, come what may in the way of such irradiations, but without postulating any differences in the underlying mechanisms of language use. Quine refers to these kinds of sentences as "stimulus-synthetic" and "stimulus-analytic", respectively, adding that, unlike the traditional distinction, this one allows gradual differentiation between sentences that are easily and promptly given up in the face of recalcitrant observations and sentences that the speaker will cling to come what may. Stimulus-analyticity is thus a graded property in Quine's book.

7. In *Der logische Aufbau der Welt*, Carnap offered a picture of the conceptual structure of language, precisely specified and of hierarchical form, forming a "constitutional system" where each level serves to define the one above it. Only a language with an explicitly defined syntax may genuinely possess this feature. Quine offers a rival picture: The concepts in language form a network of crisscrossing interconnections, exhibiting the manner in which assent to a sentence involving one concept will increase or decrease the speaker's tendency to utter, or assent to, a sentence featuring other concepts.

These connections come in all strengths, making the bi-conditionals expressing them range from the radically stimulus-synthetic to the stubbornly stimulus-analytic. Put in a more orthodox terminology, the inferential relations between sentences that serve to define their semantics are no longer held to be purely deductive, but also inductive. The network indicates the way in which sentences are taken by the agent to lend epistemic support to each other. But this means that the network may equally well be construed as a model of the sum total of the agent's *beliefs* about reality: his global "theory" of the world. And this is indeed how Quine represents matters in "Two Dogmas of Empiricism".

Quine takes a holist view of scientific knowledge, depicting it as an all-encompassing network of sentential interconnections of varying strengths. The outer border of the network defines the interface between the knowing subject and physical reality. At this interface, physical reality impinges on the knower's sensory surfaces and evokes the utterance of appropriate sentences, or a least a propensity to assent or dissent to such. By virtue of the interconnection of sentences, the "impact" from the sensory stimulus will spread, as it were, to other, less observational sentences; in traditional parlance, an observational report may support a theoretical assumption, or throw it in doubt, by virtue of their logical or empirical interrelations. The more theoretical the sentence, the more the force of the observation will be dissipated before it reaches the sentence, and the less is the chance that the impact will bring about a change. Metaphorically, we may talk of sentences being located “deeper” in the network in proportion as they possess this property. The most deeply embedded parts of our knowledge system consist of logic and mathematics, which are largely, but not absolutely, secure from the impacts upon the surface of the system: Observations may occasionally bring about deep theoretical upheavals in our system, even extending to the logical foundations of the system (quantum logic might be an example of this).

Quine recognizes two global forces operating in the interior of the sentential network, structuring and partly modifying impacts on the periphery of the network due to sensory stimulation. They are cognitive concerns that help shape the scientific edifice. The chief and most important of these is our predilection for *simplicity*. We tend to adopt the simplest overall account that is consistent with the sum total of our observations. But this is counteracted by a different concern, which Quine refers to as *conservatism*. (STS’ers such as David Bloor would savour this political metaphor, to which Quine attaches no substantial significance.) This is the injunction to minimize change in the event of an impact. The two strategies give different, and at times contradictory, counsel. The call for simplicity would occasionally dictate major upheavals in our total picture of the world, e.g. by replacing a geocentric structure with a heliocentric one, while conservatism impels us to limit change as much as possible, encapsulating it in a local modifications of the system; it dictates the introduction of ever more complex epistemic epicycles at the periphery of our belief system.

Quine famously held that there are often several equally good ways to accommodate a new, recalcitrant observation in our global theory of the world. Some modification is imposed upon us, but often this can be effected in different, equally satisfactory ways. A local change, conforming to the principle of conservatism, may be as satisfactory as a global change, responding to the principle of simplicity. “Equally good” here means “equally in conformity with the observations” (this is the celebrated Quine–Duhem thesis). Quine maintains that underdetermination even holds when all observations, future as well as past (indeed, all possible observations), are in. The underdetermination problem means that our theories about the physical world extend further than our means for effectively deciding them, even in principle.

8. The issue of the underdetermination of theory by evidence leads to the famous doctrine of the *indeterminacy of translation*, and of meaning (Quine 1960). We shall go into it in some detail since, first, it demonstrates the radical consequences of naturalizing philosophical semantics and, second, it will be useful as a standard of comparison when we examine the semantic implications of the Edinburgh Strong Programme. The issue of indeterminacy emerges when we apply the underdetermination doctrine to a special case, that is, to theories of meaning for a particular language or, as Quine prefers to call them, “translation manuals”. Such manuals are underdetermined by the evidence, too.

It is important to separate, in Quine, the indeterminacy of translation and meaning, *sensu strictu,* and the *inscrutability of reference* (Quine 1969a). The latter introduces the famous *gavagai*: Suppose we encounter a foreign tribe in whose verbal output the utterance “Gavagai” figures. Noticing that this utterance typically occurs when the natives are in the vicinity of a rabbit, we hit on the translation of “rabbit”, understood as a one-word sentence, such as, “There goes a rabbit”. But now it occurs to us that our total record of data about native behaviour is equally consistent with the translation, “There goes a bundle of undetached rabbit-parts”. No amount of further information will warrant a preference for one translation over the other. Quine contends that what we have here is not a case of the inscrutability of the native mind, but rather a case of genuine indeterminacy: there is no fact of the matter as to what the natives mean.

In cases of inscrutable reference, there is never any discrepancy between the *truth values* of native utterances, as translated either way. Their truth values are necessarily identical; the sentence, “There goes a rabbit”, must always share the same truth value as the sentence “There goes a bundle of undetached rabbit parts”. The disagreement between translations pertains exclusively to what Quine calls the “quantificational apparatus” of the translated language, that is, its referring terms and quantifiers. These define the ontological commitments incurred when speaking the language. Thus, the whole issue may fairly be said to involve differences of terminology only.

The situation is different when we turn to the more intriguing case of indeterminacy of translation, *sensu strictu*. Here, rival translations of the native output ostensibly lead to incompatible assignments of truth value to certain sentences. Such contradictions will always pertain to deeply theoretical matters and are hence empirically unresolvable; this might for instance occur when we attempt to interpret the natives’ religious beliefs, or, closer to our current interests, their physical theories. Either translation manual is compatible with all the empirical evidence, whether past, present or future. Hence, there is no empirical choice between the translations. For Quine, this is a situation of *indeterminacy of translation*: There is no answer as to whether the native physicists subscribe to theory A or theory B.

However, Quine does not draw the conclusion that A and B hence are equivalent and that there is no fact of the matter as to whether reality conforms to A rather than B: If the natives have firm intuitions to the effect that A and B are incompatible, this is evidence that the two theories are not equivalent, although we are incapable of explicating the difference.

The final step is to move from the natives to ourselves. According to any methodology not blatantly invidious, what applies to the natives applies to ourselves as well. We are thus forced to admit that, whenever we are faced with rival theories that transcend empirical testing (think of esoteric rival interpretations of quantum mechanics such as the Copenhagen interpretation or the Many Worlds Theory), there is no question either as to whether we really subscribe to theory X or theory Y. Still, if we share the conviction that those two theories are indeed incompatible, this leaves room for the possibility that reality may conform to one of them and not the other, although this fact is forever undetectable.

9. The picture of human knowledge offered in "Two Dogmas" is naturalistic in the sense that language and theory are reconstrued according to a model provided by empirical science, namely behaviourist psychology. Yet this naturalistic aspect is not highlighted in the article. We get a different story when we turn to the later article, "Epistemology naturalized" (Quine 1969b). Here, Quine makes an explicit commitment to naturalism. As in "Two Dogmas", the point of departure is taken in logical positivist orthodoxy. Quine first distinguishes between two concerns of epistemology, one conceptual and the other doctrinal, thus echoing a distinction made by Carnap in the initial sections of *Aufbau* between constitution of concepts and axiomatization of scientific knowledge. In the *Aufbau*, Carnap concerns himself only with the former. This is a question of tracing the connections between concepts, rather than assessing and systematizing the truths expressed in terms of them. The connections together form a complex, hierarchical structure: The relations between the sense-data level and the level of objects are complicated logical inferences; taken together, they amount to a complete phenomenalist analysis of experience, the main elements of which Carnap sketched out in the *Aufbau*.

Quine briefly traces the demise of this picture in the developments between the World Wars. Carnap soon had to give up the idea of a strict equivalence between object concepts and concepts couched in observation terms: natural languages fail to show such determinate logical interrelations that would allow definition of the former in terms of the latter. A crucial step on the way was Carnap's introduction of "reduction sentences" (Carnap 1936–37). Here, he gave up specifying (material) identities between sentences, instead just listing some of the implications licensed by the use of a concept, and those that conversely license such use. However, as Quine sees it, this strategy loses much of the virtues of a Carnapian "constitution", which is the possibility of wholesale elimination of large numbers of the terms of a language, thereby achieving cognitive economies.

In place of pursuing the vain attempt to describe these fictitious conceptual structures, epistemology, Quine counsels, should transform itself into a naturalistic, scientific enterprise. This new discipline would simultaneously deal with both the conceptual and doctrinal aspects of knowledge: It would describe, as an empirical phenomenon, the processes through which human beings come, by virtue of socialization into the language and of exposure to external stimuli, to produce, "in the fullness of time", a torrential verbal output that we take to describe a world made up of material objects. This account thus would cover both the process by which a human being acquires the conceptual structure inherent in the natural language it learns and a body of knowledge ("doctrine") expressed in this language. Quine has provided an outline of this in *The Roots of Reference* (Quine 1973) and, in part, in *Word and Object*.

We notice that the emphasis of "Two Dogmas" and "Epistemology Naturalized" are slightly different: Whereas the former deals with the structure of science, the latter is focused upon a topic closer to the concerns of traditional epistemology, that is, the process though which the individual subject acquires an understanding of the world surrounding him. But there is hardly a difference in doctrine here: In "Epistemology Naturalized", too, epistemology is described as being concerned with the structure of science. Moreover, it is characteristic of Quine's mode of thought that he would describe both individual, everyday and scientific knowledge in the same terms, that is, as "a tool, ultimately, for predicting future experience in the light of past experience" (Quine 1953, p. 44).

10. As is happens, the discipline that Quine called for under the name of "naturalized epistemology", and of which he cites a few early examples towards the end of the article, was already establishing itself firmly within academia when Quine wrote these words. It is known as *Cognitive Science*, and it might be of interest briefly to trace its roots and to assess its standing vis-a-vis contemporary philosophy. This will provide a convenient contrast when we turn to the alternative, sociological mode of naturalization.[5]

As we have seen, Quine's own inspirations in the empirical investigation of cognition came from Skinnerian behaviourism. Behaviourism was indeed one of the contributory streams to cognitive science as well (Skinner 1953, 1957). Behaviourism is essentially a theory about *learning*; a theory, that is, about how animals and men learn to cope with their environment by means of a few simple cognitive mechanisms, basically of a simple "inductive" or "associationist" nature. But, of course, most behaviourists would have reservations with respect to that way of putting things, which would strike them as overly mentalistic. Behaviourists saw themselves as harbingers of a truly scientific revolution in psychology, the "science of the mind", which had for too long been mired in the bog of an outdated introspectionism. The rallying cry was for a way to do psychology that, in J.B. Watson's famous words from a previous generation, would be a "purely objective experimental branch of natural science" (Watson 1913). Behavourism would focus upon the objectively observable manifestations of mind, that is, behaviour, but would explain these not as expressions of the workings of an obscure mind-substance, but as the product of aspects of the environment that were thought to shape behaviour through simple mechanisms. Still, we may justly describe a large part of their efforts to be addressed to problems of thinking and learning.

In the 1950s, a general opening towards recognition of powerful cognitive structures, far exceeding simple associative mechanisms linking inputs and outputs, became visible in American psychology, in part inspired by gestalt theory and other European contributions. Central in this development were Jerome Bruner and George Miller; in a series of ground-breaking experiments, Bruner demonstrated the role of antecedent categories in perception (Bruner and Postman 1949). Miller examined the cognitive structure of memory and discovered the notorious limitation of normal memory: it will hold only seven items. His article "The magical number seven, plus or minus two" has been hailed as marking the birth of Cognitive Science (Miller 1956).

There were other attempts afoot to explore the gap between input and output, which behaviourism had tried to bridge with an extremely simple cognitive mechanism. One attempt was a computational approach, which in itself had several roots. One was in engineering, which would analyse control processes in terms of the notion of “negative feedback”. Emerging from WWII studies of techniques of weapons delivery control, such as the aiming of anti-aircraft guns, this approach, under the name of cybernetics, would enjoy a brief period of flourishing just after WWII (Wiener 1948). But by the beginning of the 1950s it was waning, being gradually overtaken by advances in computers that spawned the rival development of Artificial Intelligence. Like cybernetics, this development was originally called forth by requirements of the war effort, among other things efforts to crack German war codes. Soon, this work led to general reflection on the nature of systematic problem solution through “computation”, i.e. the stepwise, strictly rule-governed approach to problem solving. It was demonstrated that electronic circuits could be made to interact in such a way that they simulated logical operations. Alan Turing and others did interesting early work here: Turing showed how a certain kind of very simple, formal mechanism, a “Turing machine”, could solve any kind of problem that was solvable through a systematic, stepwise procedure (Turing 1936, 1950). In the next generation, people like Herbert Simon and Allen Newell made optimistic predictions about the possibility of developing a “general problem solver”, a computer programme that would enable a machine to solve all types of problem in a systematic way (Newell and Simon 1972).

Even later, more specialised programmes of great promise were developed by Terry Winograd and others; these programmes constituted what came to be known as Artificial Intelligence (AI). Robots were built that could navigate a simplified environment consisting of large geometrical, three-dimensional shapes, and perform simple actions with them at the experimenters’ linguistically formulated request (Winograd 1972).

Still another important input to cognitive science came from linguistics. This discipline, too, witnessed a turning away from an initial behaviourist phase, represented by such figures as Morris and Bloomfield. The crucial event was Chomsky’s demonstration that the mathematical structure of sentential syntax is more complicated than appears on the surface (Chomsky 1957). We have to postulate strong formal cognitive structures to account for the complexities of natural languages. The same structures play a role in the learning of languages, where Chomsky specifically argued that Skinnerian mechanisms are woefully inadequate to account for the facts (Chomsky 1959).

11. The key notion of the emerging cognitive science is *information processing*. This is a brilliant conceptual innovation that establishes a secure neutral platform on which the new science can operate. Information processing is not a mentalistic notion; we need not see such operations as the conscious workings of a Cartesian mind. Whatever mentalistic connotations still cling to this notion are easily put aside as merely metaphorical, in particular by a generation of researchers who grew up *pari passu* with the development of the digital computer. We could hardly describe the computer without using this terminology, but would not take ourselves to be committed thereby to a mentalist ontology for such devices. On the other hand, the terminology of information processing is not physicalistic or otherwise reductionistic: To describe something as an operation is to adopt a formal, functionalist stance;

it describes a transformation of an input into a output, but without revealing how the transformation is engineered, physically or otherwise.

Moreover, while the terminology of information processing is conveniently uncommitted both "upwards" towards a mental realm and "downwards" towards physiology, it is nevertheless easily developed in the direction of either and thus is well-suited to eventually linking up the two levels. By unpacking such broad and fuzzy mentalistic notions as "thought", "memory", "attention", and so on, we may gradually operationalize them and develop them into detailed specifications of the processes involved. The strategy, aptly described later by Dennett (1971), is to gradually remove any reliance on unanalysed "insights" or "understandings" and similar mentalistic notions, replacing them with sequences of operations so elementary that they are realizable by simple on-off devices. At this stage, we are free to start speculating about what purely neural mechanisms could do the trick.

There is a corresponding motion in the opposite, "bottom-up", direction, ascending from neurophysiology to the information-processing level. Discoveries about the neural architecture of the brain may point us towards the right description in information-processing terms. Admittedly, neurophysiological discoveries are not likely to prove particular functional descriptions to be uniquely correct, at least at the current stage of development of neurophysiology; but they may at least constrain hypotheses about the structure of information processing in the brain. Neurophysiological findings have indeed on occasion served to eliminate one out of a number of competing hypotheses. This pertains in particular to theories about the degree to which the brain is segmented into "modules", or whether information processing occurs in a more distributed manner.

The notion of information processing is thus a suitably neutral notion for cognitive science to work with. It does not imply consciousness, thus enabling cognitive science to sidestep thorny metaphysical questions concerning the nature of conscious thought. It allowed cognitive science to engage with what much later (Chalmers 1996) was to be dubbed the "soft problems" concerning the mind, that is, how the mind generates its cognitive results, while sidestepping the "hard problem", which concerns the nature of phenomenal consciousness, such as the "raw feel" of a pain, or the mind-body problem.

12. The developments within cognitive science examined so far might be described as *rationalistic*, in a broad sense. They treat cognition as a ratiocinative activity, with processes of logical inference serving to extract information from premises of sentential form, or to solve sententially formulated problems. The powers attributed to the knowing mind are reminiscent of the rich battery of powers with which the rationalist philosophers endowed it. Already at a fairly early stage, however, there was input to an alternative line of development in cognitive science coming from neurophysiology, where advances were being made in the techniques for describing the neural architecture of the brain. Since Paul Broca's famous study (in 1861) of a man with heavy, acquired speech impairments, so called "deficit studies" had been the main tool of experimental neurophysiology, and were used with particular success by Luria and others in the 1930s. However, by the 1950s, more sophisticated

techniques had been adopted by such researchers as Wilder Penfield and his collaborators (Penfield and Rasmussen 1950). They inserted tiny electrodes in patients' brains during surgery, in order to localize centres of activity without causing damage. The result was a far better "map" of the crucial areas of the brain than had been obtained by Broca and others. Similar, but even more sophisticated, techniques were involved in so called "single neuron electrophysiology", applied mainly to animals. Using minuscule electronic gadgets, it was possible to record activity in individual nerve cells.

Reflection concerning the neural fine-structure of the brain led to alternative ways to formally model this organ's information-processing. The brain's fundamental structural atom is the neuron, which is a highly specialized cell having numerous interconnections with other cells of the same kind. Through these interconnections, a neuron receives inputs that it relays to other neurons by a single outlet, the axon. Out of reflection on the features of such networks grew so-called *connectionist* models, which are networks of electrical circuits that are either on or off and hence share the binary *modus operandi* of neurons, which are either firing or not. Such simple models were soon refined to a level where they could simulate simple mental functions, such as vision (Rosenblatt 1962).

Connectionist models break with the rationalist standpoint of cognitive science. The typical AI model sees the mind's basic operation as the handling of sentence-like items. Its overall architecture is that of an axiomatic structure of general principles descriptive of reality; this is the "knowledge base" of the machine, from which it deduces more particular bits of information by means of a battery of deductive and inductive principles of reasoning. By contrast, the connectionist approach does not deal in representations at all: We do not have to attribute to the model any such features. The procedure may be described as inductive, at two levels: First, the thought processes modelled are themselves "inductive" in the sense that they move from particular inputs to general "conclusions" – more correctly, to a general capacity of the device to produce an output of an abstractly specified kind. Secondly, the modelling process in itself inductive, what we might call "approximative induction", consisting in the repeated and systematic variation of the transmission frequencies of the interlinked nodes until the desired output is attained, largely on a trial and error basis.

This brief sketch of the development of AI thus stresses the contrast between a rationalistic approach (often sentimentally referred to as Good Old Fashioned Artificial Intelligence, GOFAI) versus the more inductive approach of connectionism. There are other lines of development, such as Dynamic Theory, but the first-mentioned two have been particularly influential. At any rate, they will offer convenient points of comparison later, when we come to examine how the network metaphor also is a powerful idea in Science Studies.

13. It is significant that cognitive science has coexisted quite peacefully with philosophy; indeed, philosophers actively contributed to its progress. An important contribution was made by Jerry Fodor, a student of Chomsky's, who epitomized the rationalist trend to which he gave a particularly poignant formulation in a book

entitled *The Language of Thought* (Fodor 1975). In it, he defends the thesis that cognition must indeed be construed as operations upon sentence-like entities; not, however, those of a natural language, but of an antecedently existing one, the mind's own "machine-code", as it were.

Not surprisingly, given his behaviourist leanings, Quine has inveighed on the other side (Quine 1972). There were further dissenting voices among the philosophers, one of the most vocal being Hubert Dreyfus (1972). To some extent, these discussions represented disagreements within philosophy as to the nature of cognition, which were now projected unto the canvas of cognitive science. Debates that had been going on in philosophy for some time could now be discussed more fruitfully in this new arena; thus, Dreyfus drew heavily upon insights deriving from German and French philosophy, in particular Heidegger. At any rate, as the cognitive scientists saw matters, the objections of philosophers did not undermine the whole enterprise, but rather provided inspiration and guidelines for further developments.

Another possible line of opposition is defined by the distinction between a factual and a normative approach. The traditional philosophical treatment of cognition had been squarely normative, trying to infer how the human cognitive apparatus must be structured if it is to be capable of giving us knowledge of reality. The cognitive structures postulated were typically such as follow from the normative stipulation. This is a characteristic feature of both rationalist and empiricist epistemology; we found the same trend among logical positivists.

This is the context in which Quine's call for a naturalized epistemology was proffered and in which its significance can be seen. As noted above, the empirical investigation of human thinking within a cognitivist framework was already well established in 1969, the year Quine's article appeared, and it hardly needed Quine's advocacy, at least not within the scientific community. The importance of Quine's admonition is that it paves the way for a collaboration between philosophers and cognitive scientists on the norm/fact issue. His article served as a call for philosophers to give up their a priorist, normative pretensions and to just observe how cognition is actually accomplished.

This call has had a wide following in Anglo-American epistemology. The empirical turn is accommodated in what are referred to as *externalist* theories of epistemology, which hold that the features bestowing the status of knowledge upon a particular cognitive process are not all intrinsic to that process, that is, given to the knower's consciousness. Rather, they may be extrinsic, such as precisely those features which make our sensory system, including the brain, a dependable source of information of the external world, but of which even the gross features are unknown to the average person. This represents a turn away from the venerable Cartesian tradition in epistemology, which insists that the knower must be able to cite the grounds for his knowledge. This is a move that many epistemologists were quite ready to take in any case, since the Cartesian route seems inevitably to lead to scepticism.[6]

The most significant versions of externalist theorizing are so-called *reliabilist* theories of knowledge, in which a belief is allowed to count as knowledge if it is acquired through a channel that is generally reliable, meaning that it produces beliefs with a high proportion of truths (Goldman 1986). Among such reliable channels are our senses, our memory and our simple procedures of reasoning, even though the knower may in no way be able to cite the statistics that would support this claim. This conforms with our intuitions, since we would indeed be willing to attribute knowledge to a young child concerning the objects that are in plain sight in front of him, even though that child would not be able even to comprehend questions as to the general reliability of the senses.

Reliabilist and other externalist theories of knowledge leave a blank in our definition of knowledge – or rather of its specific sub-kinds, such as sensory knowledge, memory knowledge, and others – to be filled in by empirical investigation into the cognitive mechanisms at work, pinpointing those features that make them capable of producing veridical information in a reliable manner. In other words, it leaves room for precisely the kind of information that cognitive science sets out to find. Thus, a broad field of opportunity for close collaboration between epistemologists from the philosophical side and empirical workers within cognitive science has opened up here. This collaboration has continued to the present day and has borne rich fruits.

Chapter 2
Wittgenstein, Kuhn and the Turn Towards Science Studies

1. We now turn to developments that would eventually usher in a programme of naturalistic investigation of science, under the name of Science Studies. When Quine presented his case for a naturalistic epistemology and a naturalistic philosophy of language, an alternative to both had been available for some time in the writings of Ludwig Wittgenstein. Wittgenstein's naturalism is less explicit, however; indeed, some of the naturalistic elements of his thought exist only in very embryonic form and must be extrapolated from his writings. Besides, Wittgenstein's naturalism is not scientistic; indeed, Wittgenstein was highly critical of scientistic attitudes.

The deepest source of Wittgenstein's naturalism, like Quine's, was disaffection with Carnap's philosophy of language. To Carnap and early Wittgenstein alike, philosophical problems of logic and of natural language were intimately linked and the former were held to represent these common issues in a particularly stark form. Indeed, in the two philosophers' earliest works, the problems of natural language *were* the problems of logic, since natural language is held to be a pure logical form shrouded in the cloak of articulated sounds or written signs; this thesis is particularly explicit in the *Tractatus*. By the mid-1930s, however, Carnap and Wittgenstein had both moved away from the logical absolutism of the early works, and the notion of convention now took centre stage in their understanding of logic and natural languages. Carnap expressed this in his famous Principle of Tolerance, touched on previously: *It is not our business to set up prohibitions, but to arrive at conventions* (Carnap 1937, § 17; italics in original).

This principle had a clearly delimited scope, however: Carnap still believed that logic imposes an independent constraint on our reasoning practices in the sense that, *once* a set of axioms and rules of inference are laid down (both by pure convention), *then* there is no flexibility in the way we must reason, inside that formal framework. We may, as it were, lay out the rails of logical convention in any direction we choose, with all the twists and turns that are needed to get us where we want to go; but once the rails are in place, they constrain our motion inexorably as we move along (*ibid.*).

In his celebrated rule-following considerations in the *Philosophical Investigations*, Wittgenstein is concerned to repudiate this notion. He makes his point with a famous example picked from what might otherwise be thought to be the absolutist's strongest ground, that is, mathematics. A pupil is asked to

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continue a sequence of numbers, 2, 4, 6, 8, 10, 12, written on the blackboard by the instructor. Wittgenstein first shows that there is no absolute or unique notion of "sameness" that would render any particular response on the part of the pupil the uniquely correct way of "continuing the series in the *same* manner". No matter how eccentrically the pupil proceeded in chalking up numbers on the blackboard, we could always describe this as "doing the same thing", under *some* interpretation of what went before. The obvious suggestion now is to define the correct continuation of the series as that which the teacher intended. But which way is that? Do we assume that the entire expansion of the number sequence was somehow already present in the teacher's mind? Hardly, since the latter is finite while the mathematical expansion is infinite. The whole extension of the series is not represented in the teacher's mind, nor is anything found there from which the extension follows by implication. In either case, the correct application would be fixed as *the same* as that which was immediately given. But, as we have learnt, appeals to "sameness" are impotent: Anything can be shown to be "the same" as anything else. The final upshot, according to Wittgenstein, is that the "correct way" is defined as that which the community would adopt. Wittgenstein concludes, or so it is commonly thought, that nothing firmer, nothing more robust, upholds this operation than a social consensus.

There is considerable controversy about the correct interpretation of the rule-following considerations and their implications. The main line of division goes between Saul Kripke, on the one hand, and Gordon Baker and Peter Hacker and a number of further interpreters on the other (Kripke 1982; Baker and Hacker 1984, 1985). Kripke's interpretation, often referred to as the "communitarian" reading, stresses the role of communal agreement in Wittgenstein's argument, to the extent that the correct application of a notion is actually defined as that on which the community agrees, or on which there is consensus in the community. The rival interpretation, while granting the importance of conformity, shies away from this strong conclusion, referring among other things to Wittgenstein's denial (*op. cit.*, § 241) that human agreement decides what is true and false. It is less clear, however, what the alternative position amounts to, in positive terms; it is mainly defined by its opposition to the communitarian view. We may happily bypass these exegetical matters here and merely note that the communitarian interpretation, or something very close to it, has been highly influential in philosophy and in social science. In particular, it serves as an important prop for Science Studies.

From the analysis of such paradigm cases of rule following, implications immediately spread out to all other practices where rules are involved – that is to say, to every single human activity. There is no privileged, philosophical standpoint from which particular social practices may be evaluated, nor may they be criticized from the point of view of other everyday practices; that would be like criticizing soccer for not being ice skating. The platform from which a critique or reconstruction of natural language and practice could be launched is not available. Such a project would presuppose an absolute, universal notion of theoretical and practical rationality – a benchmark against which extant human modes of thought and action could be measured and possibly found wanting. But such notions are chimerical, according to Wittgenstein. This means that philosophy's prerogative, exercised for two and a half millennia, of critiqueing existing human practices, has to be given up at last; in the future, philosophy can only *describe* such practices.

Thus Wittgenstein's naturalism springs from his insistence that philosophy must abandon its critical, judicial pretensions: He declares that philosophy "may in no way interfere with the actual use of language; it can in the end only describe it" (*op. cit.*, § 124). "Interfere" here means criticize or legislate for. The scope of this injunction is broader than appears from the quote: For Wittgenstein, languages are intimately interwoven with practices to form *language games*, which together form entire *ways of life*. Thus the ban on interference with language is really a ban on interfering with human practices; these have to be taken at face value and just accepted. ("What has to be accepted ... is ... *forms of life*" (*op. cit.*, p. 226).

2. We need to consider a further aspect of Wittgenstein's naturalism that will be important later. His position is explicitly *anti-scientistic*. Unlike the scientific approach, his is purely descriptive; Wittgenstein insists that philosophy must not only abstain from "interfering with" anything, but must not try to *explain* anything, either (*op. cit.*, § 109). What Wittgenstein has in mind in this quote is primarily *philosophical* "explanation", that is, the elucidation of a multiplicity of philosophically opaque phenomena by reducing them to underlying principles; the way that Wittgenstein himself, in the *Tractatus*, had explained the nature of thought, meaning, language and logic by means of the picture theory. But he clearly also holds that we are not after *scientific* explanation, either: "It was true to say that our considerations could not be scientific ones" (*ibid.*); thus late Wittgenstein agrees with his younger self speaking in the *Tractatus*. This anti-scientism undoubtedly in part reflected the conservatism inherited from contemporary *Lebensphilosophie*. But, as we shall see later, it had more internal, systematic sources as well.

In the *Philosophical Investigations*, Wittgenstein applies the methodology developed in the initial part of the work to a number of problems, chiefly in the philosophy of language and the philosophy of mind. All spring from a common misconception: that language refers primarily to certain inner occurrences investigated by the philosophy of mind that, repackaged as "meanings", constitute the contents that language communicates. The cure for this ill is to view language as a public practice in which meanings are constituted through consensual action. This goes even for the psychological vocabulary, the referents of which are conceived not as ghostly inner occurrences but as aspects of the very same public practice. In all of this, Wittgenstein's method is just to describe, not to theorize, simplify or reduce.

According to Wittgenstein, language is a public practice, based upon propensities of reaction that are part of the "natural history" of man. It is a fact of human nature that, upon presentation of paradigmatic instances of objects of various categories, human beings will readily recognize other instances of such things and react to them in similar ways, among other things by applying the same designation to them that they learnt when confronting the paradigm. It is a brute fact that people generally develop similar patterns of reaction when exposed to such drill. This does not rule out occasional disagreement; in such cases, communal consensus settles the right way to proceed, and hence defines the rightful meaning of the term. (We still adopt the communitarian interpretation here.)

It is instructive to compare the above story with Quine's picture of language. Like Wittgenstein, Quine has grave reservations concerning the usefulness to philosophy of the concept of linguistic meaning. Like Wittgenstein, he rejects the notion that language deals in private items, "ideas" or inner "meanings"; the belief in such entities is branded as "the Museum Myth" (Quine 1969a, p. 19, p. 27 f), a counterpart to Wittgenstein's metaphor of the "Beetle in the Box" (Wittgenstein 1953, § 293). And finally, like Wittgenstein, he basically reduces individual language use to a pattern of verbal dispositions inculcated in the individual. The difference resides in the role that Wittgenstein attributes to communal agreement in language use. For Wittgenstein, there is no distinction between a correct and an incorrect usage, and hence no *meaning*, unless we appeal to communal agreement; strictly idiolectic meaning – such as would be possessed by a purely private language – is no meaning at all. Quine, on the other hand, has no scruples about idiolectic meaning once meaning is pared down to its stimulus core; strictly speaking, every individual has slightly different propensities to utter particular sentences upon sensory prompting and hence speaks his own idiolect. This raises no metaphysical problems, nor does it compromise communication, as long as these idiolects largely overlap. In the final analysis, this difference reflects Quine's adoption of a purely causal construal of meaning (under the name of "stimulus meaning"), whereas Wittgenstein makes it clear that meanings determine use in a noncausal manner.

3. In other, related works, *Remarks on the Foundations of Mathematics* (Wittgenstein 1967a) and *On Certainty* (Wittgenstein 1969), Wittgenstein extends the same treatment to the realms of mathematics and cognition, respectively; in these contexts, he thus practices naturalistic (descriptive) philosophy of mind, naturalistic philosophy of mathematics (including the epistemology of mathematics) and naturalistic epistemology with respect to empirical knowledge. There is next to nothing, however, that might be called a philosophy of science in Wittgenstein's writings; still, it is possible to extrapolate from Wittgenstein's general epistemology and philosophy of mathematics a guess as to what such a doctrine might have looked like.

In Wittgenstein's epistemology, the notion of "hinge sentences" plays a crucial role (Wittgenstein 1969, §§ 341–342, also §§ 83, 87–88, 95–97). Hinge sentences serve as reference points in cognitive space, helping to structure the edifice of human empirical knowledge. They are distinctive in being immune to refutation, a feature giving them the status of fixed points around which the rest of empirical knowledge revolves. They form the counterpart, within empirical science, of the theorems of mathematics that also serve as resources for the organization and expansion of knowledge. Both are a species of sentences; however, they are not merely sentences, or sets of such, but sentences *viewed in the context of a practice* in which they are put to work. The fixed points of mathematics, according to Wittgenstein, are not just the theorems, but also the proofs that bestow this privileged status upon them; and this power exists only within the particular, rather esoteric, social practice we call mathematics.

If we transfer this view to the philosophy of science, the role of fixed points would be served by scientific laws and theories; these, however, must not be construed as bare sentential structures, but would be considered in the context of those achievements, in the form of successful experiments or explanations, to which they owe their canonical status. This would involve the consideration of details of the practices, instrumental design and ways of organizing experiments in terms of which the abstract laws were brought into contact with reality. Thus, scientific practices revolve around clusters consisting of exemplary achievements, instrumental practices, theories and laws (cf. Wittgenstein 1969, §§ 167–170, 292–298).

Whereas common sense knowledge is fairly static and does not constantly strive to expand its scope, science is different: as a social institution, it is defined by its dynamic nature. It is part of the stereotypical conception of science as being essentially progressive, which makes it differ not only from common sense, but also from such institutions as religion, which see themselves as guardians of God's sacred word, or of an inheritance of traditional lore. Still, there are alternative ways to construe the scientific stance: Popperians, for instance, would see science as the very embodiment of the willingness to critically overturn everything we have held true in the past. To a conservative like Wittgenstein, by contrast, the dynamics of science would be construed as the attempt to extend and articulate what we know already, while still preserving the authority of past achievements. This applies in particular to the canonical elements in scientific theory and practice which are the counterparts of the "hinge sentences" of everyday thought and of proofs in mathematics: We might expect to find substantial efforts to extend these results to novel but related fields, but we might expect even stronger efforts to defend them whenever they are threatened by recalcitrant experimental results or heretical interpretations.

Even a conservative like Wittgenstein, however, would have to concede that, within the scientific "way of life", changes – even profound ones – occasionally occur. Given Wittgenstein's insistence that rationality is always local, and always intrinsic to a particular social practice, such changes could not be construed as rational, since this would presuppose the existence of some sort of meta-practice, at a level above particular "language games", within which the change could take place. Wittgenstein rejects the idea of such practices or institutions, however. Instead, profound changes in science would have to be construed along the same lines that are adopted in other realms of social life; for example, as akin to *revolutions* in the political sphere, or to *conversions* among religious believers. Such changes are characterized by their abrupt and radical nature, with no smooth transition from the former state to the new one. Moreover, they only occur once internal difficulties in the existing societal institutions have reached intolerable levels, since only in this situation can the inherent conservative tendencies of the prevailing "language game" be overcome.

Wittgenstein hints at what such a total "conversion" would involve within the cognitive sphere: It would have en impact all the way down from the theoretical level of science to the observational parts. For observing or seeing for Wittgenstein is always "seeing-as", of a kind explored by Gestalt theory and illustrated by the notorious "duck-rabbit" (Wittgenstein 1953, p. 194, 1969, §§ 291–293). Hence, people would experience the world differently after a scientific revolution, where they would suffer a "Gestalt switch".[7]

4. The reader, I hope, will already have recognized the picture that emerges as largely similar to that offered by Thomas Kuhn in *The Structure of Scientific Revolutions* (Kuhn 1962/1970). Indeed, Kuhn is explicit in registering his indebtedness to Wittgenstein in this work; however, it seems that Kuhn articulates a Wittgensteinian point of view in a much deeper sense than he himself recognizes. As Kuhn sees it, that influence resides narrowly in Wittgenstein's theory of concept acquisition. This is Wittgenstein's doctrine of ostensive learning, to the effect that we learn concepts by being confronted with typical exemplars of the concept's extension. It seems as though Kuhn developed, of his own accord, a philosophy of science that closely conforms to what a Wittgensteinian philosophy of science would have looked like.

The "hinge sentences" and the social context of their acceptance are evidently closely similar to Kuhn's "symbolic generalizations" and the "exemplars" that explicate and support them, which, together with a couple of additional elements, go to constitute Kuhnian paradigms (Kuhn 1962/1970, p. 182 ff). The paradigms, considered as social practices bound together by epistemic norms, are close kin to Wittgenstein's "language games". Also, the methodology is Wittgensteinian: Kuhn's study is largely descriptive, or, more precisely, descriptive-historical. It is so for the very reason that Wittgenstein constantly impresses upon us; that is, because anything further is forbidden. Here, as elsewhere, all we can do in approaching a "language game" is to describe it, not legislate for it nor criticize it. Every established language game is "in order as it is". This is precisely the stance adopted by Kuhn with respect to scientific practice, much to the chagrin of some of his commentators and critics (cf. Feyerabend 1970). Kuhn does not explicitly denounce efforts to reform science, but there are clear hints that any attempt to rationalize and improve upon scientific practice would be futile. Each discipline defines its own language and has its own standards of proper procedure.

So in a sense there is already a Wittgensteinian naturalized philosophy of science on offer, namely, the one laid out in *The Structure of Scientific Revolutions*. (It could be supplemented, on various points of detail, with elements from Russell Hanson's *Patterns of Discovery*, Hanson 1958.) Due to the immense impact of Kuhn's work, ideas that are basically Wittgensteinian thus have played a crucial part in dismantling the logical positivist hegemony on philosophy of science and so paved the way for the more direct use that we shall find in Science Studies.

The powerful influence of Kuhn's work, of course, did not merely spring from its provision of a picture of science visibly truer to life than the "rational reconstructions" offered by logical positivism, or by the heroic falsificationism of Karl Popper. The ground was well prepared by the gradual collapse of logical positivism considered as a purely *prescriptive* programme within the theory of science. The logical positivist model of science was built upon two fundamental elements: a basis of "given" and certain empirical data, recorded in "protocol sentences", and a battery of inductive procedures wherewith to extract (or justify) hypotheses on the basis of such evidence. On both counts, positivism came to grief. No agreement was ever reached concerning the formal properties of the basic observational language of science. Moreover, despite energetic efforts, in particular by Carnap (1950, 1952), the positivists never succeeded in devising a viable inductive logic – that is, one that would combine formal rigour with the absence of fatally counterintuitive consequences.

Stringent criticisms of these weaknesses of positivism had been voiced by Karl Popper as early as the 1930s.[8] Popper went on to offer his own alternative system, which "solved" the problem of the empirical basis by a conventionalist turn, and dissolved the problems of the inductive procedures by dismissing inductivism altogether, replacing it by the method of conjecture and refutation. However, these proposals were found to be open to much the same objections: The method of conjecture and refutation could not be regimented into a rigorous formal procedure, since no algorithm can be found for deciding which element to put the blame on when a cluster of theory, auxiliary hypotheses and empirical assumptions is found to clash with observations. Moreover, Popper largely shared the logical positivists' narrow focus upon the abstract theoretical aspect of science, to the neglect of its instrumental and organizational side, and showed little interest in the history of science beyond anecdote.

Thus, Kuhn could build his new theory of science on the ruins of Logical Positivism and Popper's "Critical Rationalism" after they had collapsed, largely as victims of their own structural weaknesses. In Kuhn's theory, the classical philosophical ambition to develop an exact, formalized model of scientific theories and a stringent canon of scientific rationality was finally given up. Focus was turned away from the products of the scientific enterprise – that is, theories – and towards the very process of scientific work itself. Moreover, much more attention was paid to details of the history of science.

5. The stage was thus set for a programme of naturalizing the philosophy of science along the lines suggested by Kuhn. Its guiding principles would be those that could be extracted from Kuhn's work, as represented above. And there were indeed inchoate attempts in this direction, for example within the school sometimes referred to as "History and Philosophy of Science" (which actually antedates Kuhn). These efforts received added inspiration from other significant figures, such as Stephen Toulmin and Norwood Russell Hanson (Toulmin 1953, Hanson 1958); both, interestingly, were influenced by the work of the later Wittgenstein.

However, at this juncture, events would take a different turn. The legacy of Kuhn within History and Philosophy of Science was eclipsed by another movement with a much more radical agenda – although even this school would refer to Kuhn as its inspiration. This is the point in our story at which Science Studies finally appear on the stage. To understand why Science Studies would emerge at precisely this time and how their agenda would interact with the long term trend of naturalization, we have to examine certain elements of the contemporary political and cultural scene that helped provoke its emergence. I am referring to certain widespread misgivings in Western democracies during the post-war decades concerning the role of (natural) science in society, and to sentiments that this relationship harbours tensions that are commonly overlooked. This attitude resulted in various attempts to develop principles for a better accommodation between science and society.

In Britain, this movement had many contributors, each with its own, slightly different, agenda.[9] One particularly influential idea was articulated by C.P. Snow in the famous pamphlet, *The Two Cultures and the Scientific Revolution* (Snow 1959). In this text, Snow claims that a chasm separates natural scientists and humanist scholars in Britain, a chasm that springs mainly out of mutual ignorance, but with undertones of mistrust and resentment. The ignorance was mainly laid at the door of the humanist scholars, whose resentment of the other side also ran deeper; but the distrust and suspicion was mutual. Snow urged the importance of reaching an accommodation between the two "cultures" and saw himself as well-positioned to bridge the gap by virtue of his double background, which combined a training in natural science with the authorship of a large literary output, including the 11-volume *Strangers and Brothers*. Still, Snow's formula of accommodation clearly favoured the science side. The major steps towards reconciliation would have to be taken by the humanists, who would have to overcome their Luddite disdain of science – or at least try to learn what it was all about. The accommodation on the science side would consist in a clear recognition of science's social responsibilities, primarily its obligation to serve as an agent of prosperity in society. As Snow saw it, this included not only a concern for increased total welfare (to use a slightly anacronistic term), but also for its just distribution in society. In particular, Snow was concerned about the unfair distribution of wealth viewed in a global perspective and urged that resources be transferred to developing societies in the Third World.

Other players on the British scene shared Snow's basic agenda, but adopted a more pro-science stance and articulated views of a more clearly socialist orientation on science's role in the economic and industrial policies. An important figure was John Desmond Bernal, a prominent crystallographer and dedicated socialist. Already prior to WWII, Bernal had published a book, *The Social Function of Science* (1939), that presented an analysis of science's societal role and laid out principles for a science policy that would transform science from being a servant of the capitalist class and of supplier of sophisticated weaponry to the military, to being an agent of general prosperity. To Bernal, the answer lay in strong societal governance of science and in central planning of industrial production.

With the outbreak of WWII, Bernal got the opportunity to implement some of his ideas of the social governance of science, although at the expense of his wish to break the alliance between science and the military. He powerfully advocated a strict reorganization of science to make it an effective instrument for the war effort and himself served as a scientific adviser to top military echelons; he was even involved in the planning of the invasion of Normandy.

After the war, Bernal remained highly influential within British science policy. He articulated a science policy for the postwar era in which the idea of science as an agent of prosperity could once more be pursued. The orthodox Marxism of Bernal's interwar policies was moulded into a more pragmatic socialism, resulting in an agenda that found a receptive audience in the Labour government after the war.

There were voices on the opposite side as well, however. One of the loudest ones was Michael Polanyi, a refugee to Britain from communist Hungary and a vehement critic of socialist planning of science, the results of which he had witnessed during travels in the Soviet Union in 1935. He saw such efforts as a threat to science's deepest values, leading inexorably to such disasters as the Lysenko affair. He was appalled at the prospect of similar policies being pursued in his adopted country and attacked them in publications with titles such as *The Logic of Liberty* (1951) and *Personal Knowledge* (1958) and from the platform of an organization of which he was a co-founder, The Society for Freedom of Science.

The voices in favour of a social management of science remained dominant among the science policy elite, however, although the efforts to translate the ideals into concrete institutional policy suffered a temporary setback when the Conservatives took over in 1951. They were resumed in 1964 when Labour returned to power under Harold Wilson. Inspired by Snow's ideas, as put forth in *The Two Cultures*, and with concrete strategies supplied by two influential reports on science and technology and their manpower requirements in a future Britain, the new Labour government started to implement the needed reforms. Among them was the establishment of a number of so-called Science Studies Units. These academic institutions were inspired by Snow's idea of an accommodation between science and society that would make the former an instrument of socially defined goals, subjecting them to a social control that would dissolve science's traditional claim of autonomy. In return, there would be a steady flow of economic resources for science's future expansion, plus a push to suppress the deeply ingrained hostility toward science and technology that was widespread in Britain's elite educational institutions.

The academic institutions that would eventually result from these efforts would differ widely among themselves and would pursue rather different agendas. Some, such as the institute in Sussex, would pursue rather rigid strategies of science policy. Others would adopt a more research-oriented, purely theoretical approach. Among these was the Science Studies Unit at Edinburgh, which will become a focal point in our story.

6. Before I go on to tell that story, however, I shall briefly mention how, outside of Britain, parallel endeavours to achieve a better accommodation between science and society took a completely different form. Here, we can highlight the fortuitous nature of the confluence of those endeavours and the trend towards naturalization of philosophy, for in another national arena, that of post-war Germany, philosophy actually served as the instrument through which this sentiment expressed itself, at least on an academic level. The Frankfurt School is the most important exponent of this phenomenon.

Like its British counterpart, the post-WWII debate in the Frankfurt school had roots going back to the interwar period, namely to the work of the Institute of Social

Research. Both national traditions largely defined themselves in Marxist terms; but whereas the main protagonists in Britain had a professional background in natural science, which they combined with fairly orthodox socialist politics, the main figures on the German scene were social scientists and philosophers deeply steeped in Marxist thought, but intent on overcoming its ostensible theoretical limitations. Among these limitations was Marx's infatuation with natural science and the productive powers embodied in it, especially once they were liberated from the shackles of the outdated relations of production inherent in capitalism. Max Horkheimer had denounced this "scientistic" tendency in Marx (albeit even more emphatically its positivist counterpart) as early as the late 1930s and the point was made even more forcefully in the book he co-authored with Theodor Adorno during the war, later to become influential under the title *Dialectics of Enlightenment* (Horkheimer and Adorno 1972). Marx was mistaken in seeing science and technology as inherently liberating, a potential that was unfortunately distorted in capitalist society, but would be realized in the future communist society. To Horkheimer and Adorno, technology with its inherent "instrumental reason" will tend to produce a technological consciousness that is itself repressive. Where Marx's technological determinism reflects an attitude of optimism, which was inherited by the British Marxists of the interwar and immediate post-war period, the early representatives of the Institute for Social Research looked upon the forward march of science with grave reservation and concern.

Among the post-war generation of "critical theorists", these concerns received an especially eloquent expression in the work of Herbert Marcuse. In his philosophical best-seller, *One-Dimensional Man* (Marcuse 1964), Marcuse strongly denounces "technological rationality", the instrumental stance that constitutes the essence of scientific thought and leads to an "instrumentalization" of Man himself. Rather than being an agent for the liberation of Man, science becomes a tool for his domination. Yet we have to turn to another Frankfurt School representative, Jürgen Habermas, to get a more cool-headed, systematic diagnosis of the pathologies befalling the complex relationship between science and society. Habermas's analysis, laid out in *Knowledge and Human Interests* (Habermas 1971, German original 1968), is articulated within the framework of a philosophical system combining elements from Marx, Peirce, Gadamer and Freud, that is, the theory of human cognitive interests. There are three: the technical, the practical and the emancipatory. Among these, the emancipatory interest is supreme, with the other two being in a sense its derivatives, since they define the principles in terms of which the goals of emancipation must be achieved by a being – Man – who is a material organism living in a material world, but also essentially a social and historical being. The goal of emancipation is not posited as a self-evident normative axiom by Habermas, but is instead inferred from man's rational nature. Rationality, according to Habermas, embodies an inherent striving towards self-realization. In practical terms, this means a push towards removing the material and social obstacles for the manifestation and perfection of human reason. The tools for this process are twofold. First, they are implements for the handling of material reality, originally in the context of crafts, in the modern world primarily in the form of scientifically-based technology. This kind of activity is impelled by the *technical* interest. Secondly, they are the tools whereby we understand our contemporaries as we engage with them in social interaction and also understand our predecessors when we enter into a dialogue with the literary and cultural tradition. At the same time, we also achieve a better understanding of ourselves; all of this is the concern of the *practical* interest, the tool of which is Hermeneutics.

From these deliberations follows a formula for the proper role of the sciences in society, *qua* instruments for the ultimate goal of all political activity, that is, the full emancipation of man. This implies, in the first place, the elimination of man's servitude to natural necessity and the overcoming of scarcities of material goods. Next, it implies the elimination of man's servitude to man, i.e. his liberation from any societal, man-made constraints that are not strictly required for peaceful social interaction. Unfortunately, there is a tendency for the social order to embody an excess of repression, generating inequalities and producing a society divided into classes. Similarly, there is a tendency for material technology to influence the ways in which society is organized, not only in the sphere of production – where Taylor's "scientific management" is the notorious early example – but even in public administration, education and the health sector. These ills are all branded as "positivist", in a rather vague, pejorative sense that covers, among other things, the adoption of strict means-end rationality in all spheres, an insistence on quantification and measurability of all phenomena, and application of a misplaced ideal of "objectivity" that is alien to the human sphere; these tendencies combine to produce a behaviourist reduction and diminution of man.

According to Habermas's theory of "knowledge-guiding interests", natural science stands at the service of man's control of nature, which involves, *inter alia*, contributing to his material welfare – an agenda that his British counterparts would gladly underwrite. Furthermore, the scientific mode of thought must be carefully contained within the sphere that is proper to it; it must not be allowed to spread to the way in which society deals with its subjects, or to the everyday "Lebenswelt". Human interaction is governed by a completely different kind of reason, "communicative rationality", which unfortunately is under pressure from societal "systems" operating according to a different logic. These ideas would only be fully developed in Habermas's monumental work from 1981, *Theorie des kommunikativen Handelns* (English translation *Theory of Communicative Action*, 1988). The spread of modes of thinking originating in natural science and technology transforms politics into technocratic management, a dangerous trend that must be exposed and opposed. In the light of such worries, the enthusiasm for science and technology characteristic of the British post-war science policy establishment would clearly be rejected as naive, and as reflecting a shallow sociological understanding of the forces at work.[10]

It is interesting to note, in passing, how the picture of natural science provided by Habermas's highly abstract reconstrual of human cognition faithfully reproduces the idealizations of logical positivism, his rejection of the latter notwithstanding. There is unquestioning subscription to the conception of scientific theories as built upon a basis of brute data, couched in a language with a precisely defined syntax to which semantic content is afterwards assigned, a feature that makes scientific language use perfectly non-contextual and hence "non-hermeneutic". Habermas has no objection to this picture as such, only to its transferral to the human sphere, where it allegedly leads to behaviourist reductions.

True to the national intellectual tradition, the way that the problems surrounding science's societal role were handled on German soil was thus characteristically speculative and transcendentalist. In a quasi-aprioristic fashion, man's cognitive activities were organized into a neat, tripartite system in which a clearly defined domain of legitimacy is assigned to scientific rationality and associated modes of thought and practice, set over against the "interpretative" practices involved in human interaction. From this would follow the gross outlines of the institutions and practices to which the production and use of scientific knowledge in society could be entrusted.

7. In the Anglo-American world, the ideological and intellectual challenges posed by the forward march of techno-science would be addressed in a completely different manner, but in equal conformity to national intellectual traditions. Here, the normative stance is much more pragmatic and political, in the everyday sense of the term. Moreover, the strategy adopted is not based upon an a priori deduction of the defining features of science, but instead involves an empirical, naturalistic approach. The idea is that a sober investigation of science, using science itself as a tool, will show that science is from the outset totally immersed in society and deeply interwoven with societal interests and that the picture of science as something elevated above society is a carefully cultivated fiction, behind which the interests currently being served by science may conveniently operate. The goal is to dispel the fiction and to make science accountable to a much broader range of interests than those currently monopolizing it.

The tools available for this undertaking were already laid out in previous sections: They are Wittgensteinian naturalism, both as it appears in Wittgenstein's own writings and in Kuhn's tacit appropriation of these ideas in his interpretation of science; Kuhn is indeed a guiding light for Science Studies.[11] However, Science Studies want to go beyond Wittgensteinian naturalism and even beyond Kuhn. To the extent that a Wittgensteinian would grant any role to empirical research in the investigation of science, beyond the simple quasi-anthropological description of scientific practices as we find them today, the science of choice would have to be *history*. It would be a *historical narrative* depicting the shifting practices that have counted as scientific; this is indeed largely what we find in Kuhn. Contrary to the stereotypical reading of Kuhn, there is no endorsement in Kuhn of a significant role for systematic social disciplines such as sociology or economics to explain the development of science. Kuhn is actually an internalist, as indeed you must be if you are a Wittgensteinian. Science Studies, on the other hand, are looking for a naturalistic approach to science in the more radical sense of a *scientific* study of science, investigating science with tools provided not by history, but by the generalizing, nomothetic social disciplines, and with systematic generation of research data. This is something that Wittgenstein would strongly oppose, since science is a language game on a par with others and thus cannot be elevated to a meta-position with respect to any of them – including itself.

8. To make this daring step, however, STS'ers not only had to transcend their philosophical mentors, but had to wage a battle even on their disciplinary home turf. They had to make a radical break with the dominant tendency of contemporary sociology of science and its undisputed leader, Robert Merton[12] Merton recommended a methodological stance that would allow sociology of science to live peacefully alongside philosophy of science, accepting a division of labour between the two whereby philosophy would develop normative models of science, while sociology deals with science, or particular aspects of science, as it is actually conducted. Sociology had even learned to live with the "rational reconstructions" of science developed by positivists, with their mongrel status between normative and descriptive. According to these rational reconstructions, successful science is insulated from social influences, being largely generated by the interaction of the external world, as it presents itself to us in sense perception, with universal, rational principles of theory construction (such as Carnap's inductive canons). These principles are supposed to be universally valid and not subject to compromise with local standards. Hence, science is to be explained by reference to universal rational norms and to the impacts of the external world. Merton tried to articulate, at the most general level, the rational norms by which science defines and demarcates itself as set apart from the rest of social life. These are the famous CUDOS norms that comprise Communism, Universalism, Democracy and Organized Scepticism.

In accordance with the division of labour outlined above, Merton restricted sociology of science to examining the societal background conditions that led to the emergence of science and remain conducive to its progress, as well as the internal institutional organization and the social norms of science. Still, he was far from naive with respect to the complexity of the relationship between science and society, as he was fully aware that the emergence of (natural) science as an institutionalized enterprise is not a social universal, but indeed highly dependent upon historical contingencies, including cultural orientations. His doctoral dissertation linked the emergence of science to the rise of Puritan values (Merton 1938). He also insisted that the roots of science reside in practical interests; thus, he pointed out how, even in science as an established institution, group interests and other societal factors contribute to defining what he called the "foci of science", that is, the areas singled out for investigation. Finally, he pointed out that the CUDOS norms are not disembodied, Platonic principles, but are socially enforced in the scientific community. Still, Merton's approach embodied a fundamental restriction: he renounced any attempt to explain the very content of true scientific theories in sociological terms. It was this stance that STS'ers had to overturn.[13]

9. In this chapter, we have examined some of the elements that went into the complex intellectual, political, economical and cultural context within which Science

Studies emerged: the demise of idealized models of science of positivist or Popperian origin, the emergence of a new style of naturalism in Wittgenstein's late work, Kuhn's theory of science, various ideological and political concerns with respect to the accommodation between science and society, and the decision of the British government to channel such concerns into an institutionalized framework. All these elements came together in the formation of the Science Studies Unit at the University of Edinburgh, which was to became the birthplace of the first organized effort within Science Studies, the so-called Strong Programme in the sociology of knowledge. This will be the topic of the next two chapters.

Chapter 3
David Bloor and the Strong Programme

1. The Science Studies Unit at the University of Edinburgh was established in 1966 and, as its first director, the university appointed David Edge. Edge had been trained as a radio astronomer, but had abandoned a career in science in order to work for the BBC, where he produced programmes introducing a wider audience to science, including ongoing debates about science and society.

Among the first generation of senior researchers hired to the Unit were David Bloor and Barry Barnes. With Bloor as its chief spokesman, the Unit in following years would formulate a research agenda known as the "Strong Programme" in the sociology of knowledge (Bloor 1976). The Edinburgh School (as it is commonly known) and its research programme forms a natural point of departure for our story about Science Studies, both from a historical and a systematic perspective. Later representatives of STS would routinely express their gratitude for the inspiration received from the pioneering efforts of the Strong Programme. Moreover, even though many of these authors would later deviate significantly from the path laid out by Bloor and his collaborators at Edinburgh, especially with respect to the methodology adopted, their undertaking would still be significantly shaped by the principles laid down by Bloor.

Before we proceed, an issue of scholarship needs clarification. Although the Strong Programme does indeed constitute a definite body of doctrine, set out in a number of explicit tenets, there is considerable diversity among its best known figures – David Bloor, Barry Barnes and Steven Shapin – with respect to the argument supporting those tenets; this applies even more to its later generation of adherents. Lack of space here prohibits examination of the individual views even of the leading representatives: in fact, it would hardly serve our present purposes. The goal here is not broad scholarly coverage, but engagement with the most interesting positions. I choose to give special attention to the acknowledged key figure in the movement, at least in regard to explicit articulation of its methodology, namely David Bloor, with frequent invocation of contributions from his close collaborator, Barry Barnes, when this serves to clarify or strengthen Bloor's position.

2. The designation "Strong Programme" is meant to signal a contrast between the high ambitions of the Edinburgh group and the much more limited goals of traditional (i.e., Mertonian) sociology of science. As we saw in the previous chapter,

F. Collin, *Science Studies as Naturalized Philosophy*, Synthese Library 348,
DOI 10.1007/978-90-481-9741-5_3,

Mertonian sociology explicitly renounces any aim of explaining the contents of scientific theories, restricting itself to examining features of science as a process. It would examine the way science operates as a highly specialized community, existing within and interacting with society at large, with its own system of organization, recruitment, advancement and rewards.

To the Strong Programmers, this limited ambition betrays a lack of nerve and their efforts are designed to redress this shortcoming (Bloor 1976/1991, p. 4 f). The goal is to explain the very content of scientific theories, thereby (as it was sometimes put) founding a sociology of science, rather than of scientists. While rejecting this traditional self-imposed restriction, the Strong Programme is deliberately quite conservative in other respects, however, both concerning general methodology and the specifics of sociological theorizing. Its theoretical guiding lights all rank among sociology's "greats". Thus, it embraces the Marxist idea that interests, relating to class membership or otherwise, are a main moving force in society, extending their efficacy even to natural science (which Marx would exempt from such influence). It blends this element, however, with certain ideas from the tradition of Durkheim and Mauss, which see the categories in which nature is described as somehow derived from the discourse in which societies describe themselves. This tradition enters the picture especially through its recent revival in the work of Mary Douglas.[14]

We noted above that Bloor attributes the reluctance of Mertonian sociology of science to engage with the contents of scientific theories to "lack of nerve". But Bloor immediately proceeds to train his guns on another target: "Of course, the failure of nerve has deeper roots than this purely psychological characterization suggests, and these will be investigated later. Whatever the cause of the malady, its symptoms take the form of a priori and philosophical argumentation" (Bloor 1976/1991, p. 4). Philosophers, we are told, are only too eager to encourage the act of self-abnegation whereby sociologists abandon any stronger ambitions (ibid.). Philosophers turn out to be the real culprits and Bloor singles out Lakatos and Popper for explicit mention. Thus, already at this early point in the book, another important theme emerges: the desire to eliminate philosophical modes of argumentation in this field. Naturalization is the goal and is, indeed, explicitly introduced in Chapter 5, which is dedicated to the naturalization of mathematics. Mathematics is chosen as an example, since this field is thought to be among the cases most resistant to the effort to naturalize our understanding of the world. A success in this area would thus be a special triumph.

The indictment of Lakatos and Popper is worked out in more detail later in the chapter. Lakatos famously proposed a method for the historiography and sociology of science that would honour the inherently rational nature of science, while still accommodating the fact that science, as actually conducted, often falls short of the ideal (Lakatos 1976). The idealized conception is given primacy from the point of view of methodology, since it circumscribes those parts of actual scientific activity that, *qua* rational, neither call for, nor indeed allow, historical or sociological explanation. What is left is an irrational residue that sociology and history may then grapple with. Against this, Bloor insists that even rational science is a worthy topic of investigation and explanation. Popper is taken to task for his celebrated argument, in *Poverty of Historicism* (Popper 1957), of the unpredictability of scientific discovery and the social changes it brings in its train, which might be taken to imply its resistance to scientific, causal analysis. Bloor argues that this merely points to the danger of irresponsible inductive projections that fail to take all relevant factors into consideration. In brief, it merely highlights the inherent fallibility of science.

3. The methodological principles by which the Strong Programme defines itself are presented in what is generally accepted as the primary manifesto of the school: Bloor's *Science and Social Imagery.* They are set out in the following four points (op. cit., p. 7). Below, "It" refers to the kind of reformed sociology of science that Bloor advocates:

1. It would be causal, that is, concerned with the conditions which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.
2. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies require examination.
3. It would be symmetrical in this style of explanation. The same types of cause would explain, say, true and false beliefs.
4. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories.

These conditions are meant to express a commitment to what Bloor sees as an uncontroversial, mainstream conception of science; thereby, he intends to safeguard the scientific credentials of the programme. The aim is clear from the following quotation:

> The search for laws and theories in the sociology of science is absolutely identical in its procedure with that of any other science. This means that the following steps are to be found. Empirical investigation will locate typical and recurrent events. Such investigation might itself have been prompted by some prior theory, the violation of a tacit expectation or practical needs. A theory must then be invented to explain the empirical regularity. This will formulate a general principle or invoke a model to account for the facts.
>
> (*Op. cit*., p. 21)

On reflection, however, the above looks like a commitment, not to scientific rigour as such, but rather to the *modus operandi* of the most advanced sciences, such as theoretical physics. Indeed, it may even be called a commitment to the idealized conception of science constructed and propagated by logical positivism. As was clear to most students of science even at the time the above lines were written, the idea of science as uniformly wedded to a concept of laws and prediction matches only the most advanced natural sciences – and perhaps not even those. Biology, for instance, does not fit this pattern. The Strong Programme seems to be the unwitting heir to the logical positivist tendency to identify the idea of science with its most eminent exemplars within natural science. There is some validity in this diagnosis, yet it does not go to the heart of the matter. The Strong Programme's stance on this point is largely strategic, being designed to safeguard the programme's credentials, even as measured by the highest conceivable ideals of scientific procedure. The tacit agenda is to show that (natural) science is unavoidably shaped by social forces and the strategy is to use (social) science as the vehicle of this demonstration; hence it is crucially important that the scientific procedures adopted in the latter enterprise be beyond reproach. Otherwise, the controversial picture of natural science emerging from the investigation might be taken to indicate flaws in the method of investigation rather than in its object.

4. Next, we turn to conditions 2 and 3, which are so closely linked that they are best amalgamated into one; since their common theme has been the topic of lively discussion under the name of "Symmetry", I shall extend the title of the "Symmetry Principle" to cover them both. The official rationale of this (combined) principle is the cherished "value neutrality" of science (here again, the Strong Programme adopts a staunchly positivist conception of science). This stance would be compromised if true and rational efforts in science were treated differently from (supposedly) false, irrational or otherwise flawed ones; such non-discriminatory treatment must extend to explanation, too.

Let us ignore here the misgiving that a preference for truth and rationality might be thought to be intrinsic to science and hence not a corruption of proper scientific procedure; here, we need only record how Bloor construes "value neutrality" and move on to examine briefly the controversy that this principle has generated. To some extent, the debate has reflected divergencies in interpretation between Bloor and his critics, revealing that this principle is both ambiguous and vague on closer inspection. Bloor has expressed surprise at the numerous readings that have been proposed, yet this diversity is just what might be expected. After all, what the (extended) Symmetry Principle says is that we should treat scientific results that we deem true and rational *in the same way* as those we consider irrational, misleading and so on. Given the plasticity of the term "same way", there are bound to be a multitude of interpretations of this principle, ranging from the trivial to the highly controversial. Bloor in particular should not be surprised by this, since the protean nature of "sameness" plays a crucial role in his own overall case for Science Studies (see the next chapter). As it happens, even Bloor himself sometimes seems to vacillate a bit on how the principle is to be read.

Be this as it may, the reaction to the symmetry principle elicited from philosophers was quite harsh and in the second edition of *Knowledge and Social Imagery*, Bloor dedicated an *Afterword* to comprehensive rebuttals of criticisms directed against this point, among others.[15] There is no need here to go into details with these arguments. For the purposes of narrating the history of STS, all we need to note is that the strong philosophical reactions, Bloor's detailed rejoinders, plus the overall hostile tone of the exchange, serve to suggest that philosophy was the implicit target of Bloor's tirade from the start. Sociologists, with their regrettable lack of nerve, are not the main offenders, but rather themselves the victims of philosophy's paralysing effect on the intellect. As for the systematic merits of those critical arguments, they sometimes overshot their mark and Bloor had little difficulty in dismissing many of them. Still, I believe the critics were warranted in their basic reservation about the Symmetry Condition and, in the following, I shall present an argument against it as far as it pertains to the truth/falsify dimension. (I shall later, under a different heading, return to symmetry with respect to rationality and the issue of relativism.)

In the case of sociological explanations of the genesis of *true* scientific theories, and in particular in true theories emerging from a rational process of scientific investigation, the items postulated by the theory will actually occur somewhere in the causal story presented in the *explanans*. The opposite is the case in explanations of false theories. For example, we expect that in the explanation of scientific belief in the existence of oxygen, specimens of that substance will appear somewhere in the story as a causal agent, without which the mentioned belief would not have arisen in the first place. The chemical element is involved in certain characteristic and striking phenomena – chiefly, combustion – which reference to the element serves to explain. On the other hand, *phlogiston* will not occur in the explanation of the belief in phlogiston, as we tell that story today. (The example of oxygen vs. phlogiston is Bloor's own, although he draws the opposite conclusion; cf. Bloor 1976/1991, p. 37 f.)

Bloor does not directly address the asymmetry just pointed out, but instead directs attention to a respect in which the oxygen and phlogiston explanations are indeed (roughly) the same. In either account, a certain substance (*in casu*, oxygen), is claimed to be involved in certain chemical processes (prominently, combustion) that have certain characteristic observable effects: heating, flames and increase in weight. These effects inspire the construction of theories with which to explain or "interpret" these phenomena. One interpretation is the oxygen theory, while the phlogiston theory is another. The former happens to be true while the latter is false, or so we believe. But they both have the same logical and epistemic status: They are both hypothetical interpretations of experience, or "inferences to the best explanation".

One might be excused for feeling, however, that Bloor has changed the topic here. His consideration goes no way to show that *phlogiston* plays a similar or symmetrical role to that of *oxygen* in such explanations; only the latter figures in an explanatory role. The difference is easily generalizable. Let the standard form of a scientific theory be along the lines of, "There exists a kind of entity E which has such-and-such properties, expressed in laws $L_1L_2L_3\ldots L_n$". Now, sociological explanations of this kind of theory are non-symmetrical in the sense that, for true theories, E occurs in their explanation, while for false theories, it does not. This difference is intrinsic to the two accounts; by mere inspection, one could directly infer that in the oxygen case, the belief explained would *true* if the explanation is valid, while in the phlogiston case it would be *false*.[16]

Bloor could of course still maintain that the two explanations are of the same kind, in the sense that true beliefs (judged by our current standards) are in equal need of explanation as false ones (Bloor 1976/1991, p. 177), and that the explanation of the latter need not invoke irrationality, ideological bias, "pre-logical mentality", or other valuational terms; there is no disagreement with him here. Alternatively, the point might be that in both explanations, the items invoked are "natural" ones, with no appeal being made to a transcendental realm. This is a significant claim, which sets Bloor's principles apart from authors who would argue that in the case of rational calculation, we engage a transcendental realm of rational principles (ibid., p. 178). However, it is not pertinent to cases of the kind considered here, where the items dealt with by a supposedly false and a supposedly true theory are both clearly natural and non transcendental (such as for instance phlogiston and oxygen, respectively).

Thus, there are points of similarity as well as of dissimilarity between explanations of successful (true) and flawed (false) science. And, given the looseness of the term "same kind", it is futile to try to aggregate these aspects into a single, overall verdict as to the identity or difference of such explanations. The issue is now turning into a purely verbal one. Bloor ought indeed to acknowledge this, since he uses the very same flexibility as a crucial premise in an alternative argument for his position, as we shall see in the next chapter. Despite of this, the Symmetry Principle has acquired the status of a fetish, hotly worshipped by the adherents of the Strong Programme and heavily denounced by their critics, but without making any genuine contribution to the enterprise.

5. The Symmetry Principle stipulates that the *same* role be assigned to social factors in both successful and flawed science, but it is silent on precisely *what* that role is. It is time to examine directly the role that the Strong Programme assigns to social conditions in the explanation of scientific theorizing. Some critics have inferred that the Symmetry Principle grants an *exclusive* role to social factors, since these are supposedly decisive in the case of flawed science and hence, by symmetry, must have the same role in successful science. This strong interpretation has motivated some of the harshest polemics against Bloor's work. The position thus attacked is not one espoused by Bloor, however: he angrily rejects the accusation that he sees science as solely a product of social factors, reminding his critics that a statement to the contrary effect occurs already in the first of the four methodological principles defining the Strong Programme. According to Bloor, reality affects the scientific process through perception, and he dedicates an entire chapter to the discussion of this topic in *Science and Social Imagery* as well as in Barnes *et al.* (1996). He steadfastly maintains that man is, among other things, a biological organism that, like the rest of its ilk, is furnished with sensory organs, forming channels through which external reality interacts with its cognitive system and thus engenders beliefs. Experience does make a difference to belief, he insists, but not by directly imprinting its contents upon the mind, as naive empiricism would have it, but only indirectly, mediated by the subject's prior beliefs. Bloor uses the parallelogram of forces as a metaphor: A force does not impose itself upon an object without interference or mediation, but always in combination with other forces; like the ensuing motion of the body expresses the resultant combined force, so the belief generated by experience is co-determined by the subject's previous cognitive state.

However, Bloor's metaphor is not sufficiently discriminating to answer our question as to the precise role of the social determinant of science. The subject's state of belief prior to undergoing his current experience may be presumed to be partly shaped by accumulating past experience and not solely by social influences such as training; thus the difference between the social and the experiential component of knowledge does not coincide with that between prior and posterior belief. We still have not been told what are the *specific* epistemic contributions of the social and the experiential, respectively. What is the special role, attributed to society, that is supposed to set Bloor's view apart from the mainstream position?

In *Science and Social Imagery*, Chapter 3, Bloor provides a succinct statement of his position that I believe holds the key to our question; its significance is apparent from the fact that it recurs, with little variation, in numerous places. Here, he says that "the social component of science is the theoretical component" (op. cit., pp. 16, 98). This is brief and suggestive and allows several readings. I propose the following.

In science, there is an unbridgeable gap between conceptual structure and the world's impact upon our sensory apparatus. The theoretical language used by science – at least the highly developed sciences – is in no way derived or educed from experience; it is something we *bring to* experience. (Occasionally, this is said to hold for all concepts: cf. Bloor 1983, p. 156.) It origins are social: it is not a matter of eternal Kantian categories, but historically evolved and societally variable ones (here, we discern a Durkheimian element in Bloor's thought). This does not mean that reality plays no role in the gestation of science, however. The world is invited to say "yes" or "no" to the questions we pose it, through the process of observation and experiment; still, it has no constructive impact upon the categories in which the questions are couched when we do theoretical, scientific work. The social influence is crucial in science, by virtue of supplying the conceptual framework in which theories are couched, although experience is allowed to say "yes" or "no" to specific theories formulated in terms of it: "The natural order provides the external stimulus, and the social order the terms of the response" (Bloor 1974, p. 76; see also Bloor 1999a, p. 90; Barnes 1974, p. 12).[17]

The overall categorial structures in which we conceive the world are not neutral, however. They highlight areas of specific interest and leave others in the shadows. Some issues may even be impossible to articulate in a certain terminology, while they are made to stand out clearly in another. This means that global conceptual structures make superb instruments for pursuit of political and ideological ends and for the domination of one societal group by another. The most efficient exercise of power is the one whereby the victims are rendered incapable of even articulating their resistance, or which at last makes oppositional thought exceedingly difficult. To take a trivial example, in a framework of description in which "man" refers ambiguously to the human species and to the male of that species, we might solemnly honour the "Rights of Man", while overlooking the special plight of women. Similarly, in a context in which "value" has come to be almost synonymous with "exchange value", the importance of non-marketable social activities tends to be overlooked and politics is gradually turned into a calculus for computing the cost-efficiency of alternative scenarios of action.

This observation points towards another respect in which science is socially determined, according to Bloor. Not only is society the source of the theoretical categories in which nature is grasped; it also provides the mechanism by which the appropriate category is selected. In any socio-cultural context, there is a plurality of conceptual schemata available, in the abstract, for the description of reality. So, what determines which one is chosen? The answer offered by the Edinburgh School says, *social interests* (Bloor 1976/1991, pp. 170–173, 1983, p. 48; Barnes 1982, p. 101 ff; Barnes et al. 1996, p. 119 ff). This answer is backed up by a long list of case-studies to which Strong Programmers constantly refer. One of the most celebrated ones was produced by a member of the school, Steven Shapin, and concerns the reception of phrenology in early 19th-century Edinburgh (Shapin 1975). This reception shows a characteristic pattern, with phrenology being enthusiastically embraced by the working- and lower-middle classes, while the upper class rejected it. In the study, this difference is traced back to the differential manner in which adoption of this theory would benefit or harm the interests of those classes. (We shall return to this example below.)

As we have seen, this does not mean that theory choice is solely determined by social interests, according to the Strong Programme. The point is made already in the first principle defining the programme, and further detail is supplied in Chapter 2 of *Knowledge and Social Imagery*, in which the role of observation as a test of theorizing is stressed. Such tests may prompt a revision of our (socially) favoured theory, or even lead to its rejection. In either case, however, we will remain within a conceptual framework that is socially engendered. A minor revision of a theory – such as, say, an adjustment of the numerical value of certain parameters – will leave the conceptual framework intact. And in cases where the entire framework is repudiated, the one replacing it will have similar social origins. In both cases, Bloor would maintain the principle that "the social component is the theoretical component".

6. Thus, Bloor's position represents a rather moderate social constructivism. It does not claim that social factors determine the detailed content of the theory to which members of a given social group will give credence, but only the conceptual apparatus in which it will be couched. True, reality may resist the imposition of any particular such apparatus, by consistently saying "no" to questions addressed to it in terms of it. But that apparatus is then bound to be replaced by another one that is equally social in its origins. There is no "filtering out" the social, as such, in the scientific process.

The radicality of Bloor's position is further blunted by considerable latitude in the meaning given to the term "social". The most interesting and challenging reading is one that takes social categories to mean *sociological* ones; this reading is natural, since the Strong Programme defines itself as an effort within the *sociology of science*. According to this reading, the concepts used in the explanation will be those used in the standard theories of sociology and in neighbouring disciplines such as political science and economics. This cluster of concepts is hard to circumscribe with any precision; still it is easy to point to paradigm cases such as "class interest", "social structure", "power" or "authority". Indeed, this is what we find in the works of the Strong Programmers.

According to a second, slightly broader reading, however, the Edinburgh approach is "social" in the sense of tracing scientific concepts back to the stock of pre-scientific concepts in which social agents describe their native society. The idea derives from the Durkheimian tradition and is alluded to by the term "social imagery" in the title of the main Edinburgh tract: In explaining nature, cultures – in particular "primitive" cultures – project categories inherent in that society's self-interpretation onto the larger canvass of nature. This cognitive mechanism typically reflects a general metaphysics postulating some sort of correspondence between human society and nature, a conception fraught with normative and sometimes religious overtones. According to the Edinburgh School, the roots of physical concepts in a society's self-interpretation persists even in our own, scientific culture (a conclusion which Durkheim himself did not draw); even the explanatory categories of modern Western science are eventually transformations of notions used to describe that same society (Durkheim 1915; Durkheim and Mauss 1963).

According to a third, even weaker reading, which has extensive textual support in the Edinburgh School corpus, the theoretical concepts of science are "social" in the broad sense of being available in the language, tradition, or culture in which the theory emerged (Bloor (1976/1991), pp. 74, 124; Barnes 1974, Chapter 3); Bloor and Barnes often refer to them as "cultural resources" used by science (Barnes 1974, Chapter 1). This suggestion is further elaborated by Barnes (1974, Chapter 3), who stresses the *metaphorical* nature of scientific concepts, representing them as transfers of everyday concepts, available as general social resources, to cover a new set of phenomena.

If this conceptual genealogy is supposed to support the claim that science can be explained precisely in *social* terms, however, a demonstration is needed that the concepts in question are indeed *socially* generated. One way to do this would be to show that conceptual structure is in general socially variable and that the concepts involved in the particular case under scrutiny are endemic to the culture in question. If, by contrast, the conceptual resources drawn upon are found in all natural languages, nothing specifically social will be involved, apart from the fact that natural languages as such are, by definition, collective and hence social. This recapitulates a discussion in linguistics and anthropology, where a "linguistic relativity principle", in vogue two generations ago, held that conceptual structure varies between societies (the Sapir–Whorf thesis, cf. Sapir 1973; Whorf 1956). In the meantime, the pendulum has swung in the opposite direction. There is now a growing literature, forming part of the discipline called "cognitive semantics", suggesting that the conceptual structures embodied in languages are indeed psychological or biological universals (Rosch 1981). Bloor and Barnes spend little time rebutting such alternative interpretations, nor do they show great care to heed this distinction in their examples. Thus, Barnes invokes Dalton's atomic theory, which allegedly used "our everyday conception of a lump of solid material" as a model (Barnes 1974, p. 51). There is no attempt to show that this conception is *ours* in the precise sense of being unique, say, to the Western, European culture which we share with Dalton (although play could presumably be made with the old Whorfian point that this way of describing nature is generated by the subject-predicate structure of Indo-European languages, and not to be found in languages that adopt a process ontology). At any rate, it is plausible that our notion of a lump of matter has close counterparts in every conceptual framework used by man and that the category of an enduring thing with shifting properties is a human universal (Sapir and Whorf notwithstanding). Thus, there is nothing specifically *social,* in the sense of *socially determined*, in the notion of a lump of solid material. If the opposite were insisted on, it could be argued, conversely, that this cultural determination has apparently been overcome in important parts of physics that use *fields* or *waves* as their core concept (such as relativity theory and Schrödingers wave mechanics).

Finally, there is a fourth and even more inclusive interpretation of the "social" nature of concepts to be found in the Strong Programme. All concepts, even such, if any, that might be firmly grounded in the neural architecture of *Homo sapiens*, are social in the sense that their essential *normativity* can only be explicated by reference to the authority of a shared social practice. All concepts are responsible to standards of correctness in their application; they must all make room for the distinction between a correct and an incorrect use, on pain of losing their power as epistemic tools. As it turns out, this is the reading that is eventually made to carry the weight of Bloor's key argument; the particular version of the above thesis is that found in Wittgenstein's celebrated rule following argument, the role of which in the Strong Programme we shall examine in the next chapter. At the same time, it is a reading far removed from the one that created all the fuss about the Strong Programme in the first place: that is, that the explanatory categories were those of social science. Above all, it is a reading that is based upon a *philosophical* thesis, thus elevating the discussion from an empirical, scientific context to one pertaining to the philosophical analysis of language.

In the rest of this chapter, I shall interpret "social concepts" as either concepts derived from theorizing in social science (sociology, anthropology, economics or political science), that is, reading 1 above; or concepts belonging to the agents' social "proto-theories", i.e. concepts in which they describe their own societies (reading 2 above). I shall argue that the explanatory ambitions of the Strong Programme turn out to be unfulfillable on these interpretations. I shall bypass reading 3, which trivializes the issue. In the next chapter, I shall examine what happens when we adopt reading 4 above. It will turn out that this reading, although even broader than reading 3, leads not so much to triviality as to absurdity.

7. We have seen that the Strong Programme stands firmly upon the explanatory ambitions of the sociology of science. But what do Bloor and Barnes mean by "explanation"? It is one of the most surprising facts about a school that prides itself on its explanatory approach that the notion of explanation is simply used as an unexamined resource, with no attempt at systematic, explicit analysis. This is particularly striking in view of the fact that, when the first edition of *Science and Social Imagery* appeared in 1976, analytic philosophy of science had spent a couple of decades and lots of ink debating over the nature of explanation, in particular whether or not the

social sciences used the same explanatory pattern as the natural sciences, or indeed were capable of delivering explanations at all. The hub around which the debate revolved were the writings of Carl Gustav Hempel, whose deductive-nomological model of explanation was the starting point of every discussion of the topic. Yet Hempel is not mentioned anywhere in the writings of Bloor or Barnes (or indeed, as far as I have been able to discover, by any of the other authors treated in this book, with the exception of Steve Fuller).

Reference to Hempel would have been natural, since, as a matter of fact, if we combine the various hints that Strong Programmers provide as to their conception of explanation, it adds up to something very close to Hempel's model, as a matter of fact in its strongest, deductive-nomological version. We saw above that Bloor endorsed a conception of scientific method according to which science explains phenomena by subsuming them under causal laws. Moreover, in a later work, Barnes insists that explanation works by invoking strictly deterministic laws (Barnes 1974, Chapter 4). This is very close to Hempel's analysis, which may plausibly be viewed as a "rational reconstruction" or generalization of causal accounts.

A particular stylistic feature of Bloor and Barnes's work allows them to voice the strong ambitions expressed in the four principles, while still leaving their explanatory commitments rather vague. They standardly talk about the sociology of science dealing with the *causes* of events, instead of their *causal explanation*. Similarly, they talk of *social causes*, instead of explanation by subsumption under *causal laws* pertaining to the social realm. By thus expressing himself in the "material mode", Bloor avoids commitment to any particular construal of (causal) explanation. Moreover, talk about causes is *extensional*, whereas talk about explanation is *intensional*. As a consequence, there is a gap between the view that science is a social activity – which anybody would grant – and the claim that science is explicable *qua* being social, which is an entirely different matter. Using the extensional idiom, however, facilitates the surreptitious slide from the former position to the latter. It becomes natural to think that since science is a social activity, its progress must be explicable in social terms. Yet this is a fallacious inference, as is demonstrated by the obvious invalidity of concluding from the fact that science is undeniably an activity undertaken by biological organisms that it is explicable in biological terms, or inferring that it is explicable in gender terms from the circumstance that science is without exception undertaken by gendered creatures.

A possible reason for Bloor and Barnes's discretion with respect to explanation will, I think, become clearer in the following section, where we examine the obstacles in the way of social explanation of scientific theorizing in Hempelian terms. These problems are also Bloor's and Barnes's problems, since this is the model of explanation to which they implicitly commit themselves.

8. Let us first briefly rehearse a few basic points about the nature of explanation, according to the Hempelian model (cf. Hempel 1942, 1962, 1965a; Hempel and Oppenheim 1948). The deepest intuition behind Hempelian explanation is that explanation proceeds by providing information showing that the phenomenon in question had to happen, given the circumstances. This means that explanations are

arguments, or inferential structures, implying a description of the phenomenon to be explained. Among the premises in the argument must be laws of nature, plus information showing that the phenomenon in question is covered by those laws; hence the name "covering law explanation". In the face of an intense dispute and a growing list of compelling counter-examples, Hempel eventually modified this stance to the point that even arguments showing that the event to be explained had a high probability of happening were allowed to be explanatory; thus he supplemented his deductive-nomological analysis with an inductive-statistical variant. Indeed, he showed some willingness to admit that an argument might have explanatory power as long as it made the occurrence of the event to be explained more probable that its non-occurrence.

If we put this in the formal framework familiar from Hempel, we get the following picture: An explanation is an argument in which a description of the thing to be explained (the *explanandum*) follows from a number of premises (the *explanans*), either with deductive necessity or high statistical probability. The explanatory argument consists of at least one sentence of unrestricted universal form, expressing a law of nature or an empirical generalization and one or more singular sentences specifying "initial conditions", that is, particular features of the situation by which the protasis of the law is rendered applicable to it. In all but the simplest cases, the universal premise in the argument will be of very complex structure, comprising an entire theory or even a cluster consisting of a main theory, surrounded by various auxiliary hypotheses. We need not record all this structure here. We can make do with a simplified model, consisting of the universal, theoretical component and the singular component(s). Thus we get the structure below:

Theory T

Explanans

Initial conditions I_1 I_2 $I_3 ... I_n$

E *Explanandum*

Thus, according to Hempel, explanation is really an epistemic undertaking, a demonstration that – given the setting – the *explanandum* had to occur, or had a high probability of occurring, or at least a higher probability of occurring than not. Any argument that is epistemically irrelevant is thus also irrelevant from the point of view of explanation.

However, the concept of explanation is not adequately captured by such formal analysis, as Hempel himself admits; certain material conditions must be added. One is what I will refer to as a condition of *relevance* which rules out spurious explanations of the following kind (I use Hempel's own example from Hempel and Oppenheim 1948): For the theory T, take "All metals are good conductors of heat". For E, take any true singular sentence, say "Mt. Everest is snowcapped". Next, for I_1, take any singular sentence describing an instance of T, e.g. "If the Eiffel Tower is metal, it is a good conductor of heat". Finally, for I_2, take a conditional formed by I_1 as the antecedent and E as the consequent (since both sentences are true, this material conditional is true as well). This gives us the following explanatory argument:

All metals are good conductors of heat
If the Eiffel Tower is metal, it is a good conductor of heat
If the Eiffel Tower is metal, it is a good conductor of heat$\Rightarrow$ Mt. Everest is snowcapped

---

Mt. Everest is snowcapped

Thus out of thin air, we have plucked an explanation of the fact that Mt. Everest is snowcapped, impeccably formed in adherence to the principles of covering law explanation.

This upshot is clearly unacceptable and calls for the imposition of constraints upon the formal explanatory structure. There is no need here to go into the logical handiwork that this calls for on Hempel's part; those with particular interest in the matter may consult Hempel and Oppenheim's "Studies in the Logic of Explanation" (p. 277 f in Hempel 1965b). Let me just say that, intuitively put, the resulting restriction works by limiting the scope of an explanation to items as described in the vocabulary that occurs in the explanatory law: Only such items are genuinely subsumable under the theory and thus are genuinely explained. Other facts, artificially constructed by means of the principles of sentential logic, may indeed follow from it logically, but do not enjoy its explanatory effects.

We may express this point in a slightly different way: Any *non-logical, empirical term* that occurs in the conclusion of an explanatory argument must somehow derive from the premises, and hence be found in them somewhere. Given the structure of scientific explanation as just expounded, this means that the theoretical contents of the *explanandum* may derive from either of two sources: from the general sociological theory, or from the premises spelling out the initial conditions (or from both of them – but here I shall concentrate on the pure cases, which suffice to illustrate the point).

A little reflection shows that this fact presents the social constructivist with a dilemma: Only a derivation of the former kind (call it Type I) can be said to explain the genesis of hypotheses in sociological terms. It does so, however, at the cost of presupposing an extreme logical and conceptual strength in the sociological theory involved; indeed, so immense as to constitute a *reductio ad absurdum* of Type I explanation. The latter kind of derivation (Type II), where the theoretical and conceptual contents of the *explanandum* come from the initial conditions, is not saddled with such a huge inferential burden; on the other hand, it leaves the genesis of theories unexplained.

Let me illustrate these abstract claims with an example, starting with the kind of explanation where the theoretical contents of the inferred hypothesis (the *explanandum*) stem from the initial conditions (Case II). An interest-based theory about the selection of scientific hypotheses might assert that those scientific hypotheses are generally embraced, among the hypotheses being offered, which underpin the interests of the ruling classes; or that hypotheses are adopted or rejected in different classes in a manner reflecting their conformity with the interests of those classes. In order to explain any particular episode in the history of science, this theory would require additional information about the hypotheses circulating in the scientific community and about how this or that particular hypothesis would support the interests of the different classes.

This seems, for instance, to be the overall structure of Shapin's celebrated explanation of the differential endorsement of phrenology in Edinburgh, cf. Shapin (1975). In terms of our little simplified model, this case comes out as follows:

Sociological theory:
Among the available scientific hypotheses in any given situation, those will be embraced by a given class that underpin the interests of that class.
Initial conditions:

a. The following hypotheses were available: H_1 H_2 H_3...H_n
(E.g: In Edinburgh, the phrenological hypothesis was available, together with rival theories H_i H_{ii} H_{iii}...H_n
b. For each of those hypotheses, there was a protocol of relevant (positive or negative) "observational" data: $P_{H1} P_{H2}$ $P_{H3} \ldots P_{HN}$
c. Among those hypotheses, H_n (the phrenological hypothesis) conformed best to the interests of the working- and lower-middle classes.

H_n was embraced by the working- and lower-middle classes in Edinburgh.

The general theory involved in this explanation carries a very light conceptual load; instead, a heavy input of additional information, including additional concepts, is supplied by the premises specifying "initial conditions". By the same token, the explanation does not account for the discovery, or creation, of the theories themselves, but only for their reception, that is, their success or otherwise in the scientific community once they have been proposed. Shapin's analysis, for instance, does not explain why someone would propose a theory linking the shape of the skull with the development of mental powers in the first place. (As a matter of fact, this happened not in Edinburgh, but in Vienna, through the efforts of Franz Joseph Gall.)

It is clear that in the above argument, the existence of the phrenological hypothesis and its currency in Edinburgh is simply introduced as a brute fact in initial condition a; it is used as an explanatory resource, without accounting for how this theory emerged, whether in terms of sociological resources or in any other way.

Notice that this shortcoming has nothing to do with the inferential strength of the explanation – that is, whether it is held to be deductive-nomological or merely inductive-statistical (which would be the case if the theory expressed in the first premise were taken to have a probabilistic form, stating what happens in *most* cases).

9. There could be an objection that this criticism overlooks the true locus of the theoretical apparatus contained in the account and that the above account actually succeeds in explaining the source of the hypothesis after all. The theoretical concepts are actually implicitly present in the observations listed in initial condition b and what happens in theoretical scientific work is really that this content is rendered explicit. All observational data are saturated with (implicit) theories. In the case in point, the conceptual framework in terms of which the working- and middle classes

describe the relevant aspects of their experience is already enriched with the terms of the phrenological thesis, which just await a proper occasion for being made explicit.

There are certain exegetical problems with this suggestion in relation to Strong Programme sources. It is indeed true that the Strong Programmers reject the existence of pure, unadulterated data. But it is not clear that they prohibited the possibility of data that are at a fairly low level of theoreticity and, at any rate, not infused with the vocabulary of the particular theory under investigation. Bloor seems to envisage this possibility in later works, in particular in Barnes, Bloor and Henry (1996, Chapter 1). At any rate, the suggested attempt to save the explanation is clearly in vain, since it merely shifts the target of the criticism: What now needs to be explained is how the language in which members of the working- and middle classes describe their experiences came to be infused with a theoretical content which, when explicitated, expresses the phrenological hypothesis. The explanation will be to the effect that the individuals in question harboured specific social interests and hence would be prone to adopt an empirical vocabulary loaded with a theory germane to those interests. The challenge is now to explain how those interests could generate that theory-loading. According to the analysis above, the explanation will involve the exercise of inferring the theory from a specification of those interests. Thus, we are back where we started, with the problems standing in the way of such an inference. Or, more correctly, we are now facing a related but slightly different problem, since the present inference belongs not to Type II, which we examined above, but to Type I in my taxonomy. We shall soon turn to this type to demonstrate that it is beset with equally grave problems.

Finally, we may note that the above schema also accommodates the case where a scientific hypothesis is "socially determined" in the sense of using terms transposed from the way that society describes itself (i.e., Case 2 on p. 43). Such hypotheses underpin the social interests of a particular class by making the status of that class appear as somehow conforming to the natural order of things. This point is familiar from the Durkheimian tradition and, when rendered explicit, amounts to a sociological theory that may serve as a general premise in the schema on p. 46. To this we add, as a premise expressing an initial condition, a description of the proto-sociological self-understanding current in the society in question, employing the vocabulary V. The conclusion of the Hempelian schema will then assert that the vocabulary V will reappear in the hypotheses generated by the natural scientists in that society. Thus, the vocabulary in the *explanandum* derives from the agents' proto-sociological self-understanding, whereas the inferential power that delivers the conclusion comes from a sociological theory proper which we, the explainers, supply. The criticism levelled above applies to this case as well: The account does not explain the *genesis* of the vocabulary that is shared by the scientific hypothesis and the native social self-description, but merely avails itself of it, as a piece of brute information contained in the argument.

10. Now, to the Type I explanation. In this type of account, we use a theory that is strong enough to explain the very emergence of hypotheses (*inter alia* a hypothesis that is embedded implicitly in the way observations and experiments are described).

One kind of working theory would be a functionalist one, to the effect that in any social situation, scientific hypotheses will emerge if their adoption is needed to satisfy a functional requirement of the perpetuation of society, or of the hegemony of its ruling class. If we managed to render such a theory plausible, it could indeed be used to explain the emergence of a scientific hypothesis. It would explain by pointing to the conditions in society, or among its ruling classes, which depend upon the general adoption of precisely such doctrines for their survival.[18]

In terms of our little simplified model, this case comes out as follows:

Sociological theory:
In any social situation, scientific hypotheses of the form H_i will emerge if their adoption is needed in order to satisfy a functional requirement of the form R_i in the society, or in its ruling class.

(As a special case of this: In any social situation, scientific hypotheses of the form H_i will shape social agents' perception of the world, if this is needed in order to satisfy a functional requirement of the form R_i in the society, or in its ruling class.)

Initial conditions:
In situation S, there was a functional requirement R_n in the society, or its ruling class.

Hypothesis H_n emerged

Such a functionalist theory would have to possess an immense inferential and conceptual strength, since it would have to be rich enough to entail, given additional information about the society in question, the basic features of any hypothesis that has actually been embraced by any significant scientific community. We may compare it to a computer programme of such power that, when data about a society or community are fed into it, in particular data about its ruling class, together with a body of observational data pertaining to a particular problem of (natural) science confronting that society, the programme generates a scientific hypothesis that will explain those data, while at the same time furthering the interests of that class. This is only possible if the functionalist theory in question contains all the terms of every hypothesis ever devised by mankind and subsequently adopted. Perhaps God could devise such a theory, but it is clearly far beyond the powers of man.

There might be an objection that the contrasting examples used to illustrate Type I and Type II explanations, respectively, do not exhaust the possibilities. It is easy to construct an example, it might be said, where the explanatory theory is not excessively strong, but still suffices to account for the genesis of a hypothesis. We use a functionalist theory as in the example above, but this time formulated without any intrinsic specification of the possible functional requirements, nor any intrinsic specification of the kinds of hypotheses whose dissemination would satisfy those requirements. The theory simply says that, in any social situation, scientific hypotheses will emerge if their adoption is needed to satisfy a functional requirement in the society, or in its ruling class.

We next add a premise (initial condition) to the effect that adoption of a certain specified scientific hypothesis is needed to satisfy a functional requirement in the society, or in its ruling class. The hypothesis in question is specified in the abstract; no assumption is being made as to whether or not it has actually been proposed in the ongoing scientific debates. Still the functionalist theory now allows us to deduce that this hypothesis will at some stage be propounded by somebody and will be embraced by the community in question.

Thus we apparently get the best of both worlds: We get explanatory power with respect to the genesis of a scientific hypothesis (moving within the context of discovery), but still avoid an excessive inferential strength and conceptual load to bog down those functionalist theories and put them beyond human reach. Schematically, it would look like this:

Sociological theory:
In any social situation, scientific hypotheses will emerge if their emergence and adoption are needed in order to satisfy a functional requirement of the society, or of its ruling class.

Initial conditions:
In situation S, there was a functional requirement R_n in the society, or its ruling class, that could only be satisfied by the emergence and adoption of hypothesis H_n.

Hypothesis H_n emerged and was adopted.

This explanation looks almost too good to be true; and indeed it is. Notice that the functionalist account makes use of information to the effect that the emergence of the hypothesis in question was needed in order to satisfy a functional requirement in the society, or of its ruling class (given the further features of the situation). Now this kind of information, which is here innocently tucked away in an initial condition, could only be supplied, as a *general* resource, by a background functionalist theory with the kind of extraordinary logical and conceptual strength that was found in the original analysis of Type I explanation and which we tried to do without in the alternative analysis above. If we want our sociological theory to be able to explain the historical course of science in general, this auxiliary component in our theory would have to be strong enough to generate, and compare, all the different scientific hypotheses that have actually been formulated and, in addition, demonstrate that (the societal acceptance of) the particular hypothesis under scrutiny was needed to satisfy a functional requirement in the society, or in its ruling class. The latter demonstration would necessarily involve the generation of a number of further, rival theories, formulated in the abstract, which might have emerged, but which would not have satisfied those requirements. Again, we are saddled with an immense burden.

Notice that information concerning the potential of a hypothesis to satisfy a functional requirement cannot be got cheaply from an analysis of the purely cognitive content of the doctrine in question. Such a purely hermeneutical, intellectualist procedure cannot tell us anything about the chances that such a theory will catch on in the general public, a parameter that is of crucial importance from a functionalist point of view. A doctrine to the effect that the aristocracy is a special breed of superior beings, or that the king received his prerogatives directly from God, might perhaps supply an argument in favour of their interests in the abstract, but would be without practical value at a stage of historical development where such doctrines would be dismissed out of hand as naive or ludicrous. Given the assumptions of constructivist sociology of science, the potential of a doctrine for gaining acceptance can only be determined on the basis of deep sociological theorizing.

Let me repeat a crucial point made above: In all these examples, the criticism is not based upon the assumption of a strong inferential link between *explanans* and *explanandum*. Thus the argument does not depend upon an overly strong construal of covering law explanation, one which Hempel himself repudiated when he admitted inductive-statistical alongside strictly deductive-nomological versions (Hempel 1962). Hence, our criticism is not undermined by the observation that, according to the Strong Programme, social interests do not assert themselves in every instance. Sometimes, even a highly socially favoured theory will be given up in the face of recalcitrant observation; social determination is thus statistical. The argument depends upon a material condition of adequacy concerning the *vocabulary* (concepts) employed in such explanation, that is, the principle of relevance. This principle says that any term occurring in the *explanandum* must already have occurred in the *explanans*. This is not a logical principle, but a material condition formulated by Hempel to avoid trivial explanation; it applies equally to deterministic (d-n) and to statistical (i-s) explanations. It presents Bloor and Barnes with a dilemma: either, they have to demonstrate that the terms of the *explanandum* (which comprise the formulation of a theory of natural science) are already to be found in the social context of the theory's emergence; or they will forfeit the explanatory power of the account.

Perhaps Bloor and Barnes's writings actually suggest a way to avoid this dilemma, however: They hold that the terms in which scientific theories are couched are *analogical extensions* of certain terms used in the social context that engendered the theories. That is, they are not logically equivalent to the latter concepts; but neither are they completely unrelated to them, which saves them from falling prey to Hempel's injunction against vacuous explanation. Barnes espouses a particularly pointed version of this view, to the effect that all deductions about empirical phenomena involve an analogical step (Barnes 1982, p. 122).

I believe that this manoeuvre brings Bloor and Barnes up against another, equally pernicious dilemma, that is, between making a claim that is false but highly intriguing, and making one that is not obviously false but is so vague as to be devoid of interest. Presumably, any scientific concept may be shown to be related analogically to some everyday concept; the Strong Programme is now rendered so weak as hardly to deserve its epithet any more.

11. We must look at a final objection to the above argument. It might be thought to overlook the fact that reductive theories in any area of science do not normally explain a phenomenon directly, but only by a detour via *bridge principles*. (For

the classic discussion of this topic, see Nagel 1961, Chapter 11.) For instance, the everyday term "water" does not occur in the basic theories of physics; hence, no conclusions concerning water may be inferred from them. On the other hand, the terms "electron", "proton" and "neutron" do occur and these permit the description and explanation of the properties of atoms consisting of those elements. Among the atoms thus analyzable are those of oxygen (O) and hydrogen (H), and so the theories in question (in particular, quantum mechanics) suffice, in principle, to explain the properties of a molecule consisting of an atom of oxygen and two atoms of hydrogen (H_2O). This has implications for water, once we add a bridge principle to the effect that water is identical with H_2O. In a similar fashion, the laws of electromagnetism say nothing about light or colour, until we supply a bridge principle stating that light is electromagnetic waves and that differences in colour are differences in the wavelengths of light.

By parity of reasoning, a reductive sociology of science need not imply, without mediation, the facts we want to explain on the basis of theories and data. All that is required is that such implications ensue once we add various bridge principles, correlating theoretical terms in the sociological theory with relevant terms in the *explanandum*.

What could it mean to say that a particular societal condition was *identical to* the condition of a certain group of agents, involved in that very situation, having such-and-such thoughts? Once we take a closer look, it becomes clear that the answers lead to a regress of the original objection, in one way or another. The most plausible bridge principle emerges if we construe the social term of the identity relation as referring to a micro-sociological state of affairs of the kind familiar from laboratory studies, where laboratory activities are described with unrestricted use of the agents' own theoretical terms. For instance, the activities in a physics laboratory, including discourse involving such terms as "quarks", and so on, could convincingly be taken as evidence that the physicists in question believed in quarks, and took themselves to have demonstrated their existence. (Indeed, on a behaviourist construal of thought, such activities and utterances might even be taken to be what thoughts about quarks *consist of*.) This would suffice to establish the identity between, on the one hand, a (micro-)sociological state of affairs to the effect that the research group in question accepted the existence of quarks and, on the other, a complex state of affairs at the level of individual psychology, to the effect that researcher A believed in the existence of quarks, that researcher B believed in the existence of quarks, and so on down to the last member of the group.

The problem is that this result has only been achieved by enriching the *explanans*, which is supposedly sociological, with terms that are not parts of the sociologists' standard vocabulary, but rather of the physicists'. The social condition is characterized with unrestricted use of the terms that the scientists themselves use to describe their activities. These terms have just been slipped in as collateral information; they are no part of any standard sociological theory invoked. Thus, what we have is not a bridge principle connecting two descriptions of which one uses physical terms while the other uses purely sociological terms: the latter is already surreptitiously enriched with physical terms. Thus the original problem reappears: We now have to ask how sociological theories that do not use such esoteric vocabulary can be used to explain phenomena described in such terms, within the principles of Hempelian explanation, which calls for an inferential relation of a specific kind between *explanans* and *explanandum*. The answer might be: by means of bridge principles. But now we are back where we started.

The identity view may still have resources to go a few more rounds in this exchange, but there is no need to go into that here since, as far as I know, nobody has wanted to defend such a view in the first place. I believe the ultimate outcome is inevitable anyway: the identity view must be rejected. But then the problems outlined above are back in full force.

12. In the self-conception of the Strong Programme, it is based upon the alleged empirical successes of its causal, explanatory approach; and long lists of such accomplishments are frequently provided (for instance Barnes and Bloor 1982, p. 23f). As Bloor emphasizes, the principles of the programme are inductively based, as they spell out the procedure which has produced the programme's acclaimed successes (Bloor 1976/1991, p. ix, 1981, p. 206). Certain particularly striking cases are always mentioned and we have touched upon some of them above: the Forman case of quantum mechanics (Forman 1971), the controversies surrounding phrenology in early 19th-century Edinburgh (Shapin 1975), and a few more. Do these cases actually accomplish what is impossible according to the above argument? Does the bumblebee actually fly, undermining our efforts to demonstrate that this could not possibly happen?

Closer scrutiny shows that this is not the case: none of the above case studies derives a specification of the contents of a scientific theory from purely macro-sociological parameters. They all turn out, on closer inspection, to be of Type II in my listing above. That is, *they take for granted* (i.e., introduce as collateral information contained in the initial conditions) the existence of certain scientific hypotheses that circulate in the scientific community. They do not attempt to explain why, among all the hypotheses that mankind could possibly devise, *this* one was generated at the time. Instead, they start out with a limited number of hypotheses that are actually available (i.e., that have actually been proposed) and proceed to show why one of these would be favoured by the scientific community in the light of the latter's interests. But it is obvious that what we are offered now is an account of the *reception* of hypotheses already articulated, not an account of the *genesis* of hypotheses. The existence of a number of hypotheses is taken as a datum, as input to the explanation, and is not itself explained. They belong to the *explanans*, not the *explanandum*. We have already shown this with respect to the phrenology case, but the same thing could easily be done with respect to the celebrated Foreman case and all the other cases frequently referred to as examples of the Strong Programme's successes.

Several elements of the Strong Programme have helped its adherents to hide this circumstance from themselves. In the first place, the convenient vagueness of the programme that has been pointed out above, underneath the apparent stringency of the four methodological principles, obscured the difference between the two modes of explanation and their differential import with respect to the programme's aims. A deeper reason, however, is that the Strong Programme was always Janus-faced, having both an empirical and a philosophical agenda. The philosophical agenda relied on a stronger, philosophical argument that would in the end render empirical, "inductive" argumentation superfluous. Behind the alleged empirical-inductive approach, a philosophical argument was always lurking, to the effect that the determinants of scientific development *could be no other* than social. We shall look at this argument in the next chapter.

13. The argument so far has moved within the general Hempelian framework of explanation. And the objection would now surely be raised that, although Bloor may fairly be said to have committed himself to something along these lines, this nevertheless amounts to an artificial constraint upon the Strong Programme; indeed, it is foreign to its fundamental impetus (cf. Collins 1981c, p. 215). The key ambition is to explain science in social or sociological terms, which calls for deployment of the patterns of explanation germane to the social sciences, not those used in the natural sciences. In their effort to make themselves palatable to the natural science community, the Strong Programmers, to their detriment, burdened themselves with standards traditionally adopted by the latter. But they should have stuck to sociological explanations instead, for which many alternative models have been proposed. Among these are various kinds of *intentional* explanation (e.g., purposive explanation, reason explanation, and others). Such models were already brought forward in the debate following the launching of the Hempel model, by Dray and others (Dray 1957); further novel models of explanation have been developed in the subsequent period.[19]

This objection is very much to the point, so long as we consider the Strong Programme as simply a project within the sociology of science. But, as I have argued, it is also an attempt to naturalize philosophy of science; to solve philosophical questions in this area by empirical means. From this point of view, there are powerful reasons for adopting Hempel-style explanation. Hempelian accounts have the attractive feature of *exclusivity*; this means that, if such an account can be provided, we know *eo ipso* that no other account can be given of the same phenomenon, at the same level of abstraction. More specifically, there is no room for an account introducing entirely different explanatory factors, described in an entirely different vocabulary. In the present case, this has the very attractive implication, in the eyes of the Strong Programmers, that no account referring to "rationality" or other of the philosopher's cherished categories can be provided – nor one in terms of purely psychological factors – unless all scientific events are over-determined. This strategic strength is given up once a weaker account is adopted; even more so, when the requirement of explanation is given up altogether.

The point is well-illustrated by a current parallel effort at naturalization, that is, with respect to philosophy of mind and its paramount preoccupation, the mind-body problem: If a complete account of human action and thought can be given in purely neuro-physiological terms, chiefly referring to brain states, there is no room for an account dealing in such items as "thoughts" and "ideas", mentalistically conceived. The physiological account establishes *explanatory hegemony*, dictating the terms within which explanation must be achieved, to the exclusion of other accounts. Mental states, described in intentional terms, may at best gain a marginal foothold in the picture if they can be shown to be identical with brain states, not merely on a one-one basis ("token identity"), but in a systematic way ("type-type identity"); moreover, the identity must not be just contingent but possess some kind of necessity. This calls for bridge principles of considerable strength; the possibility of devising them remains a moot point in the philosophy of mind (Kim 1996, Chapter 9).

To Strong Programmers, the recent developments in the philosophy of mind should indeed stand as a success to be carefully emulated. The explosive growth in the neurophysiological understanding of the brain, although in itself neutral on the philosophical aspects of the mind-brain issue, provides the best possible platform for the age-old attempt to do away with the shadowy inner Cartesian theatre. There is no need to critically target the odd metaphysical features of the mental, a strategy forever vulnerable to the counter that mental states are, after all, given in even the humblest everyday introspection. Instead, mere scientific facts may be adduced, followed by the observation that they hardly seem to leave room for the "ghost in the machine". To launch a similar argument within Science Studies would constitute the dream strategy for Strong Programmers; indeed, they often suggest that this is how they actually proceed, as witness Bloor's insistence that his programme is "inductively" based. But this calls for accounts that are explanatorily *complete* and hence *exclusive*: accounts that, if valid, leave no room for alternatives.

Notice that exclusiveness is not merely a feature of Hempel's early, d-n version of the covering law model, but applies even to the weaker, inductive statistical version as well, to the extent that the accounts given are taken to be *complete*; that is, that no rival explanation can raise the statistical probabilities involved. For instance, quantum mechanics is irredeemably statistical, assigning merely statistical values (or superpositions of such values) to certain subatomic events. Still, quantum mechanics is complete, since there are no further factors to be invoked that could render the accounts fully deterministic; at least, so the celebrated Copenhagen interpretation has it. Hence, the conceptual resources invoked in quantum mechanics need no supplementation by other concepts to fill the gap. The import of this feature of Hempelian explanation for our current concerns is clearly illustrated, too, by naturalization in the theory of mind: Some of the recent attempts to explain the mind in physical terms postulate rather esoteric quantum-mechanical events in the human brain, and the resulting accounts thus will not be fully deterministic (Penrose 1989). But this feature does not let the Cartesian mind back in again, unless it is assumed that the *res cogitans* hovers patiently over the physical substance, waiting for quantum-mechanical gaps in the causal chains that it can exploit to push physical particles in one direction or another at its fancy.

The above reflections anticipate a main theme of the subsequent chapters of this book, which deal with post-Edinburgh developments. They involve positions that abandon the strong explanatory ambitions of the Edinburgh School, for reasons that are largely left unstated but are probably akin to the ones I have articulated in this chapter. Such positions are in a predicament, *to the extent* that they still hang on to the ambition of naturalizing philosophy. We shall see how, with the promise of delivering a strict social explanation abandoned, this proves much harder to achieve.

14. We now come to the fourth condition of the Strong Programme, the reflexivity condition. It goes as follows:

> It [the sociology of science] would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle because otherwise sociology would be a standing refutation of its own theories.
>
> (Bloor 1976/1991, p. 7)

This principle requires that, no matter whether we choose to so apply them or not, the methods adopted by sociology of science must be applicable to the sociology of science itself, without raising issues of inconsistency or incoherency.

Unlike the principles examined earlier, there is no opposition to this one on the part of philosophers. There is widespread suspicion, on the other hand, that the Strong Programme does not actually live up to it, since its application to the programme itself will indeed generate incoherence, contradiction or pragmatic inconsistency. Charges to this effect have been brought against the programme time and again, but Bloor and Barnes have dismissed them all. Some versions are indeed easy to deflect. Thus, it has been said that in admitting the social origins of their thinking about science, the authors implicitly admit that their thinking is false. Bloor and Barnes have little patience with this. They point out that the critic commits the genetic fallacy, taking the theory's origins to prejudice its truth value (Bloor 1976/1991, p. 17 f). We shall see below that this dismissal is somewhat rash, however.

A common objection is that the Edinburgh programme brings a relativistic notion of truth with it, but that claim, too, seems unsubstantiated. At any rate, Bloor and Barnes explicitly oppose the relativization of truth (cf. Barnes and Bloor 1982). The situation becomes somewhat more precarious, however, when we turn from the truth of a theory to the question of its *justification*. Would not the fact that the genesis and adoption of a scientific theory in a given social group were determined by some societal parameter of that group imply that adherence to the theory is not justified? And do not the Strong Programmers incur a pragmatic inconsistency in trying to justify their position by invoking scientific evidence, while at the same time insisting that such evidence cannot outweigh the parties' social biases?

The following rejoinder to this challenge is suggested in various places in Bloor and Barnes's writings: By the logic of their own argument, Bloor and Barnes are as committed as their critics to the standards defining proper scientific procedure in the current era (Bloor 1976/1991, p. 44). They take upon themselves the strictest methodological precepts that their opponents might care to advance; indeed, they seek to emulate the most highly respected natural sciences (Bloor 1976/1991, pp. 141, 157, 160–161; Barnes 1982, p. xi). Their critics would hardly be justified in disputing the empirical findings of the Strong Programme, as long as these are generated by procedures that the critics themselves would accept as scientifically valid. The fact that, unlike their critics, Bloor and Barnes consider these norms to lack universal validity and transcendental underpinnings, should not undermine the validity of their empirical findings in the eyes of opponents who subscribe to the same methodological norms. What divides the two sides is a philosophical meta-issue irrelevant to the scientific findings.

This rejoinder does not meet the argument from justification head on but rather sidesteps it by an ad hominem move. This strategy might actually work, if the standards to which Bloor and Barnes appeal were generally shared in the scientific community over long stretches of time. This would be the case with Kuhnian "paradigms" or perhaps even more with Durkheim's "collective representations", which are supposed to inspire whole communities during an entire era. Unfortunately, the social parameters that are said to determine scientific theorizing are often much more restricted than this, locally or temporally. The crucial category, as we have seen, is "interest", which suggests something rather more short-lived and partisan. True, according to the term's Marxist origins, "interests" were fairly permanent and global items, "objectively" shared by entire social classes and as enduring as the latter. But in Bloor and Barnes's writings, the term "interest" is gradually stretched to cover a motley of different things, some of them very local and ephemeral indeed. As a matter of fact, Bloor makes it clear that an "interest" in the relevant sense may only be a scientist's concern for his professional reputation and his struggle to gain recognition for his pet hypothesis (Bloor 1983, p. 157, 1981, p. 203; Barnes 1982, p. 115). Here, "interests" may be purely individual, private concerns.[20] But this brings the reflexivity objection back in full force: The Edinburgh School can no longer coherently hope to demonstrate the merits of its programme by its "inductive" approach – that is, by empirical case studies – since the audience it addresses must now be considered irremediably divided by divergent interests, to a degree that makes most of the addressees resistant to the evidence put before them. The members of the scientific community no longer view the world through the same spectacles; people will remain blind to the evidence put before them, if their interests are not served by the conclusion they are invited to draw. The hope of achieving a scientific demonstration, as it is normally understood, is futile. Indeed, if the Strong Programmers managed to persuade opponents who were strongly entrenched in the orthodox conception of science of their case, it would have been proved that scientific evidence, carefully presented, may overcome contrary interests. The Strong Programme would thus indeed be "a standing refutation of its own theories", as it is put in the Reflexivity Condition, above. And whether or not it succeeded in convincing its opponents, it would incur a pragmatic inconsistency, merely by attempting to persuade people with opposed interests of the truth of its position, while holding, as a corollary of that very position, that such persuasion is impossible.

There is worse to come. The evidence marshalled in favour of the Strong Programme would not only lose its evidential force with respect to persons with contrary interests, but even for those whose interests would make them prone to accept it. Such inclination could not be taken as indicative of the truth of the hypothesis, but only of its conformity to that interest. No matter how persuasive, the evidence provided could not be taken to render the truth of that programme likely, and thus provide a *reason* for believing it. Strong Programmers must confess that they have no reason for their faith in their own conclusions.

We may give a more precise form to this worry. Methods of knowledge generation are supposed to be "truth-tracking", to use Robert Nozick's term (1981). That is, to count as knowledge, a belief must not only correspond to reality as a matter of contingent fact, but must be generated by a method that is likely to produce truths, in actual as well as counterfactual situations. This special provenance provides us with a *reason* to believe in its truth.[21] If the Edinburgh School is right, however, belief does not track truth, but rather tracks *conformity to the protagonists' social interests*; those two do not coincide, except by accident. Since there is only one truth, but a multitude of conflicting convictions, most of these must necessarily be false. Thus, the Strong Programme depicts science as a source of locally useful falsehoods, rather than general truths. Or even worse, of falsehoods that *flatter* local interests, rather than genuinely benefiting them; for theories can hardly track the benefits that would accrue to a given class of people in various scenarios, if those people cannot compute *which* scenarios would bring them *which* benefits. For genuine utility to emerge, we must assume that theory choice is not merely *swayed* by interests, but is *results from a correct calculation* of their satisfaction, identifying which of the available theories would bestow maximal benefit upon the group, if that theory were generally adopted.[22] There are two major problems with this assumption, however. In the first place, it takes us back to the kind of rationalist myth-mongering about science that Strong Programmers wanted to eliminate, only now with the ideal *theoretical* reasoner replaced by the ideal *practical* reasoner (cf. Fuller 1993, p. 106). Theory selection in science is depicted as the outcome of a process of rational choice modelling of truly staggering magnitude, where the benefits accruing to the group in question must be calculated in a number of alternative social scenarios of awe-inspiring complexity. There is not a glimmer of evidence that this kind of calculation ever takes place. Secondly, for such practical calculations to have any import, accurate information about the world must be fed into them; hence, the proposal presupposes that a large stock of truths about the world is available. But now we are back with the problem that, if the Strong Programme is right, there are no truth-tracking methods around to produce this resource. The suggested story implies that cognition is wishful thinking all the way down. Admittedly, beliefs thus generated may be true, by sheer chance; hence, the Strong Programme does not strictly imply its own falsify, as Bloor and Barnes correctly point out. But it is devoid of any rational basis, since its truth – if granted – could be no more than just a happy accident.

Could this argument be repudiated by the observation that Strong Programmers recognize the existence of pre-social modes of reasoning that might save the reality-tracking feature of science? In the article, "Natural rationality" (Barnes 1976), Barnes states that there are such natural proclivities of thought, which are a condition of man's ability to acquire socially transmitted cultural resources in the first place. As Barnes himself puts it, these natural abilities also allow man to subject to critical testing his culturally acquired framework for understanding the world: Following Hesse 1974, he describes man (alone or aggregated into a community) as an "inductive learning machine", who generates predictions within the conceptual framework handed down to him by the cultural tradition. If such predictions consistently fail, however, the framework itself will eventually come under critical scrutiny and will in the end be rejected and replaced by a different one. This is a process of trial and error, as we know it from Popper, and, with enough time and a lot of luck, it might in the end push our thought in the direction of conformity with the true structure of reality.

However, as we pointed out in Section 5 of this chapter, the cognitive process envisaged by Barnes will still be socially conditioned, and hence not reality-tracking, in the sense that the categorial framework in which a new theory about the world will be formulated, after the demise of its predecessor, will invariably be a socially conditioned one. There is no wresting ourselves loose from the social determination of the very conceptual scheme that society puts at our disposal. The role of the "inductive machine" is merely to say "yes" or "no" to proposed hypotheses, couched in terms that derive from outside itself – never to generate such hypotheses. (In Chapter 10, we shall explore the potential of Hesse's model once it is freed from the Strong Programme's strictures.)

15. The above argument highlights both points of similarity and of dissimilarity between Strong Programmers and Popperians, upon which it might be instructive to dwell for a moment. Both emphasize that theories are not extracted from reality by some procedure such as induction or abduction. Induction, according to Popper, is sheer fiction, even as a psychological process (Popper 1963b). Thus, Popper would fully agree with Bloor and Barnes that theories – including the vocabulary in which they are couched – are not elicited from the reality observed. Only when a theory is already at hand does observation come into play, subjecting our free conjectures to the harsh discipline of testing. What testing accomplishes is only to say "yes" or "no" to our theories. (Popper would add that only the latter outcome has interesting epistemic consequences for our theory.) This is quite similar to the position of the Strong Programme, so far.

What distinguishes Bloor from Popper is the former's claim that the categories in which we describe the world theoretically are determined by the social context within which scientific investigations take place: "the social component of science is the theoretical component". Popper, on the other hand, believes that theories are free creations, manifestations of the genius of such figures as Newton and Einstein. In the Strong Programme, there is an underlying urge to debunk the status of such celebrated figures; they are better construed as conduits through which the *Zeitgeist* expresses itself. Given this difference, it is no surprise that Popper is highly critical of the sociology of science, both in its early versions with their Marxist roots, and in its later incarnations (cf. Popper 1945, Chapter 23).

The above conclusion has general import. We may grant that the Strong Programme does not assert that interests are the exclusive determinants of theory choice, but reserves a certain role for other factors, in particular observational data. This creates, in principle, an opening for reality to influence theory choice and thereby, for scientific theorizing to be "truth-tracking" after all. But once this kind of extra-social determining factor is admitted, the Edinburgh position faces a dilemma. *Either,* it will collapse into something indistinguishable from a standard fallibilistic (Popperian) position to the effect that, although science is a highly precarious business even at the best of times and endlessly corruptible by social influences, we are allowed to hope that the latter will filter out in the end, given proper institutional safeguards of the scientific process of trial and error. *Or* Strong Programmers will stand fast on the thesis about the social origins of the theoretical component in science, thus saving their position from trivialization, but at the cost of sacrificing science's truth-tracking propensity. Science now tracks certain *social factors* rather than reality. There is no jumping between the horns of this dilemma.

16. However, the charge that the Strong Programme's construal of science precludes the latter from being truth-tracking misses its target, it might be said, since Strong Programmers are *instrumentalists* with respect to science and hence are not concerned with truth in the first place, but rather *usefulness*. "Usefulness" here might be taken to mean precisely, "tendency to further the interests of the group adopting the theory". This might be thought to render the overall position admirably coherent. Adoption of a theory is indeed dictated by societal interests, not by conformity to reality; but the goal of the exercise was always to further such interests, rather than to gain truth, which means that there is no conflict between the goal and the means employed to reach it.

In a sense, we have already dealt with this argument above when we pointed out that, on Strong Programme premises, scientific theorizing cannot even track social utility, but merely the *vacuous promise* of such utility; systematic pursuit of the real thing is rendered futile by the lack of objective data and theories to guide the search. Besides, the suggested interest-driven instrumentalism surely runs afoul of the Reflexivity Principle, which is the object of our current examination. Strong Programmers can hardly hope to win any converts to their cause if they openly concede that their position never laid a claim to truth, but merely to usefulness with respect to its protagonists' goals (such as fostering a fairer accommodation between science and society). Strong Programmers must either declare that their sociological findings are not themselves to be construed instrumentalistically, thereby infringing the reflexivity condition and becoming a "standing refutation" of their own programme, or they must apply the instrumentalist analysis to their own practice, which will put people with different cognitive interests at liberty to ignore their results. Thus, the self-refutation problem is back with a vengeance.

As a last line of defence, one might propose a rather abstract philosophical argument that is nowhere explicitly articulated in Strong Programme writings but is hinted at in various parts of the body of work (Bloor 1976/1991, p. 37 ff, 1999a, p. 89 f): The idea of "truth-tracking" as a meta-constraint upon scientific method, to which our accepted standards of theory choice are somehow answerable, gets things back-to-front; it assumes that we have an independent line into truth that permits us, when choosing methods of inquiry, to select those that are most likely to generate truth. But it is really the other way around: our only line to truth is via our accustomed scientific procedures. It is idle to speculate whether or not those standards of rationality are *really* reliable (that is, whether or not they *really* make their conclusion likely to track truth) since we have no independent access to the latter (cf. Wittgenstein 1953, §§ 482–484.) All we have to go on are the methods that are standardly accepted in our society as providing "good reasons" and are held, as a consequence, to "make the truth of their conclusion likely". There is thus no "higher" notion of rationality than conformity to standardly accepted epistemic practices. The only rational commitments are *social* commitments. (We shall see in the next chapter how such a conception could be based upon Wittgensteinian arguments by which the Strong Programme sets so much store.)

Now, this rejoinder might be to the point (which is not to say it is compelling), if Bloor had been charged with providing an a priori guarantee that our cognition establishes contact with reality. This Cartesian undertaking is indeed a famous dead end and invariably ends in scepticism. Bloor is right that we are forever barred from checking the validity of our thinking by direct comparison with reality, unmediated by our cognitive apparatus and our epistemic principles. There is no Archimedian point, nor, to use Rorty's suggestive imagery, is there a "sky-hook" that will lift us out of our cognitive predicament (Rorty 1991, p. 13). We have to take the basic veracity of our thought for granted, if we want to think at all, saying, with Wittgenstein, "This is what we do". But the Strong Programmers' predicament is not the general human predicament; their problem is *specific* to their position, because the latter compels us to draw the awkward conclusion that human thought produces not truth, but mostly pleasing untruth. This is supposed to be the upshot when human thought examines itself in the systematic, disciplined way we call science, only to discover that human cogitation is largely determined by social interests and couched in conceptual frameworks of socio-cultural origin. Starting out with the assumption that we have a line into truth, the argument produces a conclusion that undercuts that assumption. The outcome must inevitably be that the Strong Programme undermines its own credibility.

I conclude that the Strong Programme fails to satisfy its own principle of reflexivity. This is more than just a minor irritant, a result of having pitched one's epistemic standards too high, and to be handled by simply dropping that requirement. The reflexivity condition is not an optional feature of the Strong Programme, but rather a general epistemic principle to be respected in any intellectual enterprise. The failure of the Strong Programme to live up to this principle betokens a major difficulty for that effort. Add to this the obstacles to the programme's explanatory ambitions that we have pointed out previously, even in cases where reflexivity is not an issue, and the conclusion is inevitable: the Strong Programme in the sociology of knowledge is one that can never reach fruition.

Chapter 4
The Strong Programme as Naturalized Philosophy

1. In the previous chapter, we examined the Strong Programme as an effort within the sociology of science, defined by the four principles articulated in Bloor's *Knowledge and Social Imagery*. The viability of that programme was supposedly shown by its vaunted record of explanatory successes. As Bloor puts it, the programme represents an inductive approach (Bloor 1981, p. 206). If the programme were to succeed, however, this would also spell the death of orthodox philosophy of science, since the ideas in which the latter trades would be proven supererogatory, or at best epiphenomenal. There is as little use for them as the notion of an ethereal mind or "soul", once we can explain every fact of human action and thought by reference to purely neurophysiological states. The Strong Programme is thus simultaneously an effort towards the naturalization of philosophy of science.

However, even putting aside the worries about reflexivity, we found that the programme's empirical claims were unfounded. The celebrated case studies all deliver something less than alleged. They are merely accounts of the *reception* of theories, not of their *genesis*. There is even worse to come: closer reflection shows that these cases all deal only with the *early stages* of the life cycle of a theory, not its subsequent course. While it may indeed be true that phrenology was adopted by the Edinburgh working class for its congruity to its needs, nobody in Edinburgh today, even in the working class, subscribes to the theory. And while it may be true that quantum mechanics was initially embraced in Weimar Germany because of its affinities with the prevailing romantic, anti-deterministic *Weltanschauung*, this connection is hardly what sustains the theory's worldwide acceptance today. Thus, the Edinburgh story seems largely irrelevant with respect to the status of established, mature science (cf. Laudan 1984a; Fuller 1988, p. 239 ff).

Let us consider a rebuttal to the latter objection. Once a theory is established in a given class, it may promulgate itself to other classes or other societies in which it could never have arisen in the first place, especially if the original host class or society is a powerful one. This would basically be the familiar story about how dominant classes insinuate their ideas to less resourceful ones and how strong nations impose their customs on small and weak ones. Yet one might accept this as a valid observation on human history and still maintain that a purely general cognitive mechanism is at work as well. If reality is allowed to exert a pressure upon our theories, however weak, mediated by experience, then the interests that motivated the early enthusiasm

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for a false theory will eventually yield to the impact of accumulating negative evidence; interests will eventually filter out. This filtering-out generates theories that are not geared to specific interests, but which are universally useful.[23]

As a matter of fact, Bloor himself articulates a similar, more specific point (Bloor 1983, p. 155 f), though only in order to rebut it: The professionalization of modern science may be thought to have rendered scientific theories invulnerable to influence from such ideological interests and metaphysical predilections as are illustrated by the celebrated case studies. Today's science is embedded in an elaborate institutional framework of evaluation and control, designed precisely to neutralize such biasing influences and to safeguard the objectivity of science.

To the extent that the Strong Programme was purely "inductively based", it ought to accept such historical facts gracefully. Moreover, the transformation of science would in itself be a phenomenon worthy of sociological explanation. Science Studies might savour its (supposed) explanatory successes with respect to past science, ranging from the phrenology study to the Weimar case, while at the same time admitting that science is now conducted in a different manner, which would be equally deserving of sociological scrutiny.

2. However, this is not the path taken by STS. Instead, Bloor falls back upon a second line of defence for his Strong Programme, in the process giving that programme a fateful philosophical twist. He invokes certain "first principles of Wittgenstein's work", notably that meanings are inherently social. This theme plays a crucial role in the writings of the Edinburgh School, where it goes under the name of *finitism*. Yet it is striking that the notion of finitism does not occur in the original, 1976 edition of *Knowledge and Social Imagery*, but only appears in the Afterword added in the 1991 edition as well as in various interim publications. Nevertheless, Bloor calls it "probably the single most important idea in the social vision of knowledge" (Bloor 1976/1991, p. 165). It is invoked to show that, no matter how high we build the institutional walls to shield science from external influences, it remains at its core a phenomenon the motive forces of which are predominantly social. In so arguing, Bloor moves from an empirical ("inductive") argument for the Strong Programme to an argument that is basically philosophical. *Pari passu,* he also shifts to the last of the four readings of the term "social" listed on p. 44.

Bloor's philosophical turn is rife with ironies. It deploys philosophical arguments, yet is motivated by the goal of putting an end to philosophical reasoning, if perhaps only with respect to the philosophy of science. The appropriate strategy for naturalizing this discipline, however, would surely itself be naturalistic, that is, the simple demonstration that purely scientific explanations could be provided for the progress of science, without any involvement with the philosophers' favoured categories, such as "evidence", "justification" or "rationality". This demonstration could most elegantly proceed *ad oculos*, that is by placing before us a long list of cases in which such explanation is actually achieved. But this "inductive" strategy faces crucial obstacles, as we saw in the previous chapter.

Instead, another strategy is adopted whereby philosophy is used to subvert philosophy. This may sound like a contradiction, or at least a pragmatic inconsistency, but is not strictly so, as long as it is a matter of using one, supposedly more robust, part of philosophy to break another, more fragile part (the way that epistemology and philosophy of language have so often been used to undermine the pretentions of metaphysics). In the present case, Bloor uses philosophy of language to drive out rationalistic philosophy of science. As it happens, there exists a line of philosophical thought that is not only useful for that purpose, but even promises to self-destruct after having been used to eliminate other parts of philosophy, leaving nothing behind. We are, of course, referring to Wittgenstein's philosophy, which, both in its early, Tractarian version and the late version from the *Philosophical Investigations*, was designed to end all philosophy. Famously, in the final paragraphs of the *Tractatus*, Wittgenstein not only dismisses all the traditional questions of philosophy as essentially meaningless, but, in a dramatic flourish, ends up declaring his statements to this effect, along with the whole of the *Tractatus*, to be equally meaningless. The performance is repeated in the *Philosophical Investigations*, where Wittgenstein declares that there is no such thing as a philosophical thesis. The core of Wittgenstein's argument to this effect in the latter work are the so-called "rule following considerations". These are precisely the ideas invoked by Bloor to provide the Strong Programme with an incontrovertible grounding, one that is not hostage to historical changes in the way science is conducted. Let us now examine how efficient Wittgenstein's philosophy is for that purpose.

3. We recall from Chapter 2 that the rule-following considerations were taken to demonstrate that only an actual communal consensus fixes what is to count as the correct extension of any rule-governed praxis, not exempting logic and mathematics. The relevance to the sociology of science of this conclusion is not transparent, however, as the difference between *norms* and *facts* creates a barrier for its immediate deployment in aid of the Strong Programme.

According to the rule-following considerations, it is the normative notion of a *correct* use of a concept that calls for a strictly social, conventionalist definition. Nothing has been said so far, however, about what determines any particular *actual* (and possibly incorrect) application of the concept: Wittgenstein tells us how a normative standard is laid down, not what determines actual happenings in the world. But what sociologists of science are concerned with is primarily the latter; this goes for the Strong Programme in particular, which is committed to sweeping away all normative, idealized reconstructions of science in order to depict its messy reality. Thus, there is a gap between a normative and a factual concern that needs to be bridged. This gap is nowhere explicitly thematized by Strong Programmers, and most often, the distinction is just slurred over.[24]

Before we move on, we should put to one side a sense in which Wittgenstein's argument does indeed imply that it is the *actual* use of concepts that is determined by community consensus, not only the correct one; unfortunately for Bloor, it is not the sense he needs. Judgements and other conceptual operations are intentional events, defined by their content. In the Wittgensteinian picture, intentional content is largely indeterminate, as long as we look at the individual language user, and awaits fixation courtesy of the community as a whole. The individual may harbour the intention of "adding 2 to the preceding number"; but only the agent's membership of a linguistic community determines how an action by that description is correctly carried out and, hence, *what the intention of performing it really amounts to*. When intending to perform the action of "adding 2", the individual intends (among other things) to do something that could correctly be described as "the act of adding 2"; and on the communitarian account, this means something that the rest of the community would agree to call "adding 2". The agent's intention is *deferential* with respect to the community; it contains, as it were, a blank, to be filled in by communal consensus as to what the *right* way of "adding 2" amounts to. Thus there is a legitimate move from the normative to the factual here: The communal consensus determines the *actual content* of individual intentions.[25]

This conclusion, if valid at all, extends to all intentional states, not only intentions in the narrow sense. Thus, it applies to all verdicts and other cognitive states, including the cognitive states that are involved in doing science, such as inventing new theories and testing them. The contents of these states are fixed by communal consensus and, hence, by social determinants. Thus, the content of the scientific process is determined by social factors, *in a sense*.

Unfortunately for the Strong Programme, this is not the sense we are concerned with when we do the sociology of science as a causal, scientific enterprise. (Our suspicions should have been raised by the very claim that a purely *philosophical* argument, pertaining to the nature of meaning and intentionality, could demonstrate that the proper categories to be employed in a causal explanation of science must precisely derive from *sociology*.) To think otherwise would betray a confusion between a *metaphysical* and a *causal* account of the way meaning and intentional states are fixed. It is true, in the Wittgensteinian picture, that (at least part of) what fixes the content of individual intentional states is group consensus; this is, indeed, a social phenomenon. However, fixing here is by way of *supervenience*, a matter of a social context bestowing an additional description upon an individual, psychological state of affairs. But what we are concerned with in the Strong Programme is *causal* fixation (causal determination) and *causal* explanation; and this issue has so far not been addressed. It does not follow from the finitist story that the *causal genesis* of individual (or indeed communal) concept applications (and intentional states in general) is to be explained in social terms; it does not follow, that is, that the determinants of the consensual (or dissensual, as the case may be) individual applications of the concept in question are social forces.

Thus, it is entirely consistent with the Wittgensteinian picture that the causes of conceptual usage might be purely physiological, through and through. It is even consistent with the Wittgensteinian picture that a particular language (say, English) were innate to man, so that no social process of language learning (Wittgenstein's "drill") would be needed to start off the individual's career as a speaker and such that utterances of perceptual reports were strictly and tightly keyed to physical cues (irradiation of sensory surfaces). It would still be conformity in use that constituted the *correctness* of every single application and its content would still be (partially) fixed by social consensus about the use of that concept. (Evidently, conformity does not follow logically from the hypothetized innateness of the concept; as with all psychological traits innate to man, there might be considerable individual variation.)

In actual fact, natural languages are not innate, of course, and linguistic command is inculcated through a process that is clearly social. Bloor describes this process in considerable detail. Following Wittgenstein, he stresses how it involves not explicit instruction and definition, but rather mere *drill*; that is, a disciplined learning exercise involving confrontation with the objects that instantiate a given term. But this only serves to show the social nature of language learning, not of language use as a going concern. In particular, it says nothing about how disagreements can arise among the participants of an established linguistic practice. The social nature of language acquisition is quite consistent with actual usage being perfectly uniform across the community and, in particular, highly resistant to social influences that might disrupt that uniformity.

Thus, there are serious obstacles to any attempt to build a social approach to science directly on Wittgenstein's finitist theory of meaning. The project would be greatly aided by a demonstration that socio-*causal* determination follows from the socio-*normative* determination established in Wittgenstein's rule following considerations; yet what we find in the Strong Programme is rather a conflation of the two. In the chief Edinburgh School texts, we find clear evidence of the steps by which the authors tacitly move from one thesis to the other. Wittgenstein's rule-following argument is typically glossed as the claim that the future application of any term (any concept) is not "logically" or "rationally" predetermined by its previous use. Strong Programmers conclude that the determination must hence be social, a move that is not so much an inferential step as the result of a simple confusion of normative and causal determination. (We find a clear example of this transition in Bloor 1976/1991, pp. 164–165.) In particular, usage will be susceptible to influence from participants' social interests; the variation engendered by these is *eo ipso* explicable in social terms. When the usage in question pertains to scientific terms, the explanation will be provided by the sociology of science; thus, by small steps largely left tacit in the text, we have arrived at the main thesis of the Strong Programme. But these steps in no way follow from Wittgenstein's rule-following considerations; as a matter of fact, a cogent argument can be made that Wittgenstein would not have endorsed the play that Bloor makes with the alleged variation in language use. We shall return to this issue in Sections 11 and 12.

4. The rule-following considerations are not the only Wittgensteinian idea mobilized by Bloor in support of his social account of linguistic variation. The notion of *family resemblance* is invoked, too. Concepts have multiple dimensions: To use Wittgenstein's own metaphor, they are like lengths of rope woven together from many strands of fibre; hence, a person's problem in confronting a new application of a concept is not merely how to project each single strand onto the novel case, but also how to weigh the importance of the different strands against each other. The extension of concepts to new instances may accentuate similarities and analogies in different ways. The novel case may show clear disanalogies to previous applications with respect to one strand and the application might yet be justified in terms of

the smooth extension of analogies for the rest. This, according to Bloor, will easily lead to divergence of application. And disagreement is precisely what invites social explanation.

In Barnes et al. (1996, p. 54 ff), still another line is adopted, in which the *analogical* or *metaphorical* nature of concept use is stressed; this is a thought deriving from Mary Hesse, rather than from Wittgenstein. The use of concepts (classifying terms) is not guided by a strict notion of (qualitative) identity between the original defining paradigm and each new case, but instead by partial similarity or analogy, or by metaphorical extension. This looseness generates considerable freedom when concepts are applied to novel cases, creating an entry point for social influences. Thus, according to the authors, the meaning of terms becomes a dynamical phenomenon that is essentially social, since, as time passes, one analogical extension is built upon another in an endless process. Each extension adds to and modifies the sense of a term, which thus becomes a repository of its own developmental history. And that history is essentially a social phenomenon; or so we are told.

These arguments add further dimensions to the flexibility of concept usage beyond that demonstrated in the rule following considerations. They still fall short of establishing that this variation is *socially* conditioned and socially explicable in any substantial sense, however. The supplementary arguments imply that there is considerable leeway for interaction between linguistic usage and interests or other social influences, but without demonstrating that such interaction actually takes place.

Any serious discussion of this matter would do well to take into account the quite substantial empirical literature that has emerged in this field. As it happens, Bloor and his co-authors briefly touch upon this literature in Barnes et al. 1996 (op. cit., p. 6 f), but only to report a finding that goes somewhat counter to their main claim. Recent findings in cognitive science suggest that, at a basic level, the vocabulary in which human beings conceptualize nature is rather immune to social penetration; in the jargon, this vocabulary constitutes a psychological "module" shielded against external influence (Fodor 1983). They nevertheless conclude, rather lamely, that concept use may still safely be assumed to be socially variable.

As it happens, Bloor might be have been able to find genuine support for his communal theory of conceptual variation in other recent work in cognitive semantics. Some of the most influential work in this field is the theory of conceptual structure developed by Eleanor Rosch (cf. Rosch 1981). Rosch's research revolves around the notion of "prototypes", which encapsulates the finding, not accommodated within traditional semantics, that certain members of a semantic class are deemed by native speakers to be more central and typical than others. The prototype effect is taken to be the surface manifestation of deep cognitive structures; one suggestion, from George Lakoff (1987), postulates items named Idealized Cognitive Models, which we may illustrate with Fillmore′s well-known example of the term "bachelor" (1982). According to traditional semantics, which tries to capture a concept in terms of simple necessary and sufficient conditions, we might define a bachelor simply as an unmarried man. But this overlooks that the term is employed within a web of general and somewhat idealized information about the society in which unmarried men live. This web specifies that people normally live in some sort of a social network, not in total isolation, that bachelors are not committed to stable relationships with women partners, that most men are heterosexual, that men normally do not marry before the age of 18, and so on. This engenders uncertainty when we come across cases that deviate from the normalcy defined by these principles. For instance, is a young Robinson Crusoe, washed ashore on a desolate inland and living the rest of his life in solitude, a bachelor? Is a man living in a stable homosexual partnership a bachelor? The answer is moot, at least, which suggests that the definition in terms of simple necessary and sufficient conditions fails to capture the complexity of the semantics involved, especially the way it encapsulates a rich background of empirical information.

Such networks of background knowledge may, quite naturally, be assumed to be socially variable. It is fair to assume that the background theories with which concepts are enriched will often hark from different sources, explicitly or implicitly. Our background knowledge about how bachelors live and behave may vary considerably according to our walk of life. In this way, Rosch's theory of concepts links up with what Putnam has termed "the linguistic division of labour" (Putnam 1975), which is the fact that our terms get their meaning from what the experts in the field hold true. In Putnam's highly idealized picture – which is only an intuitive sketch – it is tacitly assumed that there is only one set of experts in any field, that there is no uncertainly as to who they are, and that their expertise is never contested. These seminal ideas have to be developed into a genuinely sociological theory, which would be rich enough to deal with situations in which there is disagreement about who holds authority and where interests, power relations and other sociological forces play a role. A socio-linguistic theory of this kind would go some way towards showing how differences in the use of concepts can have a genuinely social explanation.[26]

We have strengthened Bloor's theory of concept use with some genuinely empirical material, as befits a naturalistic approach. This material supports the conclusion that the basic mechanism of concept application is open to socially conditioned variance. Thus, we may pick up Bloor's discussion again to see to what use he puts that conclusion.

5. Bloor and Barnes's reflections on concept formation form a key part of their overall picture of science. That picture is fairly orthodox in its basic elements: Science is seen as the enterprise of formulating regularities, or laws, concerning empirical phenomena. Formulating such laws often requires the use of theoretical concepts, which will be forged out of the conceptual materials available in the culture, typically by way of analogical extension or refinement of concepts already in use in more mundane sectors of communal life. Science is basically the cognitive handling of reality by means of conceptual resources inherent in, and indigenous to, each particular culture. As Bloor puts it, "The theoretical is the social". As we saw above, there is some vacillation in the Strong Programme as to how deep social influences penetrate the vocabulary of basic observational terms. However, there seems to be no retraction from the tenet that the higher reaches of theoretical vocabulary are fully socially conditioned.

While basically quite mainstream in its conception of the internal structure of science, however, the Strong Programme still sets itself apart by its very strong focus on the *classificatory* aspects of science (Barnes 1982, pp. 23, 67). Knowledge is fundamentally a matter of classifying the items we encounter in our dealings with the world; in the case of science, these encounters typically involve experimental handling. In abstract scientific thought, classification is primarily a matter of the application of technical linguistic terms. Such terms are mostly not learned by explicit definition, but through "drill", which, in science, means the assimilation by the novitiate of the discourse that accompanies scientific procedures, such as conducting experiments.

Learning activities of this kind are open to sociological explanation; the resources needed for this, according to Bloor, are basically the very same that will explain the initiation of novices into any other social practice. In particular, the representational function of science – the fact that it describes reality – plays no significant role and gives it no special status, as compared to such non-representational activities as playing music, dancing, or engaging in sports.

We note in passing that Bloor's account of the way concepts are learned is very simplistic and mechanical, sorely in need of updating in terms of more recent developments that show the process to be much more complicated and with much greater cognitive involvement. It overlooks the obvious fact that the unobservable, theoretical concepts of science can hardly be learnt by ostension, as their contents reside chiefly in their conceptual interconnections with other concepts, which means that they are learnt *pari passu* with the learning of theories.[27] However, we observed above that Bloor's point may actually be sustained by invocation of Rosch's results.

As science progresses, novel results and findings constantly appear, which in the normal run of things will be assimilated effortlessly into the conceptual network. But occasionally, findings will come up which put a strain upon it, as they resist being pigeonholed into the framework that scientists learn as part of their training. The different strands of which the concepts are composed – compare our remarks above about family resemblance – will pull the efforts to extend them in contrary directions. This is where interests get a foothold and where another chapter of sociology becomes relevant, for the decisions will inevitably be made on the basis of the divergent interests in the scientific community. The protagonists will decide the issue on the basis of their individual interests.

In radical cases, novel theories will have to be introduced to cope with the situation. Although such theories will break with past normal-scientific practice, their basic concepts will still derive from resources contained in the general culture. The new concepts, like the ones they replace, will be analogical extensions of the concepts used in the previous theory, or of other conceptual resources available in the culture.

6. In Bloor's Wittgensteinian picture of science, adjustment of our scientific theories to recalcitrant observations is thus primarily a matter of modifying the *meaning of the terms* employed, be those terms observational or theoretical; this ties in with

Bloor's emphasis on the classificatory aspect of scientific knowledge. It seems difficult, on the Wittgensteinian picture, to construe scientific disputes as being about matters of non-linguistic fact, or as pitting two (or more) factually different pictures of the world against each other. This is apparent from the fact that the two ostensibly rival theories would really be *compatible* and, hence, would not express alternative conceptions of the world.

To substantiate this claim, let us invoke the example used by Wittgenstein to demonstrate the social nature of rule following (cf. Chapter 2), but modified slightly to fit our current concerns. A computer prints out sequences of natural numbers and two persons, A and B, try to guess at the formula generating them. The computer starts by printing out the sequence "2, 4, 6, 8", and both persons volunteer the answer "add two" as the generative formula. This guess is corroborated by the subsequent figures emerging in the printout. However, after reaching 1000, the computer continues thus: "1004, 1008, 1012..." Now, A concludes that his original guess was wrong; B, however, sees no problem: To B, this continuation indeed is what "adding 2" amounts to, once we get beyond 1000. In this case, we are compelled to conclude that the protagonists use the expression "adding 2" in different senses. The way we set up the example, there is clearly no room for other ways to account for the disagreement (for instance, by postulating that one of the parties actually misperceived the original numbers on the printout, or was hallucinating). The issue is solely whether going on in the manner of "1004, 1008, 1012..." is to be *called* "adding 2". If a consensus is not reached, we are forced to conclude that the participants use that expression in different senses.

Now, let us consider an example closer to real-life science. Suppose a team of chemists is conducting an experiment which, according to theoretical predictions, will leave a green precipitate in the vial. Instead, what emerges is something indistinguishable in colour from ripe tomatoes. Determined to defend their pet theory, the experimenters dig in their heels and insist that the substance is, indeed, green. A rival faction of the scientific community, however, is equally adamant that the substance is red.

Standing on the sidelines, the finitist philosopher will invoke Wittgensteinian license and insist that the decision is not forced upon the protagonists either way: Neither alternative is dictated by the previously established meanings of the terms involved; hence, social interests will determine the outcome. Now the crucial point is that on such a philosophical analysis, the quarrel is solely over language, not reality. There are two ways in which the disagreement might be resolved in a consensus: the experimenters could persuade their critics that the stuff is indeed green, or the critics could make the experimenters concede that it is red after all. In the first case, the experimenters might hold on to their hypothesis, for example, to the effect that when two substances S_1 and S_2 are mixed, a third substance, S_3, of green colour will emerge. In the second case, the experimenters will be forced to adopt a different hypothesis, to the effect that when S_1 and S_2 are mixed, a red substance, S_3, will emerge. If a consensus is not reached, however, either side will take its opposition to be introducing a "bent" rule for the predicates "red" and "green", respectively, along the lines (roughly) of "use the notions of "red" and "green" as usual, but switch them around for substance S_3". The experimenters will hold that their critics are bending the meaning of "redness", turning it into a new predicate, "redness_b", while the critics will hold that the experimenters have introduced a bent notion of greenness. The crucial point is that the scientific hypothesis that says that mixing S_1 and S_2 will produce a green substance is now *compatible* with the hypothesis that posits a red substance, as long as one of the hypotheses adopts a bent rule for red or green. Being red_b is *compatible* with being green, with respect to the substance in question; as a matter of fact, it implies the latter, for the particular application in question. And, again, it is vice versa for greenness (greenness_b). It could hardly be otherwise, since all the evidence is available to either side and misperception is no part of the story. Any disagreement between the two parties must perforce come down to a difference of semantics.

Notice that this conclusion is not forthcoming on a traditional, non-Wittgensteinian notion of rule-following. Here, we are compelled (except in cases of ambiguity) to say that (at least) one of the two hypothetical consensuses would simply be *mistaken*: Either the red-sayers or the green-sayers would have got things wrong – perhaps inexplicably so. But on the Wittgensteinian view, the community cannot go wrong, it simply goes; if there are two rival communities, then *both* are right, by their own lights. This I take to define the communitarian interpretation of Wittgenstein, which is the position adopted by Bloor. But this means that the whole issue now comes down to a case of linguistic indeterminacy.[28]

7. The above argument depended among other things upon the assumption that no perceptual mistake was involved on either side. More generally, we assumed that the disagreement did not result from a difference in the evidence available to the two sides; they were both assumed to have all the relevant evidence in hand. This assumption was plausible with respect to the concrete example, which involved an observational quality (colour). But, in science, observational qualities interest us mainly because they serve as evidence for unobservable, theoretical states of affairs. For the latter, the assumption of total evidence will never be fully satisfied, since theoretical issues transcend such evidence. Does this open the door for the possibility of rival interpretations of experimental results that do not simply boil down to differences of meaning?

This question touches on familiar and heavily contested issues of realism and anti-realism in recent philosophy. Here, our concern is merely whether the notion of irremediably undecidable theoretical questions has a place within a Wittgensteinian view such as that adopted by Bloor. I believe the answer is no. Wittgenstein's philosophy of language is governed by a particularly strong version of the *publicity principle*, to the effect that speakers may only be allowed to possess a grasp of a concept if they can manifest it publicly. This is the lesson supposed to be learned from the Private Language Argument; that issue is closely related to the one under consideration here. Consider in particular Wittgenstein's argument that the use of a term "S" to designate some private mental state would acquire meaning if, and only if, it were connected with some external sign, say, a rise in blood pressure (Wittgenstein 1953, § 270). Once this connection is established, however, the alleged private item drops out as irrelevant, leaving only the public aspect as a genuine part of the language game. In a similar fashion, if there were publicly observable indications for the presence of some theoretical physical state T, speakers' reaction to those indications would at the same time constitute the evidence for speakers' grasp of the concepts of T; the totality of such reactions would define the full content of that concept. Any allegedly private states of understanding in the speakers' minds, which on a mentalist reading would be what actually constituted the understanding, would drop out as irrelevant. This is basically Dummett's "manifestation argument" for an anti-realist position (Dummett 1975). And something like this seems actually to be adopted by Wittgenstein.

Thus in a Wittgensteinian picture (given the communitarian interpretation), we do not choose between divergent commitments when, in the face of full evidence, we decide to apply a given predicate P_1 to an object O, rather than another predicate P_2, even though it is agreed by all sides that the two predicates are incompatible with respect to all their previous applications. From the point of view of either side, the opposition will appear to have adopted a "bent" rule with respect to O. But there is no objective standard of "bentness" to which to appeal; any application of a term will be "straight" to those who accept it and "bent" to the rest of the community.

8. Thus two radical consequences seem to follow from Bloor's "finitist" conception of science. First, when we invoke Wittgensteinian license to argue that two (ostensibly) divergent scientific hypotheses may still both conform to all the evidence, those two hypotheses will not be factually, but only verbally, distinct. Second, the scientific terms involved in such cases become semantically indeterminate, since their meaning becomes fuzzy enough to encompass both of the apparently divergent interpretations.

So far, this conclusion has been stated with respect only to situations in which one side of a scientific debate persists in defending its position by what the other side regards as a bending of the semantics of the terms involved. But of course there are many situations in which no Wittgensteinian subterfuges are invoked and, in such cases, choice of one theory over the other might seem to be a real one, with genuinely different consequences. Scientists will notoriously sometimes bow to an unexpected and unwanted experimental result, even though resistance would have been possible by "bending" the description of the outcome.[29]

Actually, the implications of the argument pertain even to such cases. The gracious *accept* of an unwanted experimental report does not suffice to bestow a precise sense upon the linguistic terms used, so long as its rejection would had been equally legitimate; obviously, the *accept* of an experiment is equally unhelpful in this respect if it simply reflects the protagonists' vested interest in that outcome. What is needed is that the decision to accept the result be somehow forced; that the semantics of the term is firm enough to rule that rejection of the result would constitute an infraction of its meaning, leaving accept as the only admissible option.

Thus the argument from Wittgenstein's rule-following considerations places Bloor in a dilemma. One option is to maintain that, in every scientific controversy, either side may persist in describing an experimental outcome in a way that saves its position from refutation, without thereby breaking any rules of semantics. It does not matter if people actually adopt this strategy; what matters is that the semantic framework within which they operate is lax enough to allow it. If this is maintained, decision in science is unfettered by semantic constraints; this, by default, leaves it open to social (and other) influence. But, by the same token, it follows that the choice is no longer between genuine rivals; the alternative theories turn out to be semantically equivalent.

The other option is to admit that we do not enjoy such total semantic freedom: beyond a certain point, refusal to accept a particular description of an experimental result will violate semantic rules. In this case, the semantic distinctness of the rival hypotheses is saved; it can be granted that the controversy has a substantial content. But at the same time, an avenue is opened up for the influence of reality upon the scientific process, mediated by an independent factor that is not just one more tool of partisan interests. It does not matter if this influence is only rarely decisive and if, most of the time, the scientific establishment will dig its heels in to resist unwanted experimental results. As long as this influence is sometimes effective, the development of science will be answerable to reality, hence, it will not only track social interests. Indeed, given suitable methodological strictures to counteract the immunization of theory against criticism, along the lines suggested by Popper, one might even strengthen the process through which science slowly approximates accurate depiction of reality.

9. It might seem that in developing this argument, we have really been breaking down a door that was already wide open. For even though there is no sign that Bloor ever considered the above argument, there are plenty of indications that he would cheerfully embrace the semantic indeterminacy that flows from it. Indeed, this conclusion follows directly from Bloor's preferred way of glossing Wittgenstein's rule-following argument: it says that nothing in previous applications or representations determines the next application of a term T. It follows that this new application does not – even combined with all the previous applications – determine the next application of T subsequent to *it*; nor does it determine the next application of any term *other* than T, to which T is linked. This is as much as saying that a person does not undertake any determinate semantic commitment by applying a term T to any particular object; this, again, is to say that T does not have any determinate meaning. Bloor seems to accept this consequence with equanimity. For instance, in Barnes et al. 1996, p. 55, Bloor and his co-authors state: "On a finitist account there is nothing identifiable as 'the meaning' of a kind term, no specification or template or algorithm fully formed in the present, capable of fixing the future correct use of the term... . *In a sense, we never know what our kind terms mean...* " (my italics). Most philosophers would recognize a *reductio* in this conclusion. It seems to reduce science to an activity that, although manifesting a strict and complex coordination between the actions of a plurality of people, and requiring a long period of drill for its proper performance, is still utterly "empty", from a cognitive point of view; it is a bit like square-dancing, or playing a rugby match.

Thus, it seems that Bloor does not quite know which leg to stand on when invoking the rule-following considerations. Wittgenstein's argument has a negative aspect, which is the elimination of the idea that conceptual (linguistic) usage is governed by Platonic essences, and a positive one, which is the demonstration that drill and social practice take over as determining factors in the absence of such essences. Wittgenstein is quite insistent that meaningful discourse is dependent upon the possibility of firmly judging certain cases of language use to be wrong; it is precisely the lack of this aspect that robs private languages of any content. Socialization into language establishes a conformity in language use, on the basis of which the occasional deviation may be identified and criticized; such censure precisely proves that we are dealing with a rule-governed practice and not a mere factual regularity, such as coughing or sneezing when exposed to a draft. Bloor seems occasionally to forget this side of the argument and to proceed as if the elimination of transcendent underpinnings of language use left a free-for-all situation. He forgets that drill and linguistic practice prevent such semantic anarchism, leaving no large-scale indeterminacy for social interests to exploit. We shall later return to the question as to how much disagreement in actual language use Wittgenstein's argument could actually allow.

Perhaps we should not make too much of these pronouncements from Bloor concerning meaning, however. They are just careless formulations pertaining to an issue that is rather peripheral to their author's concerns; after all, the Edinburgh School's business is not philosophy of language, which interests them only to the extent that it may be used to reorient our perspective on science. We may grant this point, and go on to examine the implications of this reorientation, which are quite radical in their own right, as we noted above. Among them was the conclusion that no substantial contradiction between rival theories remains, once their proponents resort to systematic rule-bending in order to defend them. What was ostensibly a genuine theoretical rivalry is reduced to a competition between different, but equivalent, linguistic frameworks. This implication remains, even if we disregard Bloor's statements about the vacuity of meaning; one might hang on to the notion of meaning and still argue that, for certain particular tracts of discourse, meanings are indeterminate (this is indeed Quine's position with respect to the intentional idiom).

Seeing that Bloor is not averse to radical conclusions, it might be worthwhile to inquire if this is one he actually embraces. A positive answer might be thought to lie in his repeated insistence that science is *conventional* (Bloor 1976/1991, p. 43, 1983, Chapter 6; also Barnes 1981, 1982, p. 27). This would certainly be a natural way to express the conclusion of the finitist argument: according to a standard philosophical understanding, a convention is a way of doing things to which there are equally satisfactory alternatives (which is not to say that all alternatives are *equally* good; cf. Lewis 1969). In the context of science, to say that two different theories are "equally satisfactory" is, minimally, to say that they are in equal conformity with the observational data. It is indeed part of Bloor and Barnes's position that, in many cases of scientific disagreement, rival theories are in equally good agreement with the facts. This is precisely the conclusion that the finitist position would lead to, since we can always bend the meaning of observational terms to secure agreement between fact and theory. Bloor and Barnes's insistence on the conventionality of science might thus plausibly be taken to express their endorsement of the radical implications of the Wittgensteinian argument in this area.

I think that this reading would be mistaken, however. Bloor and Barnes do not use the term "convention" in the philosopher's customary sense. Rather, they are just making the point that our scientific practices are "contingent" and not being forced upon us by features of the physical world, working in conjunction with trans-cultural and a-historical principles of rationality. Instead, they are adopted by dominant groups in the light of their social interests and upheld mainly by social authority, or power. To call this "convention" may be somewhat misleading, even in a looser, sociological sense, since this term suggests a consensual process; but this is just a terminological matter. The important thing is that, in the present sense, to call a practice conventional is not to suggest that it is *semantically* equivalent with its *functional* equivalents; no more so than "driving on the left" is semantically equivalent with "driving on the right": Those two expressions clearly do not have the same meaning.

There is a further, rather ironic consideration that supports this reading of Bloor and Barnes. If they had embraced the radical version of the conventionality thesis, they would certainly have used it to fend off the charge of relativism that many see as the greatest challenge to the Strong Programme. If ostensibly rival theories are really semantically and cognitively equivalent, it is hardly a problem that we have no objective grounds to choose one before the other; indeed, it follows that we could have no such grounds, save expediency in the most superficial sense. Thus, the relativism charge would evaporate into thin air and, with it, the charge of irrationality and the suspicion that the Strong Programme falls afoul of its own reflexitivity condition. No defense of this kind is ever even hinted at by Bloor and Barnes, however. I take it that this supplies strong evidence that they do not embrace this radical implication of their view.

We have noted before that Strong Programmers embrace an *instrumentalist* interpretation of science (Barnes 1977, Chapter 1, 1982, p. 103 f). This by itself does not shed further light on the exegetical issue currently under discussion, however. For instrumentalism, like conventionalism, does not entail (nor does it prohibit) that rival theories are construed as semantically equivalent under certain conditions. Instrumentalism implies that rival theories are equivalent considered as instruments of prediction (van Fraassen 1980), but not that they are semantically equivalent; for instance, two equally good instruments for opening wine bottles, such as a conventional corkscrew and a fancy device using pressurized air, are not "semantically equivalent" in the sense of that they have synonymous descriptions.

All this has no bearing, of course, on the question whether Bloor and Barnes are in fact committed to the semantic equivalence of rival theories defended against recalcitrant evidence by rule-bending. I argued above that they are: If theories are brought into conformity with the data by Wittgensteinian meaning-bending, those theories are thereby rendered "equally good" in the very strong sense of being equivalent in meaning. They are just equivalent ways of describing reality, like two different, but equally accurate, cartographical conventions for mapping the face of the earth. This has two very counter-intuitive corollaries: that scientific truth is conventional in the strong, Lewisian sense of that notion and that scientific disagreement is non-factual and merely verbal. In this section, I have tried to establish that Bloor's philosophical defence of the Strong Programme saddles him with these undesirable consequences.

10. When the notion of indeterminacy of meaning comes up, thought inevitably turns to a fellow philosophical naturalist, Willard Van Quine. It might be instructive to compare his version with that which we have just elicited from Bloor and Barnes.[30]

We distinguished in Chapter 2 between two kinds of Quinian indeterminacy: the *indeterminacy of meaning* and the *inscrutability of reference*. The former is the stronger notion, while the latter has less drastic implications. Bloor's cases of scientific disagreement generated by divergent, but equally legitimate, projections of terms are close kin to the first Quinian kind. In both cases, it is a matter of assertions that at first seem to make divergent and incompatible claims about reality, but which further reflection reveals cannot possibly express divergent and incompatible semantic contents in the utterers' minds. Quine himself delivers a similar reflection, in his famous indeterminacy argument, but goes on to argue that, in certain cases, semantic indeterminacy exists alongside determinacy of the associated facts. Bloor is innocent of such sophisticated semantic meta-reflections, but seems to assume that genuine scientific disagreements are indeed involved.

However, Bloor's version of indeterminacy has stronger tools at its disposal than Quine's. According to Quine, the means available to the physicist to neutralize unwelcome experimental observations comprise such well-known stratagems as pleading malfunctioning of the observational apparatus, or – in a pinch – even hallucination; but it does not involve the brute adoption of "bent" semantic rules. This means that, intuitively (and according to any semantic theory which respects those plausible intuitions), Quine can uphold the semantic difference between a scientific theory T_1 with the observational implication that some object O is green and another theory, T_2 , with the implication that O is red, although recognizing that this difference may be rendered undetectable by the above stratagems. The semantic difference would consist in the differential inferential implications into which T_1 and T_2 would be enmeshed; T_1 would be tied up with assumptions about experimental malfunctioning, while no such relations would occur concerning T_2.[31]

On Bloor's account, no such differential implications exist. *No* further differences between T_1 and T_2 follow from the adherents of the former adopting a bent rule and insisting that the object in question (which, with respect to its colour, looks rather like a ripe tomato) is indeed green. As Bloor and Barnes insist, *nothing whatsoever* follows; no semantic commitment is incurred. Thus, on Bloor's account, a "bent" rule application is as it were completely inferentially and, hence, semantically insulated. It has, as the author insists, no meaning at all.

Thus we see that in Quine's analysis, too, certain issues that are ostensibly concerned with rival theoretical understandings of the world are really not genuine issues at all, but are purely linguistic. This is the famous doctrine of indeterminacy of meaning (translation). It follows that disagreements about scientific theories are, or at least can be, only partially factual; indeed, it follows that our theories are only partially factual. Our theories are essentially instruments for the prediction of sensations but contain considerable conceptual "padding". There is no way to sequester out this conventional element, however.

Quine's unmasking of seemingly substantial issues as really merely verbal is less extensive than in Bloor and Barnes's version, however. For although Quine never gives us concrete examples to illustrate the scope of the indeterminacy, and indeed occasionally hints that it may be quite extensive (Quine 1970), the characteristic physicalist tenor of Quine's philosophy reveals that he is fundamentally a realist about physical nature. To Quine, the ontological ground level of reality is physical, and the only explanations that are entirely beyond metaphysical suspicion will, hence, refer to physical processes. In particular, Quine eschews intentional explanation (of which interest explanations are a subclass) as metaphysically second-rate.[32] Thus, whereas Strong Programmers give explanatory priority to sociological explanations vis-a-vis explanations in natural science, it is the other way around in Quine. Strong Programmers are realists with respect to the furniture of the social world, but instrumentalists with respect to the physical world, whereas Quine, although operating within an overall instrumentalist construal of knowledge, still places the accent of reality with the opposite priority. This indicates that the scope of the non-factuality of natural science is much less extensive according to Quine than according to Bloor and Barnes. At any rate, unnoticed by most critics, and even apparently by Bloor and Barnes themselves, their doctrine comprises a very strong, and very controversial, indeterminacy thesis.

Thus, the pictures of science offered by Quine and by the Strong Programme display strong similarities, combined with a striking difference in perspective and emphasis. Although he describes himself as a pragmatist, Quine is supremely uninterested in examining those "pragmatic" concerns that are held to shape the global network of inferentially connected sentences that make up science; in particular, he has no inclination to examine how these interests are embodied in the agents and institutions that produce science. He is satisfied with airy references to a concern for *simplicity* and another, occasionally countervailing, interest in *conserving* the existing structure of knowledge, while leaving the nature of the subjects harbouring these abstract interests completely unspecified. (In this respect, he shows himself as a true pupil of Carnap). Instead, he is consummately interested in the semantic indeterminacy that flows from this picture. Bloor, on the other hand, is quite unconcerned about the unfactuality of scientific controversy that follows from his finitist approach; indeed, he is happy to accept that the meaning of scientific terms dissolves altogether. His concern is solely with the societal interests and forces that incline the parties to scientific controversies to choose their respective interpretations.

11. As it happens, references to social interests as determinants of our "language games" are almost as rare in Wittgenstein as in Quine. This suggests that the sociologization of epistemology is as remote from the former's concerns as from the latter's.

Bloor is aware of this and, indeed, of the anti-scientific spirit of Wittgenstein's thought, but is satisfied that he can graft a sociological theory of interest onto Wittgenstein's basic framework (Bloor 1983, p. 3 f, 1992).

It is highly questionable, however, if Wittgenstein could countenance the extent of disagreement over the application of scientific terms that Bloor needs for his argument. As far as I know, Wittgenstein has not addressed the issue of disagreement with express reference to science, but there are frequent remarks on the parallel question with respect to mathematics:

> Disputes do not break out (among mathematicians, say) over the question whether a rule has been obeyed or not. People do not come to blows over it, for example. That is part of the framework on which the working of our language is based (for example, in giving descriptions).
>
> (Wittgenstein 1953, § 240)

> Of course, in one sense mathematics is a branch of knowledge, -but still it is also an *activity*. And 'false moves' can only exist as the exception. For if what we now call by that name became the rule, the game in which they were false moves would have been abrogated.
>
> (Ibid., p. 227)

And as a matter of fact, there is explicit mention of disagreement with respect to the kind of terms figuring in our little example above, that is, colour words. Wittgenstein expresses doubt concerning the intelligibility of the thought that people might not generally agree on the use of such words; at least such disagreement would clearly mark their language game with these words as different from ours: "If there did not exist an agreement in what we call 'red', etc. etc., language would stop" (Wittgenstein 1953, p. 226, cf. Wittgenstein 1967a, p. 96, also Wittgenstein 1969, §§ 624–626).

As it happens, people have almost come to blows over certain scientific issues and the methods through which they were investigated; witness the discussion over global warming or creation science. The cause of such tension is plausibly tied to divergent underlying interests. It will be hard to put a coherent interpretation upon such divergences within a framework that borrows as much from Wittgenstein as does Bloor's. The divergences cannot be construed as rifts within one and the same "language game", generated by opposed interests; such rifts are inconsistent with the assumption that the parties use language in the same way and, hence, engage in the same "language game". Nor can they be construed as oppositions between different and autonomous "ways of life" (i.e., science against some variety of non-science). It is imperative for Bloor's sociological analysis of *scientific* disputes that it allows both sides of such disputes to count as sciences, and does not force us to declare one of the parties non-scientific.

In a Wittgensteinian conception, a rivalry between two radical different "ways of life" is not a difference of *opinions*. The sentences in which the antagonists express their positions cannot be understood to belong to the same language game and hence fail to contradict each other. This point is neatly illustrated by Wittgenstein's reflections over the relationship between science and religion (Wittgenstein 1967b). He makes it very clear that the religious person's belief in Judgement Day is not at the same level as the atheist's disbelief in what is apparently the same doctrine.

The two "beliefs" do not contradict one another; they are not "beliefs" in the same sense (op. cit., p. 55). One of them may indeed be a case of assenting to a given sentence; but the other, the religious one, rather expresses a commitment to a particular way of leading one's life. Thus an orthodox Wittgensteinian would be inclined to interpret the opposition between orthodox Darwinian biology and its creationist counterpart as a clash of science with non-science. This analysis would not apply to most ordinary cases of disagreement in science, however, and Bloor would no doubt be loath to accept it.[33] For instance, it would hardly make sense to treat the dispute between phrenologists and their opponents, or between the early adherents of quantum mechanics and their critics, in this way.

12. I believe there is an even deeper reason why an interest-based theory of disagreement is inimical to Wittgenstein's mode of thinking. This has to do with the quasi-transcendental status of agreement in Wittgenstein's system. Agreement about the use of a certain term does not determine what is true and, hence, what is real; but it makes it possible to use that term to express (and think) truths. Let us look at the famous paragraph 242 in the *Philosophical Investigations*:

> If language is to be a means of communication there must be agreement not only in definitions but also (queer as this may sound) in judgements. This seems to abolish logic, but does not do so. -It is one thing to describe methods of measurement, and another to obtain and state results of measurement. But what we call "measuring" is partly determined by a certain constancy in results of measurement.

We might paraphrase this as follows: If agreement, or a close approximation, cannot be attained in the use of a term T within a linguistic community, we cannot take this term to possess a determinate meaning, nor to be descriptive of reality; that is, there is no possible (conceivable) feature of reality captured by this term. But this means that there is no intelligible issue as to whether the world does really possess (or fail to possess) this feature, since either assumption is empty, in the absence of suitable agreement in the application of T. *A fortiori*, there is no intelligible issue as to whether it would be in the interest of either of the parties to the debate if the world did, or did not, possess the feature of T-ness. More simply put, if a certain level of agreement on an issue is a condition of the intelligibility of that issue, then the agreement (or disagreement) cannot come about as a product of the practical interest that the disagreeing parties take in its outcome. Or even more simply, if sense emerges through social agreement, then agreement cannot be explained in terms that already presuppose the emergent sense. There is a vicious circle involved here.

This, I think, is at least part of the reason why Wittgenstein insists that ways of life simply have to be taken for granted (Wittgenstein 1953, p. 226). There is no explaining them, neither in the philosophical sense of deriving them from general principles of rationality or meaningfulness, nor in the scientific sense of deducing them from general empirical laws, or by citing their causes (ibid.). This is true *a forteriori* if those laws refer to the computing of interest or similar cognitive processes. Bloor's analysis must at least presuppose that an agent can coherently entertain thoughts about states of affairs describable by means of T, spurring his interests and motivating him to adopt the term or reject it. This point applies even if interests are held to determine conviction in a purely causal way, i.e. by *swaying* the latter, rather than by supplying premises for deliberation, whether explicit or implicit, about the practical consequences of adopting the term T. For if the causal power is held to reside in the very T-ness of those objects of interest, rather, say, in some physical characteristic shared by T-like things and upon which T supervenes, it must at least be possible for the agent to entertain a thought to the effect that certain objects exhibit T-ness. But this does not appear possible, given Bloor's Wittgensteinian conception. Meaningful thoughts concerning the T-ness of things are only possible once a consensus "in judgements" has emerged in the form of a consensual practice in the application of T. Hence we cannot *explain* why people reach agreement in certain areas (mathematics, say), and why they fail to do so in others; at least, we cannot do so in terms that presuppose the intelligibility of the issues at stake. We simply have to take agreement, or disagreement, as given. Indeed, this is what Wittgenstein impresses upon us: "What has to be accepted, the given, is – so one could say – *forms of life*" (op. cit., p. 226). Agreement is transcendental, it defines "the limits of empiricism" (Wittgenstein 1969, p. 96).[34]

The Strong Programme and related developments in the sociology of science are concerned to show that scientific controversy is fundamental and ineradicable, since its existence follows, by strict sociological laws, from the rival interests that are at play in science. This thesis is fundamentally at odds with the tenor of Wittgenstein's thought, which celebrates the transcendental role of agreement and tries to show that irremediable disagreement over an issue implies either that the questions addressed are meaningless, or that they involve people talking past each other, because they play different language games. It would be highly surprising if a theory of this kind could provide the metaphysical underpinnings precisely of a theory of intellectual *controversy*.[35]

13. Our reflections in this chapter have revealed a tension between the Strong Programme as an empirical research programme and as a philosophical project. The empirical programme aims at producing sociological explanations of the contents of scientific theories. Officially, this programme is inductively based; it stands or falls with the solidity of its empirical support. I have argued that it falls; this was the outcome of measuring it with the yardstick of Hempelian explanation. The philosophical project, on the other hand, aims at no less than the *naturalization of the philosophy of science* – that is, the demonstration that sociological explanation will answer all the (legitimate) questions that philosophers have raised concerning science. These two sub-projects could live together in harmony as long as the philosophical project would rely solely on the findings of the empirical enterprise. We found that the empirical record is highly disputable, however; and even if it had been impeccable, the philosopher would be free to argue that with methodological circumspection, the scientific process may raise itself above those social factors. Indeed, he might turn the tables on the Strong Programme, arguing that the results garnered by the social studies of science precisely help us to curb those influences, in the same way that medical studies identify and analyze human ailments and then proceed to prevent their occurrence.

Bloor's Wittgensteinian argument is designed to block this move. Wittgensteinian finitism allegedly implies that there is nothing in the sheer sensory evidence, *no matter how comprehensive*, that compels us to describe the outcome of scientific experiments in one way rather than another. We may always dig our heels in and insist that our test sample is indeed *green*, although we have a hard time distinguishing it from the colour of ripe tomatoes. Thus, even if all the evidence is in, in the form of sensory experiences derived from an experimental setting, it is always just a matter of social convention how we describe that evidence (Barnes 1982, p. 75).

I have argued that this reasoning is based upon a misinterpretation of Wittgenstein. More importantly, I also tried to show that the price of this manoeuvre is forbiddingly high. In particular, it implies that fundamental scientific disagreement is nonfactual and merely verbal. Thus, in order to clear away the obstacles to a radical sociology of science, the Strong Programme brings in a powerful and controversial philosophical ally: Wittgenstein and his rule-following considerations. This results not only in an extreme revision of science, but also in a revision of the sociology of science, which suggests that the marriage of the sociology of science, as a purely empirical enterprise, with Wittgensteinian finitism in the philosophy of language is not a happy one.

The lengths to which Bloor is ready to go in this matter and the intellectual risks he is willing to run, remind us that there is a "hot" agenda behind these supposedly cool and rather esoteric academic debates. The aim is to discredit an image of science as somehow detached from and elevated above the normal hustle and bustle of society, an image that, adding injury to insult as it were, is often used to secure a place of special authority for science vis-a-vis ordinary societal concerns. This image stands in the way of a better and fairer accommodation between science and society, the terms of which are not to be dictated by science, or based upon an idealized image of its nature. In the following chapters, we shall see how this complex agenda was inherited by the tradition started by the Strong Programme and how they developed the tangled arguments used to promote it.

Chapter 5
Harry Collins and the Empirical Programme of Relativism

1. Harry Collins dubs his project in the sociology of knowledge the *Empirical Programme of Relativism* (Collins 1981a). The name signals the programme's naturalistic orientation, which sets it apart from mainstream philosophical approaches to relativism. The contrast is underscored by the fact that, whereas philosophy is traditionally hostile to relativism, Collins's programme embraces it. The Empirical Programme of Relativism examines the dynamics of science in order to demonstrate that science in not a monolithic other-worldly phenomenon, but is rather an ordinary social enterprise, sharing the multiplicity and diversity that characterizes all social and cultural undertakings.

As was true of the Strong Programme, this research objective is closely associated with an ideological or political agenda, which is expressed thus by Collins in a book with the telling title, *The Golem – what everyone should know about science*: "To change the public understanding of the political role of science and technology is the most important purpose of our book and that is why most of our chapters have revealed the inner workings of science" (Collins and Pinch 1993, p. 143). This quote also indicates the method adopted to reach this goal, which is one shared with the rest of STS, viz. the simple empirical exhibition of the actual workings of science which have hitherto been shrouded in philosophical mystery. The point is driven home in the book's Introduction: "The book is very straightforward. To show what Golem Science is, we are going to do something almost unheard of; we are going to display science, with as little reflection on scientific method as we can muster. We are simply going to describe episodes of science, some well known, and some not so well known. We are going to say what happened. Where we reflect, as in the cold-fusion story, it will be surprising. The shock comes because the idea of science is so enmeshed in philosophical analyses, in myths, in theory, in hagiography, in smugness, in heroism, in superstition, in fear, and most important, in perfect hindsight, that what actually happens has never been told outside of a small circle" (ibid., p. 2). Sober, unprejudiced and "symmetric" description of what science is actually like behind the scenes will demystify this societal institution and serve to raise public consciousness about it; this may inspire initiatives to redress the imbalance between science and the larger society which is its host.

The Empirical Programme of Relativism (EPOR) is inspired by the Strong Programme and largely shares its explanatory ambitions. There are significant

F. Collin, *Science Studies as Naturalized Philosophy*, Synthese Library 348,
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differences, however. Thus, Collins takes exception to Principle 1 of that programme, which calls for explanation to be causal. Collins comments that this principle carries with it "problems about the relationship between sociological explanation and scientific explanation which problems are, at best, a distraction from the main thrust of the programme and, at worst, a positive hindrance" (Collins 1981c, p. 215). Collins does not go any deeper into the nature of these problems and hindrances, but we may safely assume that they have to do with the trouble-spots we pointed out in Chapter 3. He also distances himself from Principle 4, the call for reflexivity that "can lead to paralyzing difficulties" (ibid.). Still, he wholeheartedly embraces Principles 2 and 3 of the Strong Programme.

Collins does not codify the differences between EPOR and the Strong Programme in a list of rival principles, but chooses instead to define his programme by a sequence of *stages of investigation*. As we shall see, however, certain principles emerge along the way, as Collins demonstrates this model to his readers by following some select case material through these stages of investigation. This difference of approach reflects a difference between the professional profiles of the central figures. Whereas Bloor is a theoretician, trained, among other things, in mathematics and philosophy and mainly concerned to defend a theoretical position to which he has himself supplied very little empirical input, Collins is more of a practitioner who was trained in sociology and has made significant empirical contributions to the sociology of science.

2. The three stages of EPOR are the following: (1) Demonstrating the "interpretative flexibility" of experimental data, which is close kin to the phenomenon often referred to as the "underdetermination of theory by data". (2) Showing the mechanisms by which, in the absence of evidential or rational determination, closure is nevertheless effected. The mechanisms which bring about this turn out to be social. (3) Linking the local closure mechanisms to wider social forces and political structures (Collins 1981a).

Much of the empirical evidence supporting EPOR has been supplied by Collins himself, in terms of a handful of case studies. These studies revolve around the concept of *experimentation*, a crucial aspect of the scientific process by anybody's standards. According to the traditional account, experimentation is the institution deciding the fate of scientific hypotheses. The fate of experiments is decided, in its turn, by *replication*. Experimental outcomes must not be coincidental and the antidote against this is replication. This is part of the ethos of science, of Merton fame: The "organized scepticism" of science, making up the OS of the CUDOS-norm, is embodied in the principle that experiments must be conducted and reported in such a way that anybody may replicate them and check if the same outcome recurs.

Experimentation seems to be fairly straightforward business, as long as it is described in the abstract, disembodied manner typical of philosophical accounts. Using logical and mathematic tools, testable sentences are inferred from the theory under assessment, T. These have the following general form: under conditions XYZ, phenomenon A will be observed. Next, conditions XYZ are brought about in a controlled laboratory setting, and a record is made as to whether A obtains or not. If it does, T is corroborated, if not, it is falsified.

In this standard account, however, no attention at all is paid to the messy practical details and the immense effort that goes into making the experimental set-up work. By contrast, this is precisely the focus of Collins's empirical case studies. Collins has published the results of these studies extensively, among other places, in a recent, almost 900-page-long monograph on the measurement of gravitational waves (Collins 2004). He draws upon all these studies in his main theoretical text, *Changing Order* (Collins 1985/1992), in which brief extracts of his empirical findings are arranged in a sequence illustrating and validating the stages postulated by EPOR. The three case studies deal, respectively, with construction of the TEA laser, with measurement of gravitational waves and with measurement of the mental life of plants. We shall retrace this structure in the following presentation of Collins's main ideas.

3. The first case presented by Collins in *Changing Order* involves the TEA laser. This is a fairly simple device, a feature partly reflecting its ability to operate at normal atmospheric pressure, which distinguishes it from most other lasers. Despite this simplicity, a lot of stage setting is needed to make a piece of apparatus like this work properly; this is precisely what Collins is at pains to demonstrate in his study. We learn how even a person highly experienced in the art of laser making needs 6 months to get a TEA laser to function properly. Yet what Collins is especially concerned to show is not the mere magnitude of this work, but the fact that the knowledge required to perform it successfully is *tacit* and *practical*. The first thesis established by these case studies is thus that the professional expertise going into experimentation is tacit, unformalizable and having the nature of a practical skill.

This result is significant, since it subverts one of the cherished myths of the scientific enterprise, that is, that its strength and objectivity are secured by the relentless scrutiny to which published results are subjected, effected in particular through replication. The possibility of such check is supposedly guaranteed, in its turn, by an important convention of the scientific literary genre: to report experiments in such exhaustive detail that every step needed to replicate them is clearly laid out. Collins's results, however, show that it is not possible to make an experimental device function merely on the basis of the abstract description that occurs in a scientific paper; there must be personal contact between practitioners to allow the "tricks of the trade" to be passed along and, above all, the opportunity for hands-on interaction with earlier versions of the same type of apparatus.

Collins sums up the results of his examination of the TEA laser in a number of propositions:

> *Proposition One*: Transfer of skill-like knowledge is capricious.
>
> *Proposition Two*: Skill-like knowledge travels best (or only) through accomplished practitioners.
>
> *Proposition Three*: Experimental ability has the character of a skill that can be acquired and developed with practice. Like a skill, it cannot be fully explicated or absolutely established.
>
> *Proposition Four*: Experimental ability is invisible in its passage and in those who possess it.

> *Proposition Five*: Proper working of the apparatus, parts of the apparatus and the experimenter are defined by the ability to take part in producing the proper experimental outcome. Other indicators cannot be found.
> (Collins 1985/1992, pp. 73–74)

The final Proposition looks fairly innocent as long as we are dealing with a practical tool such as a laser, for which the standard of proper functioning is agreed on and uncontroversial, and its satisfaction easily recognized by simple observation: the laser must, for example, be able to vaporize concrete. The revolutionary potential of this proposition becomes clear, however, when we turn to apparatus that produces *measurements* rather than broken concrete and examine its role in the scientific testing process. In this case, a vicious circle, or regress, is produced, dubbed by Collins the "experimenter's regress". This is illustrated in Collins's second case study, which involves gravitational waves and the devices built to measure them. In turning to this case, we embark upon the first of Collins's three stages of EPOR, to which the examination of the TEA-laser was only a preliminary.

4. According to general relativity, a large accelerating mass, such as an in-spiralling binary star, will generate gravitational waves, but only exceedingly weak ones, and physicists have never been able to detect them. In the late 1960s, however, the American physicist Joseph Weber designed a piece of apparatus of hitherto unheard-of sensitivity to measure them. He came up with what he claimed was a positive result; more specifically, he claimed to have measured distinctive, high-energy fluxes of radiation. However, other physicists were sceptical, mainly for theoretical reasons: Weber allegedly had found bursts of such violence that the universe would soon exhaust itself with this large expenditure of energy were they genuine. Such theoretical reflections were not conclusive, however, so researchers had to turn to that recognized panacea, checking the replicability of the results.

Other teams of researchers tried to repeat Weber's results, mainly with negative results. But this did not settle matters either, since these experiments, in their turn, were not unassailable. Weber would attack the rival experimenters for lacking expertise and their setups for being based upon faulty assumptions. What ensued was a protracted battle that eventually ended in the conclusion that high fluxes of gravitational radiation did not exist and that Weber had not measured any such. (The negative verdict did not pertain to gravitational radiation of much lower energy, the search for which continues to this day, as reported in Collins 2004.) Collins follows this process to its conclusion and documents its dynamics. It emerges that the process was not governed by an explicit methodological protocol, dictated by some clearly defined algorithm of scientific rationality. There is no code-book of proper experimentation to turn to in deciding which side does the better experiment, no *experimentum crucis* which rationally compelled everyone to reject Weber's findings. Rather, there was a slowly growing body of contrary evidence, all of it individually contestable, but collectively decisive. Historically, a few clearly defined incidents served to fuse this evidence into a single, critical mass. But there is no algorithm dictating that this point had a unique and uncontestable rational significance. The process was contingent, from a rational point of view.

The vicissitudes of the search for gravitational waves illustrate the subversive power of Proposition Five: Since the only positive test of the proper functioning of a piece of apparatus is its ability to generate the right output, a vicious circle ensues when the "right output" is not known beforehand. Yet this is precisely our predicament when dealing with a measuring device, employed to detect a phenomenon whose existence is moot. For the outcome to count as a (valid) measurement or detection at all, the soundness of the measuring apparatus must be assumed beforehand; but this assumption is unfounded if the soundness of the device can only be established by its ability to produce a correct output. Thus a circle ensues; or at least this is so if there are not other ways to define the proper outcome. Collins prefers to construe it as a regress: the "experimenter's regress" (Collins 1985/1992, p. 83 ff).

Collins expresses this predicament in two further Propositions:

> *Proposition Seven*: When the normal criterion – successful outcome – is not available, scientists disagree about which experiments are competently done.
>
> *Proposition Eight*: Where there is disagreement about what counts as a competently performed experiment, the ensuing debate is coextensive with the debate about what the proper outcome of the experiment is. The closure of the debate about the meaning of competence is the "discovery" or "non-discovery" of a new phenomenon.
> (Op. cit., p. 89)

The latter proposition would not be controversial if it stated that decisions on whether to grant that a novel phenomenon has been detected are contingent upon decisions concerning the validity of scientific tests; this is as conventional scientific wisdom has it. But the connection runs in the opposite direction as well. In controversial cases, the verdict on the skills of experimenters and the validity of experiments will be highly dependent upon the protagonists' willingness to accept the outcome in the first place. There is no rigorous and independent way to test experimental competence.

Collins makes much of the point that what gave weight to the negative evidence marshalled against Weber differed greatly among researchers (op. cit., p. 87 f). Some would see a particular technical detail as crucial; others, the use of advanced computer programmes to detect the effects. Still others put more store upon the professional credentials of the experimenters and their background experience in running a large laboratory, the style and presentation of results, size and prestige of university of origin, and many other apparently "external" standards of assessment.

5. These features are illustrated even more strikingly in Collins's third case, which involves a highly controversial experimental study within parapsychology. It concerned the alleged emotional life of plants, more specifically the claim that plants are subject to moods that can be manipulated experimentally by inducing physical or mental stress. Using electronic circuitry basically identical to that found in lie detectors, the experimenter, Clive Backster, claimed to discover strong reactions in plants when they were subjected to stress – not only caused by direct physical impact such as draft or heat, but also by the mere verbal expressions of intentions to inflict such stress. The plants were even able to pick up stress signals from other living organisms, for example from shrimps that were being thrown alive into boiling water in the vicinity.

Backster's result were immediately challenged and a lengthy controversy ensued. Backster's conclusions ran up against strong metaphysical and scientific prejudices against attributing sophisticated mental states to such primitive organisms as plants, which lack a nervous system. In the debate, standard principles of scientific experimentation were called into question, too, such as the importance of eliminating any direct impact of the experimenter's presence upon the experimental setup. In contradiction of this principle, Backster and his collaborators refused to accept a number of negative experiments on the grounds that the experimenters had failed to establish the requisite emotional *rapport* with the plants before the tests started. Much attention was given to purely technical details, such as the materials used in the electrodes attached to the plants.

For a while, persons outside the parapsychological community participated in the debate. But soon, they withdrew with a negative conclusion and this was the note on which closure was achieved. But Collins is at pains to stress that this outcome was not due to a precise, algorithmic experimental protocol, but was a product of contingent features of the entire episode.

Collins sums up his findings in the claim that closure through experimental testing is not achieved as the result of rational algorithmic rules, but as a product of the contingent social forces impinging on experimentation as a communal practice. Thus, the course of science is mainly shaped by such social features, not by rational standards, nor by the pressure directed at our theories by physical reality. The locus of this impact is through experimentation; this is where theories confront reality, at least in the more advanced natural sciences. As we have seen, however, this impact is never direct, but always mediated by social interactions, consisting of the scientific community's efforts to interpret the experimental outcome.

6. So far, we have only covered the first two of the three stages of EPOR, which are in focus in Collins's own case studies. They extend only sparsely to the third stage, in which the restriction to the laboratory is lifted to allow larger societal forces to come into view. These forces supplement the local contingencies of the laboratory in the process of establishing closure. Collins invokes the familiar findings of the contemporaneous social study of science to establish this result. He refers to the phrenology case and other examples, which we have already examined in connection with the Edinburgh school.

Collins does not set up a simple binary contrast between local forces operating within the laboratory and global forces operating in society at large, however, but offers a differentiated picture that represents novel scientific findings as moving into the larger society through a series of steps, diffusing into ever wider circles (Collins 1981b). First, there is the "core set", consisting of the researchers who actually conduct the experiments and the theorists who interpret the results, possibly in mutually contradictory ways. The next circle consists of scientists who learn about the outcome of the activities in the core set only through "testimony", be it hearsay or formal scientific publications. This circle might be subdivided into two, the first consists of scientists sharing the same specialty as those making up the core set;

the second one consists of scientists at large. The next ring consists of people who are not themselves (active) scientists, but who interact with the latter as administrators, politicians or donors of funding. The fourth and final ring consists of society at large, which uses the products of scientific activities.

For the last mentioned group (and even for the scientific but non-core set), their reason for adopting a particular theory cannot be an insight into the actual scientific evidence, since they are incompetent to evaluate it. Instead, scientific authority must play a role, as will larger societal interests. This is a crucial break with a residual Cartesianism, or individualist empiricism, in epistemology and philosophy of science, according to Collins. He makes the interesting observation that conviction of the truth of a theory is at its strongest in the more remote parts of the societal landscape, where it is inculcated through an effective rhetoric that censors away the messy detail of the experimental history behind it. Members of the core set know about the precariousness of the process through which the results emerged and are hence less naive about its epistemic status.

As will have emerged, Collins's interest in the scientific process revolves around the notion of *closings*. He is not interested in "openings", the manner in which scientific innovation arises. In *Changing Order*, the point is expressed thus: "This is a picture of social and conceptual order, but if there is to be substantial change then new ways of proceeding must be invented and sustained. But in earlier sections of this chapter we have seen how easily contradictions can be initially created. A potential scientific revolution can be read into any trivial mistake. Thus the origin of creativity in itself is not an interesting problem" (op. cit., p. 148). This, however, seems to confuse the frequency of occasions on which scientific novelties are ostensibly needed, which may indeed be plentiful, with the frequency of genuinely original, revolutionary ideas, which seem to be much more scarce. It is basically a confusion of demand and supply. In any case, Collins eschews explanation of the latter. His aim is not to explain the *genesis* of scientific theories in terms of purely social parameters, but only to explain their *reception*: "[W] e . . . are not concerned with the context of discovery, or rather creativity; this we are prepared to accept as a 'black box': an affirmation of the open-endedness and unboundedness of individual human thoughts. . .We are concerned with the processes of acceptance and rejection of beliefs. . ." (Collins and Cox 1976, p. 438. See also Collins and Cox 1977, p. 378). In other words, he confines himself to what I termed Type II explanation in Chapter 3.

7. So much for EPOR as a purely naturalistic, empirical programme. However, like its Edinburgh cousin, Collins's programme is helped along by a solid boost of philosophy; like Bloor and Barnes, Collins does not in the end trust that the mere marshalling of empirical evidence will do the trick. Hence, Collins opens his chief theoretical text, *Changing Order*, with a dose of what he calls "philosophical scepticism", which is claimed to be a "safe, legal and efficient" device with which to clear the ground of common-sense presuppositions (op. cit., p. 6). Thus, sceptical philosophy (in Collins's terminology) is deployed to push out dogmatic philosophy of science.

We find in Collins the same, rather ambivalent attitude to philosophy as in the Strong Programme. On one hand, orthodox philosophy of science is roundly dismissed; indeed, this is one of the objectives of the entire programme. On the other, critical or sceptical philosophy is deployed to eliminate familiar preconceptions, thus clearing the ground for the proper scientific understanding of science.

These philosophical ideas are used in a rather omnibus fashion and with considerable insouciance about technical details; many different arguments are mixed together. As in the Strong Programme, Wittgenstein's reflections on rule following play a major role, although they are presented in a somewhat simplified version under the term "awkward student". Where Wittgenstein's original story involved a student sincerely trying to grasp his teacher's instructions for continuing a sequence of numbers (but constantly failing), Collins suggests a more devious version, where the student deliberately misconstrues the teacher's instructions in order to demonstrate that no amount of verbal instruction or explanation will ever bridge the gap between rule and actual performance. No matter how hard the teacher tries to predetermine the student's behaviour (writing numbers on the blackboard), the student always finds a way to avoid doing what he knows the teacher wants, while always staying within logically permissible interpretations of his words.

Collins also throws in Goodman's reflections on non-standard predicates as well (Goodman 1973). Goodman presents his idea as a modern challenge to inductive reasoning, adding a twist to Hume's classical scepticism and calling it "the new riddle of induction". Consider the following: Over the span of millennia, mankind has observed a large number of emeralds and found them all to be green. Thus, the collective experience of mankind up until, let us say, 1 February 2035, supports, by inductive generalization, the proposition that all emeralds are green. However, the same evidence also supports the conclusion that all emeralds are *grue*, which is the property of *either* being observed prior to 1 February 2035 and being green *or* being observed after that date and being blue. All emeralds observed prior to the cutoff date are thus *grue*, we may agree, thus supporting, by impeccable inductive reasoning, the conclusion that emeralds observed in the future will be *grue* as well. Yet this involves the highly counterintuitive implication that emeralds will change colour from green to blue on the night between 31 January and 1 February 2035. The challenge is now how to block this kind of inference. A natural move is to ban "bent" predicates such as "grue" from occurring in inductive inferences, allowing only our familiar "straight" ones such as "green" and "blue". This calls for a criterion to distinguish them, however, and an obvious suggestion is that "bent" predicates involve a temporal index, while straight ones do not; yet this suggestion founders on the observation that, viewed in terms of bent predicates, straight ones are defined by a temporal cut-off point. In any case, bent predicates may be defined without using such temporal indexes at all, so a more general solution is required.

Goodman's own solution to the puzzle is that straight predicates can only be defined as those that are "entrenched" in our actual linguistic practice. This is merely a fancy way of saying that they are the ones we actually use. Not all philosophers are satisfied with Goodman's pragmatic answer and the thought experiment has triggered numerous alternative attempts to resolve the issue in a more formal manner.

We are not surprised to find, however, that Collins accepts Goodman's "social" resolution of the riddle. That mode of resolution shares a common theme with Wittgenstein's rule-following considerations. A thesis underlying both Wittgenstein's and Goodman's arguments is that rule following is essentially a tacit, practical ability. The case of Wittgenstein's pupil shows that it is impossible fully to compress a person's knowledge of a mathematical sequence (a mathematical rule) into an explicit, verbal formula. Goodman's "new riddle of induction" shows that there is no abstract, verbal formula that will capture what appears to be a clear intrinsic difference between "straight" and "bent" predicates. We are left to characterize "straight" predicates in an indirect manner, that is, precisely as those that we find intuitive and natural. Goodman, however, prefers a more objectivistic definition, where we talk of predicates being "entrenched" in our practice.

There is another, more specific lesson to be learnt from these philosophical reflections, which is also adopted by Collins. Both lessons point to the *non-straightforward nature of similarity*. Wittgenstein shows that similarity or being "the same" is not a simple self-explanatory notion, but is deeply imbedded in a social practice. The import of Goodman's reflections is the same. Our intuitive reaction to bent predicates is to say that straight predicates collect items that are similar, whereas those of bent predicates are dissimilar. But, if Goodman is right, this characterization will not work, since there is no formal way to express this notion of similarity. In the final analysis, we have to characterize the former as those (kinds) that are as a matter of fact adopted by the community.

Collins transfers these semantic points to the institution of scientific experimentation. Whether or not an experiment qualifies as a check upon a previous experiment depends upon whether or not the former replicates the latter in relevant respects, that is, whether or not it is the relevantly *similar* to the latter. Thus, the notion of "similarity" or "sameness" moves to centre stage. The import of Wittgenstein's and Goodman's reflections is that the answer to the question whether something counts as *the same* or not is never straightforward, either, but emerges through a lengthy process that is endlessly molded by all the forces that make up the social sphere.

8. Let us pause briefly at this point to compare Collins's use of Wittgenstein and Goodman with Bloor's use of the former. The two turn out to be rather different: While Bloor used Wittgenstein's rule-following considerations in a direct way to show the social nature of concept use – indeed, as I argued, in a *too* direct way, since he moved straightaway from norm to fact – Collins tries to establish the social nature of concept use in a more roundabout manner, the details of which we shall look at below. The immediate corollary drawn by Collins of his analysis of Wittgenstein and Goodman pertains to the *tacit* nature of rule-following and the non-straightforward nature of similarity, as we saw above.

Collins's use of Wittgenstein, however, seems to share some of the weaknesses found in Bloor. Thus, the tacit nature of the skill displayed in concept use does not in itself imply that such use must invite disagreement. As a matter of fact, this inference goes against Wittgenstein's rather conservative predilections, as I argued in the previous chapter. The fact that the tacit nature of rule-following does not in itself imply disagreement is suggested by the observation that Michael Polanyi famously tried to base the authority of science precisely on the fact that scientists are privy to tacit, unformalizable knowledge about how science is conducted (Polanyi 1966). The scientific process cannot be adequately assessed or criticized by outsiders (to the particular discipline in question), since they are ignorant of the rules by which it proceeds. In particular, they could not have acquired those principles by explicit instruction, such as by reading about them in a book, since they are inherently tacit. Obviously, Polanyi's position would be fatally undermined if a simple inference existed from implicit status to diversity of opinions and one would have thought that he would have spotted any such flaw. By the way, Collins briefly mentions Polanyi's work, but does not touch upon this problem.

The same applies to Collins's invocation of Goodman. The fact that there is no explicit formal grounds for distinguishing a bent use from a straight one and that our linguistic knowledge is thus in a certain sense tacit, does not imply that there would be any disagreement in use. It is noticeable that Goodman does not draw any such conclusion. He is not trying to make us speculate if part of the English-speaking community will suddenly apply the term "green" to blue objects after 1 February 2035, to the utter astonishment of the rest of us. The point is precisely that they will not (and that we know this), but that it is impossible to characterize the way that they actually use the term other than that it is the term they actually use. We cannot express their shared insight explicitly (since the formula offered two sentences back *presupposes* the standard meanings of "green" and "blue" and thus does not help us to *define* them). But this does not make their linguistic practice more vulnerable to disagreement.

This, by the way, betrays a difference between Goodman and Wittgenstein that is slurred over by Collins. Wittgenstein is concerned to show that the future use is not anticipated in any way in previous applications, nor in users' intentions, or in other "mental" media, and so could not be projected on this basis, either. Not even God could discern it, were He to peek into our minds (cf. Wittgenstein 1953, p. 217). This means that the correctness of future applications cannot be defined by its conformity with such mental facts. Wittgenstein concludes that correctness can only be defined as conformity with the actual future use adopted by the community.

Goodman has no such agenda. He is not concerned to argue that even God could not find evidence of a future "kink" in the application of a predicate, were he to look into human minds. He only wants to insist that there is no absolute or intrinsic way to characterize such bends in order to contrast them with our normal usage. The difference is precisely that the predicates in question are those we happen to have projected in the past. To Goodman, there is a fact of the matter as to which predicates we have projected in the past; if this were not the case, there would be no fact of the matter either as to how we should project those predicates in the future. Hence, there would be no resolution *available today* as to the proper way to project "green" past 1 February 2035.

In any case, the bearing of all this upon Collins's "experimenter's regress" should be obvious. He sums it up in this slogan: in experimentation, whether or not an experiment should count as the same as another is not a matter of strict explicit rule (Collins prefers to talk about "algorithmic" rules). By the same token, it is never straightforward whether a negative experiment should count as a refutation of a hypothesis (or for that matter, a positive experiment as a vindication). For it is always open for someone determined to defend the hypothesis to argue that there was a discrepancy in some relevant factor. This is amply illustrated by the case of the gravity wave detector, and the parapsychological experiment.

9. On the basis of a number of case studies and a supporting philosophical argument, Collins has thus established, to his own satisfaction, that the celebrated scientific institution of experimentation is not governed by precise algorithmic rules. If closure is not dictated by rational procedures, how then does it come about? The answer was anticipated in the reflections upon the case studies above: social factors, in a general and all-encompassing sense, do the trick.

Collins fleshes out this analysis in terms of a more general model of the structure of scientific knowledge. For this purpose, he invokes "Hesse-nets". With these, another resource from recent philosophy is invoked, if only indirectly: namely, Quine's network theory of human knowledge, into which we looked in Chapter 1. In the sociology of science literature, they are known as "Hesse-nets" as they were introduced into this context by Mary Hesse (1974).

Hesse develops this conceptual model by way of a criticism of the venerable distinction between observational and theoretical terms. Along with other recent critics of this distinction, Hesse points out how even apparently clear examples of observational terms – such as "red", or "simultaneous" – can be undermined on closer inspection (the latter example famously by reference to General Relativity, which shows the notion to rest on certain implicit, theoretical assumptions about how the world is constituted, specifically assumptions about the velocity of light). She concludes that all predicates that are to serve in inter-subjective communication have a theoretical component to their meaning.

This theoretical component does not spring from a simple one-to-one nexus between a term and a particular theoretical assumption. Rather, it is a one-to-many tie, since the theoretical term to which we are first led will inevitably involve links to further such terms; moreover, all these terms will have an inferential trail leading back to their evidential basis and the terms in which it was couched. Thus, the theoretical and observational terms of science form a large network, where the concepts are the nodes and the strands consist of the inferential connections between them.

Like its Quinian source, Hesse's network model repudiates the standard division into inductive and deductive ties. Empirical predicates are typically first introduced in language on the basis of situations of direct sensory ostension of an object and this procedure leaves a permanent residue in their meaning. But later, the subject acquires theories in which the properties (predicates) thus acquired are tied together with other properties (predicates); these ties sometimes give us sufficient grounds for withdrawing a previous application of a predicate, based upon simple sensory inspection. Thus the theoretical embeddedness of predicates directs various inferential forces upon judgements featuring those predicates. However, there is no basis for drawing a sharp distinction between deductive (analytic) and inductive (synthetic) ties. All we can record is the epistemic point that these ties are sometimes strong enough to force revision even of judgements based upon direct inspection.

We should view Hesse's account as a mainstreamed version of Quine's position in *Two Dogmas of Empiricism*, purged of the eccentricity of seeing sentences as linked together by the force of Skinnerian operant conditioning. What is left is the general view that the terms of language are connected into a vast crisscrossing network of inferential ties, with no distinction to be drawn between deductive and inductive strands. This also means that the model of the language of science is at the same time a model of our knowledge of the world. The two collapse into one, once the inductive ties – seen as a part of our *theory* of the world on a standard account – become a part of the *meaning* of the terms occurring in it.

In Collins's adaptation of this model, the original Quinian idea undergoes a further transformation, in the form of a *sociologization* of its fundamental elements. Inferential ties are now reinterpreted as vectors of social interest. Here, we have reached the third and final stage of EPOR, in which we follow the processes through which scientific closure is achieved as a case moves out of the scientific community and into society at large.

We may illustrate the basic mechanism by elaborating upon a little example found in Hesse (and repeated by both Collins and Barnes). Imagine a community that is familiar with fishes and terrestrial mammals, but has never encountered a whale. Its concept of a fish would comprise such features as living in water, having fins and laying eggs, whereas its concept of a mammal would include being warm-blooded, living on dry land, having legs and having viviparous reproduction. Now, consider what happens when these people observe a whale for the first time. This discovery will create tensions within their conceptual network, since the animal cannot be fitted into any of the ready-made slots. It is warm-blooded and viviparous, which disposes towards classification as a mammal, but also lives in the water and has fins, which makes it similar to a fish. As there are no strictly analytic, definitional conventions dictating the answer, closure will be determined by various interests, extant in the wider community, which will pull at the strands of the conceptual network. Local fishermen might be interested in commercially hunting the newly discovered animal, a concern that would be promoted by classifying the animal as a fish and thus a permissible food during periods where religions customs forbade the consumption of meat. Hence, it would be in the interest of this segment to pass the animal as a whale-fish, which might agree with an interest in the larger community to enjoy a more varied diet during periods of fast.

Notice that this account is well-suited to explain differential classifications in different social groups and thus conforms to the relativistic agenda of Collins's project. A neighbouring society, which did not share the dietary constraints of the first one, might be so distressed by the manner in which this animal defied standard classifications that they put it in a category all by itself, and declared it to be unclean and unfit for human consumption.

10. Let us pause briefly to ponder the ways in which this quasi-Quinian picture goes beyond the largely Wittgensteinian picture discussed in relation to Bloor (although Bloor himself mixes a little Quine, mediated by Hesse, into his argument). According to Wittgenstein, every individual application of a term is perfectly insulated, not only from the push of all its previous uses, but also from the pull from other terms with which it is interdefined, according to the standard conception. Past use of a concept does not constitute a set of logical rails pointing future use in any particular direction, nor does its interconnection with other concepts form a network through which interests may pull at it. What determines the actual use of a concept in an individual, according to Wittgenstein, is only the causal efficacy of the drill originally undergone with that concept. In Quine and Hesse, on the other hand, semantic forces of both the push- and pull variety are at play. The push is supplied by relations of likeness or analogy with previous cases; the pull, by practical interests in classifying things together or keeping them distinct.

As a matter of fact, there is a certain tension between the idea of Hesse nets and the radical conventionalism of Wittgenstein (on the communitarian interpretation). Considerations very similar to those that are fielded here by Hesse and Collins have actually been used to rebut the Wittgensteinian picture. We soon shall return to this criticism, not so much in order to show that Wittgenstein and Quine between them form an unstable basis for Collins's position (although they do) but because this criticism may have a bearing upon Collins's sociological reinterpretation of Quine as well. The ties between concepts could be taken to prove that there are objective relations of similarity after all, when the entire network of concepts is taken into consideration.

11. So much for a presentation of Collins's position. Now for criticism. First, we may observe that Collins's contributions have not moved our understanding of the scientific process very much beyond what is already captured in the notorious problematics concerning the under-determination of theory by data. The "experimenter's regress" just adds an extra epicycle to the celebrated Quine–Duhem argument and makes it possible to enlist the authority of Wittgenstein in support of EPOR.

By "data", let us agree to mean experimentally generated data – more specifically, very "raw" data, consisting in the bare recordings of the readings of measuring apparatus. Now, the standard under-determination thesis says that such data are always open to numerous alternative interpretations. For instance, it is always possible is to construe them as due to malfunction of the measuring apparatus, in which case they do not even qualify as evidence *pro* or *con*, relative to the hypothesis under examination. Collins's discussion basically just reiterates this familiar point, with a slight elaboration that serves to block a possible rejoinder: If we suspect a malfunctioning of the measuring apparatus, it might he claimed, we should simply build a second apparatus, and run a new sequence of experiments with it. Collins's reflections show that this option does not significantly change the basic predicament. Even if the new series of tests issue in a different outcome, it is still open to us to stick to the original experiment, on the grounds that it is the new session that is flawed. This is the "experimenter's regress" (or vicious circle), which is just a further complication of the general issue; as it were, an epicycle on the circular movement between hypotheses and data (which incidentally is close kin to the "hermeneutical circle").

Another objection: It may be doubted whether the inferential ties between scientific concepts are smoothly reinterpretable as social ties (more precisely ties expressing purely practical interests), when such ties extend from the scientific community into general society. To substantiate this doubt, we may elaborate upon an example originally adduced by Simon Blackburn against the radical conventionalism both of Wittgenstein's rule following considerations and of Goodman's "bent" predicates (Blackburn 1984, p. 74 f).

According to Wittgensteinian principles, a person who had been trained to use the term "circular" by ostension of such items as dishes, wheels, coins and CDs, might well go on to apply the term also to shapes that the rest of us would describe as *square*. Now, suppose this person sets out to build a car, but, oblivious of any difference between round and square objects, fits it out with wheels of the latter shape. Our subject may now choose to persist in his predicate-bending ways, insisting, for instance, that the bumpy ride of his care is *the same* as the smooth ride of his neighbour's car, or that it actually took him *the same* time to drive to the office as his colleagues. The situation is even worse if we assume that some of the cogwheels in the engine and the gearbox have been replaced with square parts as well, rendering his car totally immobile: the owner is now forced to insist that the absolute immobility of his car is *the same* as the unimpeded roaming about of other vehicles. (We have to assume, of course, that our subject's behaviour matches his linguistic output, and that he would not surreptitiously introduce behavioural discriminations that are not reflected in his verbal classifications – such as, tacitly but systematically picking circular rather than square parts when it comes to repairing his car. If not, the entire thought experiment we are invited to conduct concerning "bent predicates" becomes a sham).

Alternatively, our subject might try to block the spread of linguistic anomaly by proffering some esoteric explanation of the poor performance of his car, other than a difference in the shape of the wheels; postulating, say, trouble with his shock absorbers that just cannot seem to get fixed. Such explanations would have to be employed on a large scale, of course, since the implications of our subject's non-standard linguistic practice would rapidly spread to every corner of his life. We would eventually be forced to treat him as a madman, living in his own cloud-cuckoo world.

Examples of this kind could evidently be multiplied and, according to Blackburn, serve to raise doubt as to whether we can really meaningfully imagine a community where predicates are used with Wittgensteinian latitude. By the same token, they also cast doubt upon Collins's less radical position, to the effect that only practical interests save us from living in a Wittgensteinian world, for it seems odd to say that the problems faced by somebody who holds that circularity and squareness are the same are *just* practical. We have difficulty comprehending how somebody could have cogent thoughts couched in such consistently "bent" terms. A person of this kind would be so alien to us that we could hardly make sense of what he said to us.

His speech would be as incomprehensible to us as that of the fabled talking lion of the *Philosophical Investigations* (op. cit., p. 223).

12. So much for some worries pertaining to various philosophical props supporting Collins's argument. We now turn to some core elements in his position. In previous chapters, we criticized the Edinburgh school on a number of points, and we have noted the affinities of Collins's position with this school. To what extent, then, is he vulnerable to the same objections?

One objection had to do with the Wittgensteinian semantics we have just examined. The problem was that, if the rule-following considerations are used to save theories from negative experimental outcomes, those theories become semantically indistinguishable from their rivals; more precisely, they will be afflicted with a local indeterminacy of meaning coinciding precisely with the apparent semantic difference between the rival theories. Is this problem still with us, after we have moved to a Quinian picture, which grants that interrelation between terms puts constraints upon concept use?

The answer seems to be that although one could easily adopt a reading of the Quine–Hesse semantics that would render conceptual networks rigid enough to prevent indeterminacy, such a reading would also make them so rigid as not to bend easily under the impact of interests. We may illustrate this with the aid of our little example above: One possible strategy of the people confronted for the first time with a whale might be to resort to a game of "naughty pupil" (i.e., predicate-bending), insisting that its fins are legs and that its newborn whale calves are actually eggs. Certainly, nothing prevents this, according to Wittgensteinian principles, and the introduction of Hesse networks will not block it either, on Collins's reading. It is a question of the strength of the social interests lined up behind this odd practice. In a rigidly conservative, religiously dominated society, a strategy of denial effected through the bending of terms might be used to defend the principle, say, that the deity had instituted a strict separation between land-living and sea-living creatures. This policy, however, would inevitably lead to a (partial) semantic indeterminacy in the issue being fought over.

Collins makes it clear, however, that this would not be the only possible policy. Drawing upon work by Bourdieu (1975), he outlines a number of different options available to scientists, corresponding to different career strategies. According to a high-risk strategy, the scientist will choose to work in novel and controversial fields that have a high risk of going nowhere, but where the potential gains are huge in case of success; in a low-risk strategy, on the other hand, the scientist will adopt a received theoretical framework where "normal-scientific" progress is almost guaranteed, but results of revolutionary import are ruled out.

However, this does not save the day for Collins. He is faced with the same dilemma with which we confronted Bloor in the previous chapter. *Either* the conceptual framework within which science is conducted is so flexible that all parties to a scientific controversy are free to describe the outcome of an experimental test in whichever way will support their own position, without thereby breaking any semantic rules. This saves the thesis of the "experimenter's regress", but at the price that the controversy the experiment was meant to decide is no longer over a substantial issue; the rival theories have been rendered semantically equivalent. *Or* such resistance to a negative experimental outcome is not possible without limit: there is a point beyond which the conceptual structure cannot be stretched without breaking (as illustrated by Blackburn's example above). This saves the semantic distinctness of the rival theories; the scientific debate will indeed be over a substantial issue. But by the same token, an opening now appears for the impact of reality upon the scientific edifice, channelled through a medium with an inherent rigidity that prevents it from being made just one more instrument for rival interests. In other words, the door is opened to a *realist* interpretation of experiments.

13. Another objection to the Strong Programme had to do with the overly ambitious goal of explaining scientific theories in social terms. How does Collins fare on this account?

It is evident, in the first place, that Collins wants science studies to have explanatory import. This is not set forth as an explicit principle, but the point is made very clearly in Collins's polemics against Bruno Latour, whom he criticizes for providing merely a descriptive framework, or even a mere metaphor, for science, but devoid of explanatory power (Collins and Yearley 1992a, p. 322 ff).

It is much less clear what *kind* of explanation Collins has in mind. We have seen that he distances himself from the way in which natural science ideals have permeated Bloor's methodology. We may safely assume that this refers to Bloor's adoption of strictly deterministic, causal accounts, which is "material mode" for Hempelian deductive-nomological explanation. We saw in Chapter 3 what difficulties this model brought with it. However, Collins's rejection of Bloor's model is not accompanied by an explicit adoption of an alternative model. As a matter of fact, Collins is even more discreet on this issue than Bloor, which is quite remarkable, given the emphasis they both put upon the explanatory ambitions of their programmes.

If Collins is silent on the philosophical meta-level of theory of explanation, perhaps there are some hints at the level of theoretical, scientific commitments. Scientific theories, or *types* of such theories, are often implicitly linked with particular ideas of explanation, such that an answer to our question could be reached by this route. Now it is evident from *Changing Order* and other texts that Collins favours a construal of science as a network. Yet this stance is open to various interpretations. Comments in *Changing Order* (p. 183) indicate that he does not consider the network model to be a substantial theory in its own right, but rather to express the formal structure of any viable theory applicable to the field. Thus, it is not in itself an attempt to provide theoretical explanations. It is clear from other texts, however, that Collins's also looks favourably upon network theory construed as a specific approach to sociological explanation. But, as he himself confesses, this kind of theory is – so far – very deficient in explanatory power. This is basically a problem of measurement, according to Collins. Weak social ties (in the sociometric sense of "weak") may be crucial in the transference of tacit scientific knowledge, but such links are, by definition, only marginal in a sociometric analysis and there is so far no alternative way in social science to operationalize and measure them (Collins 1974; Collins and Yearley 1992b, p. 375 f).

Perhaps we get the best insight into Collins's ideas on explanation from his empirical studies. On inspection, these prove to be very similar to historical case studies. For instance, data about the protagonists' personal background is allowed to play a considerable role. Thus, much is made of Joseph Weber's earlier career as a navy commander and expert on anti-submarine warfare, in which the mandatory epistemic strategy is to avoid "false negatives" – that is, undetected enemy submarines – at all cost; false positives are a minor nuisance in comparison. This strategy was supposedly perpetuated in Weber's scientific career, where he calibrated his measuring apparatus so that it would never miss a pulse of gravitational radiation, at the cost of making it overly susceptible to artifactual effects (Collins 2004, p. 143). Thus, Collins's methodology focuses upon particular causal factors, but with little reliance on general laws or principles under which they may be subsumed. This is indeed consistent with Collins's opposition to the methodological ideals of the natural sciences.

A striking thing about Collins's case studies is that Network Theory, which is hailed as a crucial insight in the philosophical texts, quietly recedes into the background in the empirical work. What takes its place is an account of the scientific community dividing up into a number of concentric rings, arranged around the "core set" (Collins 2004, Introduction). As a matter of fact, there is a certain tension between the two pictures: according to network theory, science and the wider social context in which it is embedded form a seamless web of societal relations. According to the later picture, on the other hand, there are definite structural discontinuities in the socio-epistemic world, defined by the shifts encountered when one moves from one circle to another.[36]

14. Whatever Collins's conception of explanation may be, there are two considerations that make his explanatory burden easier to bear than Bloor's.

First, as we mentioned above, he eschews explanation of the *genesis* of scientific theories in terms of purely social parameters and undertakes to explain only their *reception*. In other words, he eschews what I termed Type I-explanation, in a previous chapter, and the overextended commitments they bring with them. This safeguards Collins from the objections raised against this mode of explanation. The price, of course, is that this part of scientific practice is now left unexplained.

Secondly, Collins's commitment to a social explanation of science is often presented as a *methodological recommendation*, not as a substantive thesis. Collins urges sociologists of science to proceed as if the contribution from physical reality counted for nothing. Thus, they should start out by looking for the social determinants of a given scientific development (the closure of a particular controversy) and, only where such factors do not tell the whole story, resort to physical reality as the determinant, by default, of those aspects of the process that cannot be fully accounted for by sociological factors: "The approach we favour is to push the relativistic heuristic as far as possible: where it can go no further, 'nature' intrudes" (Collins and Cox 1976, p. 439); "Do sociological analysis which takes it that reality in no way constrains what is or can be believed to be" (Collins and Cox 1977, p. 373). This methodological recommendation nicely stands the procedure of Mertonian sociology of science on its head. The latter would start out by construing a scientific development as the accommodation of certain new observations or experiments (i.e., as the result of interaction with physical reality) and would only resort to social influences to explain disagreements between rival interpretations of the data. This means that only the latter exercise would be a part of sociology of science proper.

However, Collins's redefinition of his stance as methodological may appear to be nothing but a ploy, as long as a precise notion of explanation has not been provided. A comparison of his position with that of the Strong Programme may serve to bring this out. Bloor claimed to be able to account for the development of science in purely societal terms, deploying a strong, deterministic notion of explanation which, as I argued, may be considered materially equivalent to Hempel's deductive-nomological model. If this goal could be achieved, there would be no room for rival explanations, such as "rational" ones. As I argued in a previous chapter, this calls for impossibly strong sociological theories, however. True, this problem may be avoided if the stringent explanatory model is relaxed; but now there is no longer a valid move from the possibility of social explanation to the irrelevance of rational explanation.

By recasting his position as a purely methodological one, where explanation proceeds in terms of social factors and recourse to physical factors is only had where this approach fails, Collins deftly shifts this problem onto his opponent, who is now obliged to show, in any concrete case, that the sociological explanation offered is not complete. Given a weak and rather underspecified notion of explanation, a compelling case to this effect can never be made; it will never be possible to convict EPOR of leaving something out. The suggestion that we have an effective way to test Collins's "methodological proposal" is a sham (cf. *Changing Order*, p. 185).[37]

As a matter of fact, there is a deeper reason why this issue will hardly be resolved at the purely empirical level. Both Collins's and Latour's positions are determined by philosophical background assumptions that are not susceptible to resolution by scientific means. Collins's philosophical background assumption is that agency can only be attributed to human beings, never to mere things. This thesis is derived from Wittgenstein's and Goodman's demonstrations that rule-following, in particular with respect to semantic rules, is a tacit, not-explicable practice. The thesis itself has two fields of application, one narrow and one broad. In the narrow application, it implies that we shall never succeed in designing machines with the cognitive powers of human beings, since these powers are essentially non-explicitable (non-codifiable) and will never be captured in a computer programme designed to mimic human intelligence (Collins elaborates this point in a book entitled *Artificial Experts*, 1990). Hence, we need never grant computers and other "smart" machines genuine intelligence and genuine agenthood. In the broad application, the thesis refers to physical objects in general and says that such things may never be allowed to "act" on us in the sense of imposing obligations upon us, or of making us answerable to them. In particular, they may never impose obligations upon us to describe them in any particular way. For instance, the sheet of standard typewriter paper sitting on my desk does not commit me to describe it as "rectangular"; I am free to describe it as circular, if I so wish.

Given this liberating background principle, a representative of EPOR will never be forced to admit that nature had an impact on the course of a scientific controversy. It plays a role only if such be freely granted by the real agents (i.e., human beings); once this is admitted, the real issue now immediately becomes what motivates such concessions. The answer, by definition, lies with social causes. The debate between Collins and Latour is basically metaphysical and can be decided only by philosophical argument. (In the next chapter, we shall see what Latour's philosophical commitments are.)

Collins indicates that the merits of his proposal are to be evaluated in terms of its "fruitfulness" (1992, p. 185). There is a hint of disingenuity in invoking this notion as a neutral yardstick, given that the tenor of Science Studies is precisely to show that the merits of a theory must always be assessed in the light of particular partisan interests. In any case, Collins is caught in the same dilemma as Bloor. Either a strict notion of explanation is adopted, with the problems this entails in a social science context, or a laxer notion of explanation is introduced, more in keeping with social science practice, but at the cost of rendering empirical science studies impotent as an instrument for the naturalization of philosophy of science. The sociological account now no longer preempts the philosopher's traditional questions concerning rationality and truth. Yet despite rejecting Bloor's stringent notion of explanation, Collins actually suggests that an exclusionary relation exists between his preferred kind of explanation and the orthodox philosophical kind (ibid.).

15. We recall that Collins's programme is named the Empirical Programme of Relativism. This signals that relativism is not considered an embarrassment to the programme; on the contrary, Collins takes pride in having uncovered the inherently relativistic character of science. We must examine if this sanguine attitude is warranted.

Collins's chief interest is in *epistemological* relativism, that is, a relativism of the standards scientists use to decide if a particular claim is true, or probable. We may here bypass the fact that, in a couple of early articles, Collins strongly champions a relativity of *truth* as well (e.g. Collins and Cox 1976). This is a far more radical claim, which we may attribute to youthful overenthusiasm. At any rate, a relativism of truth would not seem to do anything for Collins's mature position, since he is primarily concerned with the forces determining the reception of a scientific claim in the wider community, once the initial phase of experimental work is over. What the sociologist of science is concerned with is the *interpretation* of the experiments, the *reception* of the interpretation in the wider community, and the forces that shape the two.

In Chapter 3, I pointed out that a naturalistic, empirical relativism that represents science as shaped by societal contingencies makes it impossible to view science as truth- or reality-tracking. If the main determinants of the closure of scientific debates are, for instance, social interests, variously distributed in the wider society, then the scientific process tracks the property of satisfying interests (or of appearing to do so), not that of being true. Any other view would make the power of science to detect truth look like a miracle. Such a view would have it that although, in its historical development, natural science was always insulated from any determining influence from physical reality, it nevertheless acquired that kind of match with reality we call "truth". This would clearly have to be the result of a pre-established harmony.

This conclusion, which seems to constitute a *reductio* of the assumptions that generated it, might be resisted on the grounds that the talk of a "match" or "harmony" between theory ad reality is totally vacuous; even the notion of "truth" might be dismissed, at least in anything hinting at a substantial, correspondence version. All we have is the power of our theories to assist us in our dealings with reality (i.e., to produce correct predictions of observable happenings) and to help us manipulate it. Such service, however, may be delivered equally by a number of rival theories. When closure is attained, an agreement is established concerning the theoretical interpretation of the data, which involves a conceptual fleshing-out of the bare bones of the abstract mathematical structure that does the real work of prediction. Such theoretico-ontological dressing-up largely serves social purposes. Its particular shape is chosen in the light of which model is best suited to prop up the current social order, or protect the interests of a dominant class. But there is no substance to the idea that this conceptual structure "corresponds" to reality, or is in any explicable sense *true* of it. What we get is thus a strongly *instrumentalist* conception of science, and a highly deflationary notion of truth as it applies to scientific theories. On the basis of such a conception, the epistemic relativism espoused by Collins might appear rather innocent.

Yet this argument will not do. The claims made for instrumentalism above are, at best, true only in a *retrospective* perspective. They imply that at any time, we can find alternative, equally good theoretical construals of the total body of observations already made, with no way of choosing between them *on that limited basis*. It does not follow, however, that these theories will handle *future* observational data equally well; they will typically vary in their future observational predictions, which means that it is not a matter of indifference which one we adopt *now*. Moreover, this also leaves them open to empirical testing. True, instrumentalism implies that when the new batch of data is in, there will once again be several different but equally good ways to interpret the new, enlarged body of evidence. But these theoretical construals will not necessarily be the same as the ones we used before, our favoured construal may have been discredited; and anyway, the argument does not eliminate the problem that even these new theories will have different future observational implications, thus forcing us to choose between them. The problem posed by the interest-dependent nature of theory choice is that it prevents our truth-tracking efforts from reaching their goal, even with respect to the purely observational aspect of theories.

It is not clear whether or not Collins actually adopts an intrumentalist view. Certain statements on his part are strongly suggestive of this position; thus in "Special Relativism – The Natural Attitude" we find the following pronouncement: "It is often worthwhile to proceed in the basis of assumptions that are implausible.

The value of an assumption lies not in its plausibility, not even in its truth; it lies in the value of the work that it precipitates" (Collins 1982, p. 140). This is highly reminiscent of Milton Friedman's radical instrumentalist position as laid out in "The Methodology of Positive Economics" (Friedman 1953). At any rate, as I argued above, instrumentalism in itself will not resolve the internal difficulties of the epistemic relativism proposed by Collins.

16. As I argued in our discussion of the Strong Programme, a dramatic way to present the problem of relativity is as a problem of *reflexivity*. When we apply Collins's epistemology to his own work, his case-studies can no longer be construed as truth- or reality-tracking. It would be a mere coincidence if truth were to come out of them. So why should we trust them? In particular, why should we do so if we do not subscribe to the values that motivated their production?

We noted initially that Collins rejects Bloor's principle of reflexivity. This dismissal is convenient, but will not make the kind of worry articulated above go away. So, how will Collins confront his critics on this score? As it happens, Collins is rather scornful of the standard philosophical arguments against relativism. In the first place, he depicts the philosophers' worry as reflecting a preoccupation with *justification*. Next, he identifies that preoccupation with the concern to find a *foundationalist* grounding for scientific judgements. Finally, this foundationalist project is dismissed, with reference to Wittgenstein's observation that the "chain of reasons" eventually comes to an end.[38] In other words, Collins argues that philosophers see relativism as a threat to the project of reaching a firm, absolute foundation for our convictions about the world. But Collins impresses upon us that such a foundation was always a pipe dream (2004, p. 762 f).

However, relativism represents a much more specific and more virulent threat than this construal allows. One aspect is precisely the problem of reflexivity: Collins needs to tell us why we should give credence to his picture of the scientific process, if the world, as he insists, played little or no role in shaping it and if his conclusions mainly reflect epistemic standards that are shaped by his own social context and which may not necessarily be shared by the rest of us.

Collins has actually developed a stance supposedly permitting him to escape this predicament, referred to as "meta-alternation". The sociological observer of science may freely adopt a realist stance to his object, although his constructivist commitments imply the opposite. He is allowed to conceive of himself as interacting with an objective social world existing independently of his research efforts and to report his findings as if they somehow reflected that reality. Without such a realist stance, we will make no progress in our scientific efforts, be it in natural or social science. If the physicist is not convinced of the reality of the elementary particles he investigates, a sense of unreality will soon invade his work; his efforts will lose motivation as well as direction.

This conclusion also applies to sociologists of science. They, too, are allowed to assume that their investigations bring them into genuine contact with those social activities labelled "scientific research" and that the reality encountered is somehow captured in their observational records and later reflected in their theoretical conclusions. They need not worry that these comforting assumptions are explicitly contradicted by their meta-theoretical stance, which views the ensuing results as determined by the social context of the investigation. In the case of Science Studies, this context would include the rivalry existing between different schools of science studies, the confrontations with orthodox philosophy of science, the wider societal interests in coming to grips with the social institution of science, the importance of opening it up to greater social accountability, and so on.

Collins construes this stance as a refinement of the sociological methodology proposed by Peter Berger under the name of "alternation" (1963). The traditional sociologist (or anthropologist) alternates between the beliefs, assumptions and normative standards of his own society and those of the target society when he "goes native" in order to explore that society "from within". Alternation is the core of the methodology of participant observation, where researchers immerse themselves in the society under study in order to get access to those social facts that are hidden to an outsider.

Meta-alternation is alternation at a higher level or, perhaps, of a more comprehensive kind. In simple alternation, the sociologist temporarily suspends belief in the common lore he shares as an ordinary member of his own society, but not in the convictions he holds as a researcher, including those that define his specific methodological stance. Thus, he will stick to his scientific ideals, although the tribe he observes may subscribe to pre-scientific notions of how knowledge is acquired. They may consult oracles to learn what is in store for them and regard scientific techniques of prediction with incredulity.

In meta-alternation, the researcher will also suspend his general theoretical and philosophical principles, whether general or specific, if this promotes his fieldwork. Thus, when he "goes native" among a group of physicists, the constructivist sociologist will suspend his constructivist commitments, as they bear on his own professional efforts. He will adopt a realist attitude, seeing himself as interacting with a segment of social reality that is really "out there" and that determines, or a least co-determines, the conclusions to which he will arrive. Without this assumption, his dealings with his "informants" will be devoid of content.

Collins apparently believes that this manoeuvre allows him to finesse the reflexivity objection. He admits to an inconsistency between his relativist position, which says that reality plays no role in shaping our scientific thinking about it, and his energetic efforts "in the field" to produce a body of empirical case studies, which allegedly prove the truth of that position. Yet he apparently thinks that the practitioner of EPOR escapes this dilemma, since he never occupies the positions of theoretician and field worker simultaneously; just as the ordinary anthropologist is not "native" and theoretician at the same time.

This solution, however, overlooks the fact that in the role as a theoretician, the relativist sociologist of science will adopt both realist and constructivist stances simultaneously (cf. *Changing Order*, p. 188). In working out the theoretical interpretation of his empirical data, the sociologist will try to demonstrate that physical reality had nothing to do with the conclusion reached by his subjects of investigation – say, a group of laboratory physicists – when they discussed the validity of a given experiment. But he will also want to urge, at the same time, that *this very finding* within the sociology of science bears the imprint of reality; that is, of the actual processes of scientific closure as captured in his carefully collected data. Without this realist assumption, he has no basis for his claim and no business to clamour for his readers' attention. When Collins engages in debate with his philosophical critics, however, it is Collins the methodologist who occupies the scene, not Collins the field worker. The former must own up to his relativist principles, on pain of insincerity; yet in this incarnation, he will not persuade his opponents. No matter how we look at it, Collins undercuts his own position when he draws a relativist conclusion from his empirical studies of science.

17. In the preceding pages, I focused attention upon the early and middle periods of Collins's work, which may justly be seen as carrying on the Edinburgh Strong Programme, despite a minor heresy with respect to the scope of explanation. Collins has described such work, both his own and that of contemporaries, as constituting the "Second Wave" of science studies (with orthodox Mertonian approaches making up the First Wave). However, in a seminal article from 2002, "The Third Wave of Science Studies: Studies of Expertise and Experience", Collins launched what he himself termed the "Third Wave" of Science Studies (Collins and Evans 2002, 2007).

The Third Wave is defined by a shift from a stance that was officially purely descriptive and explanatory to one that is openly normative. While the Second Wave prided itself on its strict scientific neutrality, the Third Wave accepts the responsibility that comes with STS's pretensions of being the pre-eminent "science of science". STS'ers are experts on scientific knowledge, and, like all experts, they should be willing to offer advice to politicians, administrators and "science managers" as to how science should be organized and conducted. The notion of *expertise* is indeed at the very core of Collins's project, since the goal is not to improve science's ability to find *truth*, but instead to develop a legitimate notion of scientific expertise and define a proper societal role for it. To this end, Collins introduces what he terms *a realist theory of expertise*. Expertise is no longer to be considered entirely a matter of social attribution, as Second Wavers would have it, making anyone an expert upon whom a general consensus would bestow expert status. Expertise is (at least in part) a matter of skills that are genuine possessions, independent of their social recognition.

There were anticipations of this transition in Collins's middle-period work. For instance, the second edition of *Changing Order* features a new Postscript, entitled "Science as Expertise", in which a midway position is adopted. In conformity with orthodox STS (the Second Wave), the homogeneity of science with other social enterprises is stressed, as is the continuity between the scientific community and the larger society in which it is embedded. Still, Collins emphasizes that a scientific discipline, represented by its core set, embodies an expertise that must be recognized. He suggests a distinction between science as a general social *authority*, a status that should be opposed, and as *expertise in restricted fields*, which has a legitimate societal role. (This distinction, by the way, actualizes the latent tension in

Collins's work between the network metaphor, which implies continuity, and the non-continuity inherent in the picture of the core set and the circles surrounding it.)

Of course, Collins is not oblivious to the fact that a general normative stance was present in STS all along, a desire to strengthen the accountability of science to the public. Collins's own voicing of such a concern was cited at the start of the present chapter. This goal was typically represented as somehow incidental to STS's scientific endeavours, however, for reasons that I indicated in Chapter 2. Collins hints at these reasons when he remarks that the rather monotonous way in which traditional STS work would point to ways of empowering local interests vis-à-vis scientific authority might raise suspicions about its acclaimed neutrality (Collins and Evans 2002, p. 263). To counteract such suspicions, STS'ers would avoid issuing explicit and concrete recommendations with respect to the proper conduct of science.

By contrast, the normative ambitions of the Third Wave are explicit and concrete. They are also somewhat transmuted, however – at least in Collins's version. Whereas the original STS concern was to increase the amount of public involvement in science, the new approach will occasionally limit it, in the interest of balancing off *legitimacy* and *extension*. The former is the familiar concern to strengthen the democratic legitimacy of science by making it more answerable to general societal interests. The concern with *extension* is the attempt to draw proper boundaries around popular involvement in the scientific process. A distinction is reinstated between experts and laymen that had been deconstructed in Second Wave work, with a view to avoiding the paralysis of decision-making that frequently follows from popular involvement in science. This often overly focuses upon negative consequences, especially as they impinge upon narrow local interests (as in the familiar "Not In My Back Yard" attitude). However, the division between experts and laymen is partially redrawn under the Third Wave regime: social groups may be found to have acquired genuine expertise in ways that are not formally recognized and accredited; hence they may earn a legitimate role in the decision process. The main source of such expertise is *experience*.

18. Collins's introduction of the Third Wave illustrates the drift in STS that I have been at pains to point out. I venture the interpretation that it is motivated not only by the monotony of the results in the normative dimension upon which Collins comments, but in the descriptive and theoretical dimensions as well: The Second Wave had reached a point of diminishing returns in their empirical work, accompanied by theoretical stagnation. One case study after another faithfully reproduced the same conclusions: that is, that the production of science was the result of historical contingencies and local interests (cf. Fuller 2000b, p. 336 f). Disturbing signs of a Lakatosian "degenerating problem shift" were clearly in evidence, and a new tack was called for.

To some extent, the Third Wave represents a return to certain of the normative evaluations that were inherent in orthodox sociology of science. Does it also imply backsliding in matters of epistemology and ontology? Such backsliding might render Collins's new approach less vulnerable to our previous criticisms. The answer is that, although the Third Wave does indeed reintroduce certain distinctions that were characteristic of the First Wave, there is no going back on the chief constructivist tenets of the Second Wave.

In the first place, while the shift implies a commitment to realism, it is realism merely with respect to *expertise*, not with respect to *truth* (Collins and Evans 2002, p. 236). The point is made that expertise is not solely a social status, but a skill existing independently of public recognition. However, we still do not get a realist theory of the *knowledge* possessed by the experts; Collins still opposes a general realist interpretation of knowledge. We may see this as a case of Collins's strategy of meta-alternation: The objects of STS work, such as expertise in detecting gravitational waves, are granted a realist existence, while the gravitational waves themselves, the objects that physicists are concerned with, are still viewed according to a constructivist perspective.

Constructivist orthodoxy is also apparent in the way Collins justifies reliance upon the expertise of *scientists*, rather than, say, the clergy, or the community of astrologists. A realist would unabashedly define expertise as involving proficiency in getting at the *truth* about the world; and he would consider professional theologians and astrologists to be wanting in this respect. This justification is not available to Collins; instead, his justification is couched in purely sociological terms: In the final analysis, scientific expertise is preferred because its recognition constitutes a crucial aspect of the kind of society in which Collins lives (along with most of his readership), and which Collins values (Collins and Evans 2002, p. 243 f). Nothing is said to analyse or justify this preference; in the end, "this is just what we (modern Westerners) do". What we get is a thoroughly social constructivist conception of the normative notion of (scientific) expertise. Thus, Collins still hangs on to all the relativistic elements of his position.

It might be suggested that Collins's emphasis upon *experience* as a defining characteristic of expertise embodies a tacit concession to realism (Collins and Evans 2007, p. 68). But here we must remember that reference to experience is invoked to define expertise across the board, not only with respect to empirical knowledge. This means that we must construe "experience" very broadly here: It calls for previous *practice*, rather than the possession of a store of past sensory experiences. The broader notion of experience-as-practice that is involved here does not call for a realist interpretation; and none such is intended by Collins.[39]

19. It is time briefly to summarize our results. In Collins, we find important retractions from the radical position of the Strong Programme. Most significantly, there is an abandonment of the claim that the sociology of science can explain every facet of science, including the genesis of theories, and manifestations of genuinely creative scientific thought. What can be explained, according to Collins, is merely the reception of theories, once they have been broached. There is also explicit opposition to the Strong Programme's very strong notion of explanation, which was necessitated by the strategy of proving that the philosophers' favoured "rational" explanations are otiose. Collins urges that a mode of explanation be adopted more in line with standard sociological concerns. Unfortunately, there is no indication of what such an explanation might look like.

These retractions render EPOR fairly immune to some of the objections directed at the Strong Programme. The drawback, however, is that they undermine the strategy of outflanking the philosophical conception of science, by showing that a complete explanation of science's progress can be given without ever mentioning evidence or rationality; only social parameters need be invoked. Finally, there is also a retraction from the reflexivity requirement. This move is detrimental to the programme. In making it, Collins undercuts his own position when he uses his empirical studies of science to support a relativist thesis.

Like the Strong Programme, Collins creates problems for himself with the blending of philosophy and empirical findings in his argument. In particular, his reliance on the Wittgensteinian rule-following argument trivializes the cases of scientific controversy that are the lifeblood of his approach, by rendering the rival positions equivalent *modulo* semantic indeterminacy. Similarly, it renders Collins's methodological position invulnerable to empirical falsification and thus turns it into a purely metaphysical stance.

What we end with is a catalogue of case studies in scientific controversy, demonstrating, in particular, the vicissitudes of experimentation. These are valuable findings, but they are hardly distinguishable, in their methodology, from typical studies produced by historians of science. There is no robust sociological theory behind them – only the loose conceptual metaphor known as the "network theory". Once the *succès de scandale* surrounding the first generation of STS publications had worn thin, a certain tedium set in, and the continued results generated by Collins and his fellow "Second Wave" STS'ers eventually come across as rather repetitive. I have suggested that this is a major reason why Collins has lately initiated the "Third Wave" of Science Studies, in which the implicit normative agenda of the programme has been moved to the very forefront of its activities.

Chapter 6
Bruno Latour and Actor Network Theory

1. In the previous chapter, we examined a position within STS that made a fairly modest departure from Edinburgh orthodoxy. The next two chapters, on the other hand, are dedicated to a figure who represents a much more radical heresy against the principles of Bloor's Strong Programme. This is the French philosopher-sociologist Bruno Latour.

As he himself tells the story (in Latour 1999a), Latour started out as an adherent of ideas originating in the Strong Programme. He saw them as useful tools in combating the contemporary French *épistémologie*-tradition (represented by Gaston Bachelard and others) which would stress the discontinuities between scientific and everyday thinking and would treat true knowledge and false belief non-symmetrically. In the work that first won him wider recognition, *Laboratory Life* (written with Steve Woolgar), positive references to the Strong Programme abound: "By maintaining the sense in which we use social, we hope to be able to pursue the strong programme at a level apparently beyond traditional sociological grasp" (Latour and Woolgar 1979/1986, p. 152; also pp. 105–107). The goal is to give a thoroughly social account of the findings of science.

At the outset, the approach shared the Strong Programme's ambition to *explain* scientific results. The rationale was the very same that propelled the Strong Programme: By fully explaining scientific results in purely social terms, we can demonstrate that science is indeed not exceptional, but rather social through and through. Latour and Woolgar draw an instructive parallel with the debate between vitalists and mechanists in biology in early 19th-century biology: So long as certain aspects of biological processes were left unexplained in mechanistic and materialist terms, adherents of vitalism would seek refuge in these lacunae, representing them as evidence of the activities of a "pure vital force". The last shelter of vitalism was the epigenetic process, a hideaway that was eventually eliminated when Watson and Crick discovered DNA and the mechanisms of biological reproduction. After this, biology was opened up as a field for a purely naturalistic approach. Similarly, Latour and Woolgar argue that the social approach to science must close all gaps on social explanation – in particular such that refer to "intimate thought processes" of a rational nature that supposedly resist social reconstruction (op. cit., p. 168).

This resonates with a point made in the discussion of the Strong Programme in Chapter 3. The suggested strategy calls not just for an explanatory approach,

F. Collin, *Science Studies as Naturalized Philosophy*, Synthese Library 348,
DOI 10.1007/978-90-481-9741-5_6,

but for one using a particularly strong notion of explanation. We need *exclusive* explanations, that is, explanations of a kind such that, if a complete account is delivered within its framework, there is no room for other, supplementary or orthogonal explanations. We examined in Chapter 3 how this requirement is satisfied by the Hempelian model of explanation and how Strong Programmers are implicitly committed to this model.

2. The theoretical framework that delivers the overall explanatory apparatus in *Laboratory Life* derives from economics – more precisely, a kind of updated "political economy" of clearly Marxist provenance (op. cit., Chapter 5). The scientific process is construed as an analogue to, or even an aspect of, the economic cycle of capitalist production. Indeed, the scientific cycle has a purely economic aspect, in the narrow sense. Scientific activities may bring rich economic rewards to those who excel in them. But the basic commodity that is traded is not money, but *credibility*. The purpose of all exchanges is the strengthening of credibility. Underneath is a Marxist analysis of capitalist economy as one the basic principle of which is the steady accumulation of capital. And apparently, science, like the material economy, is propelled by the principle of growth. As the authors put it, "Consequently, there is no ultimate goal to scientific investment other than the continual redeployment of accumulated resources. It is in this sense that we liken scientists' credibility to a cycle of capital investment" (op. cit., p. 198).

This is a macroscopic approach, in the sense that it does not deal in individual motivations. We might call it a structuralist approach. Just as Marxism does not trace the basic feature of the capitalism system back to the personal greed of the capital owner, or any other peculiarities of the "capitalist mind" (such as the "entrepreneurial urge" of naive organizational psychology, or David McClellands "n factor"), it is a crucial element in Latour's conception that the basic features of science do not reflect qualities of the "scientific mentality" or similar individual traits of mind (see further below).

We may note another aspect in which Latour and Woolgar abide by the Edinburgh programme: They devote the final section of their text to a discussion of the ability of their presentation to pass what is in effect the reflexivity condition (op. cit., p. 252 f). Their conclusion, not surprisingly, is that it does – although they concede that their account of science may justly be called a "fiction". This term alludes to what many critics have considered the main weakness in Latour and Woolgar's approach, that is, their commitment to an ontological constructivism; we shall return later to this topic.

Yet already in this early work, Latour and Woolgar take the first step on a path that would eventually take them far away from Edinburgh orthodoxy. They choose to descend from the macroscopic level, at which the Strong Programme operates, to the level of societal micro-explanations. This is necessary, according to the authors, in order to come to grips with the small-scale processes through which science is actually produced. Latour and Woolgar describe their approach as an "anthropology of science" (op. cit., p. 27 f). Their method is the celebrated one of "participant observation", with Latour entering the Salk laboratories at La Jolla, California, to observe what goes on in a biochemistry laboratory.[40]

They will not "go native", however; that is, they will not employ the terms with which scientists themselves describe their own enterprise. They adopt an "irreverent" approach to science, refusing to accept its authority in the same way as they would refuse to "bow before the knowledge of a primitive sorcerer" (op. cit., p. 29). Scientists' discourse about science is regarded as a design to veil the true character of the scientific process (i.e., its status as a human artifact), just as magical practices are designed to conceal that nothing out of the ordinary really takes place, and that only human dexterity is in play. Magical practices are meant to make it appear as if spirits are at work, without human intervention; similarly, science is conducted in such a way that it appears there is an independent nature that reveals itself directly to us, without intermediaries.

Thus Latour and Woolgar invite us on an anthropological journey to visit the strange tribe of neurochemists, to witness one of their most sacred practices: the creation of Scientific Facts. What emerges as the first result of their anthropological endeavours is the surprising finding that laboratory science is basically about the production and circulation of *texts*. This activity is a tool for the creation and maintaining of credibility that, according to the initial macro-sociological picture, is the essence of science. Thus, what goes on in the laboratory is the production of texts with the aid of various "inscription devices", pieces of apparatus that can transform matter into written documents (op. cit., p. 51). Examples from the study in question are mass spectrophotometers, radioimmunoassays and high-pressure liquid chromatographs, among others. These are pieces of measuring apparatus that immediately produce graphs or numbers. This output is later transformed into texts, which are then published as contributions to the "credibility cycle".

3. The production of texts, however, is only the immediate goal of the scientific process, although texts form the hub around which workaday activities in the laboratory revolve. The ultimate goal is the construction of *facts*. Now, the term "fact", more precisely "physical fact", has a dual reference. It points in part to an aspect of the material world and in part to a phenomenon in the social world – that is, an agreement about, or common recognition of, the former, physical item. Latour makes it clear that his constructivism extends to both aspects; he is not only concerned to examine the process through which a scientific consensus concerning some aspect of reality comes about. To him, the very objects of science – traditionally referred to as "theoretical entities" – are the outcome of the scientific process, not something existing independently.

He puts the point as follows:

> Despite the fact that our scientists held the belief that the inscriptions could be representations or indicators of some entity with an independent existence "out there", we have argued that such entities were constituted solely through the use of these inscriptions. It is not simply that differences between curves indicate the presence of a substance; rather the substance is identical with perceived differences between curves. In order to stress this point, we have eschewed the use of expressions such as "the substance was discovered by

> using a bioassay", or "the object was found as a result of identifying differences between two peaks". To employ such expressions would be to convey the misleading impression that the presence of certain objects was a pregiven and that such objects merely awaited the timely revelation of their existence by scientists. By contrast we do not conceive of scientists using various strategies as pulling back the curtain on pregiven, but hitherto concealed, truths. Rather, objects (in this case, substances) are constituted through the artful creativity of scientists.
>
> (Latour and Woolgar 1979/1986, pp. 128–129)

Latour goes into considerable detail with respect to the semiotic mechanism that creates this "truth effect". It is a kind of splitting-up:

> Once the statement begins to stabilize, however, an important change takes place. *The statement becomes a split entity*. On the one hand, it is a string of words which represent a statement about an object. On the other hand, it corresponds to an object in itself which takes on a life of its own. It is as if the original statement had projected a virtual image of itself which exists outside the statement.
>
> (Ibid., 176)

The above quote introduces a crucial element in Latour's system: that of the stabilization of a statement. Stabilization is, in the first instance, a semiotic process: It is a matter of a sentence undergoing a transformation or purification, in which certain components, referred to by Latour as "modalities", are gradually deleted. "Modalities" are the italicized expressions in sentences such as "*XX claims* that x is f", "*It is generally supposed* that x is f", "*There is evidence* that x is f", and so on. In other words, they are sentential components in which certain reservations or misgivings are suggested, or which diminish the force of a sentence by reminding us of the setting in which the sentence was first produced. After having been cleaned up, what is left is the pure unmodalized, contextless sentence, "x is f". It is this sentence that is, as it were, projected onto the world to generate an "object" out there with the property f.

Thus, we see that where Bloor's inspiration was clearly from social science, Latour draws upon ideas from the humanities as well. Latour stands in the tradition of French semiotics, with guiding lights such as Lyotard and Barthes. Thus, Latour refers approvingly to Barthes's theory of the "Truth effect" generated by texts (op. cit., p. 150).

However, while the "truth effect" and the dropping of "modalities" are in themselves semiotic processes, the dynamics of the overall stabilization process is not textual. It is fundamentally a micro-social process of negotiation, in which the author of a sentence (or of some larger text which that sentence condenses) tries to attach that text to other texts by means of citation, references and so on. This is the process that Latour describes as the "stabilization" of the sentence.

Like other students of science, Latour and Woolgar stress the contingent and a-rational nature of this negotiation process (op. cit., p. 152). There is no need to refer to rational thought-processes to explain the outcome of the proceedings; indeed, it is not necessary to refer to thought processes at all – for the very good reason that, strictly speaking, such processes do not occur at all but are just fictions generated by a particular way of reporting scientific research (op. cit., p. 171). They are the effects of a literary genre, the principles of which serve obvious strategic ends on the part of the scientific profession. The anthropological observer of science must look through such fictions to identify the real micro-sociological processes at work, which eventually produce a consensus.

Thus, in the end, our "anthropological observer" is able to conclude that laboratory activity, and hence the institution of science to which that activity is central, is the "organisation of persuasion through literary inscription" (Latour and Woolgar 1979/1986, p. 88). In the same process, physical reality is somehow created. What is characteristic of the scientific process, from an anthropological perspective, is the way that it hides its own nature from those engaged in it and derives its force precisely from this concealment. For science to be effective, it must appear as if its products are *not* the result of persuasion and as if the objects dealt with are *not* generated by this very process, but were "out there" all along.

With the adoption of ethnographic methods, Latour and Woolgar take a step away from standard sociological approaches based upon such traditional macro-categories as "power" and "interest". Still, the authors insist that their approach is a social one; however, they use the term "social" in a somewhat unorthodox sense, in which it refers to the "processual" aspect of science, as they put it (op. cit., p. 32). Their approach might fairly be called "micro-sociological", since it deals with the dynamics of small groups and the role that negotiations play there.

4. Let us for a moment step back from Latour and Woolgar's presentation to enter a few critical remarks. It is easily detectable that, as presented in the text, the contingency and irrationality of the scientific process are largely products of the outdated epistemology that serves to frame it. Latour and Woolgar are at pains to show that actual science does not proceed by "deduction" from data; but then, nobody thought that it did – at least not since the days of Descartes and Hobbes. Perhaps one could detect rational features in it, on the basis of a more up-to-date epistemology of science. As a matter of fact, even the authors' quite rudimentary anecdotes might seem to invite analysis in terms of a Popperian methodology: science is a matter of making daring and risky conjectures and then leaving them to friendly collegial criticism that will eliminate the invalid ones. Latour and Woolgar's glimpses into laboratory life illustrate the glee with which scientists mete out this kind of treatment to each other. Latour would later describe science as a matter of constant "trials of weakness"; this is easily translated into the Popperian conception of science as a constant probing for the weaknesses of daring proposals.[41]

This is not to suggest that laboratory discussions are smoothly reconstruable as strictly rational, according to some universally accepted standard of scientific objectivity. The demolition work of a whole generation of sceptical philosophers of science such as Kuhn, Feyerabend and Laudan would make that claim highly precarious. The point is merely that Latour's text adds nothing to this lesson; the picture of contingency is largely an artifact of the presentation. (But then of course Latour and Woolgar concede as we have seen that their own text is nothing but a "fiction".)

Latour's account met with indignant objections not only from philosophers, but even from the Edinburgh orthodoxy. Strong Programmers, who (as we recall) only embrace *epistemological* constructivism, took strong exception to Latour's ontological version (Bloor 1999a; Collins and Yearley 1992a, b). This was with good reason, since, as I shall try to show in a later section, Latour's ontological constructivism, which is of a very complex and extremely eccentric nature, does indeed face insurmountable obstacles.

However, what was overlooked, in particular by Latour's Edinburgh critics, was how small a step beyond Strong Programme orthodoxy Latour's position actually represents, especially when shorn of some of its eccentricities, terminological and otherwise. As I have stressed in the previous chapters, STS'ers typically embrace a strongly instrumentalist conception of science and of the theoretical terms it employs. From such a perspective, all theoretical entities are really "abstracta", rather than "illata" (Reichenbach 1938) or, if you prefer Russell's terminology, they are "logical constructions", rather than things inferred. They all have the same status as, say, the Equator, or the centre of gravity of the earth and not the status of Austin's celebrated "middle-sized items of dry goods". Most of us would agree that, at least in a certain sense, the Equator did not exist before people arrived on the scene. What we would object to is merely the idea that all scientific terms obey the same logic. Still, we could be swayed in this direction by noting that scientific concepts are *models*; they are not meant to represent reality in all details, but are figurative representations that allow us to grasp certain privileged aspects. We all know that the standard atomic model, which represents atoms rather like miniature solar systems, is not a one-to-one depiction of reality.

True, few philosophers of science would concede that no parameter of the theoretical models in science represents nature. Yet, as we have seen, this happens to be the position of the Edinburgh School; science is purely analogical or metaphorical. This instrumentalist attitude is embraced by Latour: Indeed, one of Latour's major claims is that we should consider science a technique for the handling of nature. As such, it is perfectly continuous with technology; indeed, Latour insists on referring to the two by one label: "Technoscience".

Thus the Edinburgh Schoolers have a very narrow base from which to launch their attack upon Latour. To a neutral bystander, it seems as if Latour merely explicitates and highlights something inherent in the Edinburgh position. It sounds hollow when Bloor inveighs against Latour, insisting on the objective existence of the material world (Bloor 1999a), since he would have to concede that the entities with which that world is inhabited – electrons, quarks and so on – are just instrumentalist posits, mere metaphorical projections of the terms used to describe the everyday world upon this inscrutable realm, enabling us to cope with physical reality, intellectually and materially.

This is not to say that Latour's position is unassailable, of course. On the contrary, it is highly precarious from a philosophical point of view. I shall subject it to critical scrutiny in the next chapter.

5. Latour would soon open up the rift between himself and his Edinburgh antecedents even wider. In the second edition of *Laboratory Life*, the term "social" was dropped from the book's subtitle, which henceforth talked only about the

construction of scientific facts, not their *social* construction. In the postscript of the revised edition, the authors represent this as a mere corollary of the tenet that the determinants of science are exclusively social. Since the terms "social/non-social" no longer mark a contrast between two actual classes of the determinants of science, the distinction has become otiose and the term "social" might just as well be dropped. The authors even go to the length of retroactively reading this shift back into the first edition, as they declare that their continued use of the term "social" in that book was merely ironic (op. cit., p. 281).

It was soon to become clear that the divergence was indeed more substantial, however. A fuller revelation of the considerations behind it was given in Latour's next major work, *Science in Action*, from 1987. Scientific fact is still seen as a construction, but is now presented as the offspring of a new type of entity, referred to as "actors" or "actants" (op. cit., p. 84). Actants do no perform their fact-creating work singly, however, but only as part of *actant networks*.[42]

This move is more than just an adjustment to the ethnographic turn taken by Latour, which would make it natural to point to human actors, rather than abstract societal forces, as the producers of scientific fact. The term "actants" is meant to cover laboratory equipment, scientific articles, research grants, university degrees and so on, along with persons; indeed, it is meant to indicate that, for purposes of the understanding of science, all these are on a par. There is no need to treat them differently. In other words, the notion of actants is meant to undermine the very distinction between human beings and non-humans as factors in the genesis of scientific facts.

This conforms to one of the main claims of *Science in Action*. When we try to account for the establishment of scientific fact, there is no gain in understanding if we describe the factors in differential conceptual schemes and look for different kinds of contribution: If, for instance, we contrast the logical or rational strength of argument with the coercive power of legislation, the economic power of big grant money, the organizational power of a well-run laboratory, or the material strength of a well-designed piece of apparatus as inputs to the scientific process. In particular, we should beware of the bifurcation between natural and social factors. This is captured in the fifth Rule of Method (out of seven in all put forth in the work):

> We have to be as *undecided* as the various actors we follow as to what technoscience is made of; every time an inside/outside divide is built, we should study the two sides simultanously and make the list, no matter how long and heterogeneous, of those who do the work.
>
> (Op. cit., p. 258)

What counts is alone the extension of the network and the way the actants may be mobilized.

Latour's adoption of the vocabulary of actants is inspired by literary theory, more precisely by Algirdas Greimas's structuralist semantics (Greimas 1983); however, Latour adopts a very liberal attitude to Greimas's doctrines and terminology. Greimas's theory, in its turn, is inspired by the studies of Russian folk tales by the Russian scholar Vlamidir Propp. In folk tales, Russian or otherwise, human beings, animals and things interact with each other without discrimination; human beings converse with animals, with the tools in the workshop, with the utensils in the kitchen and with the objects they encounter in the forests and fields. All are actors on an equal footing, but Latour prefers the technical term coined by Greimas: *actants*. Actants, according to Greimas, fall into certain characteristic types (which one might also term *roles*), which reappear in all (Russian) folk-tales. Among these, for example, are the hero, the villain, the helper and the princess. It is a characteristic feature of folk-tales that the hero prevails in his endeavours because he treats the (non-human) actants kindly, thus making them his allies and helpers. The villain, on the other hand, mistreats the other actants, inciting them to turn against him. This idea of success gained through an alliance between very heterogenous elements serves as a model for Latour's interpretation of the scientific process, as we shall see.

Another novelty is that "the social" is reintroduced again after having been dismissed, in the Postscript to the second edition of *Laboratory Life*, as being no longer of interest. Latour had come to appreciate that the notion of the "social" was still useful (and indeed indispensable), not as a basic explanatory category, however, but as a derived category complementing that of nature. In the creation of science, we are dealing with a double genesis. Scientific facts are created, and thereby the very reality with which science deals. But at the same time, and by the same process, *social* reality is generated: scientific facts only achieve this status when they are supported by societal structures (by firm "social facts").

6. Latour's suggestion was received with incredulity in most quarters, both among traditional philosophers of science and among fellow STS'ers. Does Latour really believe that Pasteur negotiated with his microbes? And does he (and Michel Callon) seriously claim that fishermen in Brittany converse with their clams?

There is no doubt that Latour actually adopts a radical ontological position according to which all actants are basically of the very same nature. (Notice that, from the very start, Latour stated that positions within science studies derive from ontological positions, cf. *Laboratory Life*, p. 280.) We shall look further into it in the next chapter. Yet there is another, purely instrumentalistic, reading of Latour's claim available and it is quite ironic (and indicative of the Strong Programmers' impatience with their former ally) that they should overlook this innocuous interpretation. Still, an instrumentalist reading should come naturally to STS'ers, who insist that the relationship between language and reality is purely nominalistic, and that theories, even at the best of times, are mere instruments for the exploration and handling of reality. When reading Latour, they apparently forget the instrumentalist standpoint shared by both parties, insisting instead on an essentialist reading of his statements. They also apparently forget the latitude of the term "sameness" that they normally extol: Latour is simply making the point that (for certain purposes, at least) such items as clams, texts, and measuring instruments are *the same* as human beings. One would have expected this claim to meet with some sympathy on the part of Bloor and Collins.[43]

The instrumentalist reading would amount to the following: In our attempts to grasp the development of natural science in theoretical terms, we may disregard the difference between human beings and material objects as elements of scientific practice. This holds generally, but is true in the case of social studies of science in particular. If we want to understand crucial episodes in the history of science, invoking this distinction turns out to be a mere distraction. What is important in order to understand the trajectory of science is the way the combatants in scientific controversies had varying luck in marshalling allies in support of their cause. What makes scientific efforts succeed is the number of allies gathered behind it. These allies are not necessarily human.

Latour's approach has an attractive theoretical simplicity to it and, in addition, suggests a very well-defined and stringent methodology: it is all a question of counting the nodes in the actant network and of registering how well they are organized. In this way, he largely sidesteps criticisms such as Collins's, which points to the difficulty of measuring the strength of nodes. Latour's answer is that we do not have to; we merely count the number of nodes.

What Latour next needs to tell us, of course, is how we count actants. Here, philosophers of science might note, with some glee, that Latour has apparently come up against the same obstacle as those philosophers of science who attempted to measure the rational merit of theories by the number of facts explained, or the number of puzzles solved. The problem is that there is no objective metric for these things.[44] But to argue thus would be to overlook that the challenge faced by Latour is rather less severe. The philosophers' metric would have to accomplish two different things at the same time: to make it possible to *explain* why one theory prevailed over its rival and to show that this outcome represented the *rational* choice in the situation. Both must be combined, if we are to grant that the actual course of science is determined by rational considerations. Latour's method of counting, on the other hand, is only supposed to do the first job. Latour explicitly disavows rationalistic commitments; hence, he may justly insist that he is not obliged to deliver an a priori answer with respect to how counting is done. The point of naturalizing the philosophy of science is precisely that such problems can be left for empirical investigation to decide.

Unfortunately for Latour, he runs into other problems. The task of counting actants turns out to be crucially dependent upon resolution of a particular problem, of a clearly philosophical nature, inherent in his system: According to Latour, certain actants only come to exist as a result of the activities of a well-established network. The conclusion seems inevitable that those actants could not have contributed to establishing the network in the first place – although this is a corollary that Latour apparently misses. This raises a serious methodological problem concerning which actants to count, and which to count out, when trying to explain the stabilization of a network. Before we can address this problem, however, we must look at a further front that was opened by Latour in his battle with the Strong Programmers.

7. As Latour sees it, the problem with the Edinburgh approach is not merely that it endeavours to explain science by invoking "society" when that entity is really an *explanandum* rather than an *explanans*. Nor is the problem that it draws a strong distinction between human and non-human actants, such as scientific apparatus. The

gravest problem is that Strong Programmers try to get by *without invoking nature as a factor* at all in the generation of scientific results.

Latour takes as his point of departure a criticism of the Strong Programme's explanatory ambitions, which is very similar to the one I have presented in previous chapters. It is expressed with more elegance, if perhaps with less technical precision, in passages like the following, where the topic is Pasteur's discoveries in the microbial world:

> Conservatism, catholicism, love of law and order, fidelity to the Empress, brashness, passion – those are approximately all we get of the 'social factors' acting on Pasteur. But they are not much if we put on the other side all the scientific work to be explained. . .the explanation has to be at least as rich as the content, not poorer.
>
> (Latour 1988a, pp. 257–258)

In other words, there is no point in Science Studies pretending that they can explain the progress of science solely in social terms – even if the "social" is understood very comprehensively to cover all sorts of negotiations and similar micro-level interactions: they perforce have to make reference to aspects of *nature* as well. That is, we have to include nature among the actants, the alliance between which produces scientific facts. (See also Latour 2005, p. 87 ff.)

Latour has a keen eye for the vacillations and evasions in the Edinburgh argument with respect to the precise role that nature is thought to play. He is aware that the official line grants that not all determinants of science are social, but is equally aware of the forces at play within the Edinburgh position that still constantly threaten to push out all non-social factors.[45] In Chapter 4, we identified the main culprit in this plot. This is the invocation of Wittgenstein's rule-following considerations, which render reality impotent to influence the scientific process since the outcome of observations and experiments may always be made consistent with any theory by judicious use of bent predicates.

It might seem that in raising this criticism and letting Nature back in again as a factor in explaining the development of science, Latour makes a retrograde step back to Mertonian sociology of science. Mertonians would agree that the outcome of scientific research is produced by the interaction between a natural and a social sphere. The former produces observations that form the raw material and input to the scientific process. When science functions properly (i.e., according to the CUDOS norms) the research process will generate theories that are in conformity with physical reality. The main social factor securing this is "organized scepticism", making up the OS in Merton's acronym, which is the critical stance that eventually will eliminate mistakes and in the long run force theorizing towards ever more faithful depiction of reality. Thus, if nature has a given feature F, and if science functions properly, it will eventually capture this feature of reality and produce theories in which F is included. An explanation to this effect would be utterly trivial from a sociological point of view, of course, which is why Mertonians choose to declare valid science outside the scope of the sociology of science altogether. Instead, sociology should examine, inter alia, cases where science malfunctions. The fact remains that the interaction of nature and society is precisely the picture that Mertonian sociology of science would deliver, were it to include such things in its compass.

In fact, the charge that Latour was regressing to an obsolete stage of science studies was brought against him by defenders of the Strong Programme: it was raised, for instance, by Harry Collins and Stephen Yearley in the famous "Chicken" debate (Collins and Yearley 1992a, b). Latour's answer was that, although nature has been readmitted as a factor in the production of science, the picture of science that results is not the trivial one that would be delivered by Mertonian sociology of science. According to actor-network theory, the answer to why any particular item is accepted as an element in a scientific theory is that the item in question formed a part of a network of actants stronger than competing networks.

8. The stage is now set for presenting the problem we announced a little earlier: When it comes to measuring the strength of networks, all that counts is the number of actants involved, Latour tells us. But do we count the actants that only came into existence as a result of the stabilization of the network? Or do we just count those nodes that existed prior to its stabilization? And how do we distinguish them?

The challenge that this question poses for Latour should be obvious. How can he reconcile the decision to allow in nature as an explanatory factor (under the name of "actants") with the claim that nature is in itself a construction? How can certain items, such as microbes, serve as allies stabilizing a network and still only come into existence once the network stabilises? How can anything be *explanans* and *explanandum* at the same time?

There is no denying that Latour makes such an allegation; numerous clear passages in his works testify to the view that parts of the network, too, are constructions. For instance, Pasteur's microbes are clearly constructions. At the same time, they are also described as elements of the network that, thanks to its large extension, helped Pasteur defeat Pouchet in the famous demonstrations before the French Academy (cf. Latour 1989).

This looks very much like a paradox. As constructions, the microbes only exist once their existence is posited by a functioning and nonsurpassed network of actants. That network has to exist before, and as a condition for the existence of, the constructions that are generated within it. But this cannot be so if at least some of the elements of that network are themselves constructions.

Notice that this is not an objection to the very idea that the theoretical entities of science are constructions (a claim that we still have not examined thoroughly); it is an objection to combining such as approach with Latour's appeal to actant networks *encompassing such constructed items* to account for the production of scientific fact. There is a vicious circle here. It is somewhat surprising that Latour should get into this quandary, since he is clearly aware, in general, of the impossibility of giving an explanatory role to natural objects in science studies, once they are viewed as constructions. Indeed, this recognition is expressed in the third of the seven rules of method put forward in *Science in Action*:

> For these parts of science [i.e., the "unsettled parts of technoscience", FC] our third rule of method will read: since the settlement of a controversy is *the cause* of Nature's

> representation not the consequence, *we can never use the outcome – Nature – to explain how and why a controversy has been settled.*
>
> (Latour 1987, p. 99)

"Nature" is our collective term for one of the two ontological regions emerging as a result of the constructive activities in the actor network (Society being the other); as such, it cannot itself contribute to and explain that construction. But Latour apparently fails to see that the contradiction reappears at the level of actants, if we hold that (some) actants are themselves constructed and yet still count among those who add strength to the network. *Given that* one endorses an ontological constructivist position, one cannot use the *constructa* to explain the occurrence of the networking processes through which those constructed entities are established; this would incur a vicious circle, where objects and cognitive states reciprocally generate each other.

9. The reason for this slip may be a conflation on Latour's part of several senses in which a thing may be a "construction", some of which are easily compatible with their playing a role in strengthening networks, while others are not. The problem is most likely caused by Latour's freewheeling use of another metaphorical term, "hybrid", which is partly interchangeable with the term "construction" (or "construct"), but still has a somewhat broader scope. Let us look at some of these senses.

A. Certain natural objects are at the same time constructions because they are (at least in part) the result of human handiwork. Latour refers to them as "quasi-objects" or "hybrids". He mentions the following examples, among others: Whales with radio collars, a special flora which exists only on certain particular slag-heaps in northern Belgium, embryos kept in vitro, and the ozone hole (Latour 1993, p. 1).
B. The next category comprises physical items as they emerge under such artificially constructed laboratory conditions as belong in category A. It is a key contention in Latour's work that laboratories, which constitute a crucial site in science studies, are not only in themselves hybrids in sense A (as they are built by human handiwork out out natural materials), but that they also generate a domesticated nature that is itself a hybrid (a construction). The facts that are generated in the laboratory extend as far, but only as far, as the laboratories.
According to Latour, traditional philosophy commits the crucial mistake of presuming that results generated in the laboratory somehow extend beyond it. They do so no more than railway service extends further than the railway tracks.
C. Certain other things straddle the divide between nature and society, not because they result (directly) from human handiwork, but because they result from theoretical extrapolation and efforts at explanation on the basis of such activities; they are the abstract parts of such human practices. This would apply, for example, to theoretical entities postulated within the framework of a particular scientific practice, such as "microbes", "quarks" or "electrons".

Latour makes much of the point that an object (of categories B and C) may shift back and forth from one domain in the constructed world (Nature) to the other (Society), cf. Latour 1992. All objects (of these categories) begin in the network, emerging from a process of theoretical discussion among scientists. Some objects never move beyond this stage; they are the topic of scientific debate, but eventually are rejected by the scientific community. To this group belong such items as the ether, phlogiston, the planet Vulcan and N-rays, which are then afterwards said to be "purely social", reflecting merely the inevitable mistakes in science as an actual social process. Other objects rise out of the network to become working elements of the natural world, such as electrons, fields of force or black holes. Still other objects shift back and forth between these two categories. (This applies to action at a distance between earth (and its inhabitants) and celestial bodies, or the atomic theory of matter.) To express this dynamical situation, Latour replaces the simple dichotomy between Nature and Society with a two-dimensional figure, where one axis represents an entity's placement on the Nature/Society dimension and the other is a temporal axis that records how the former placement varies as a function of time (op. cit., p. 285).

Notice that this vacillation does not apply to the first category of "hybrids" listed above. Everybody would accept that whales with radio collars are real, despite their resulting (in part) from human intervention. Their existence is not that of a theoretical posit that might be given up if we lost faith in the theory sustaining it. More interestingly, the same point applies to pieces of test apparatus, such as gravitational wave detectors, whose existence as "hybrids" would of course have to be granted, even if we later questioned their status as reliable detectors of further aspects of reality.

With these distinctions in place, we may now venture the following diagnosis of Latour's troubles with explanation and construction. Certain elements in the network – certain actants – aid in constituting and strengthening the network, although they are themselves constructions: they are *hybrids* of type A or type B, above. Central cases would be pieces of experimental apparatus, or samples of substances or high energy levels that only emerge under laboratory conditions. Other hybrids are only virtual members of the network, as they are theoretical posits that only come into existence when the network is established and did not serve to strengthen it beforehand (type C). These are items like quarks and other "theoretical posits". Latour's failure to distinguish between these various types of "hybrids" invites the kind of confusion through which hybrids of the latter kind are allowed to contribute to the strength of those very networks through which they themselves emerge.

An objection could be that this diagnosis overlooks Latour's doctrine of the "Janus-faced" nature of science. There are two perspectives on science: one represents science as a process ("science in action"), the other represents it as a finished product (accepted theories). Within the former perspective, such actants as microbes, neurotransmitters or quarks may not be invoked. Still they genuinely exist, albeit as constructed items, and hence occupy a place in the latter perspective (Latour 1987, p. 97 ff). But the problem is that, in Latour's account of the "stabilization" of networks, we are required to adopt both perspectives at the same time.

In numerous concrete examples, such as that involving Pasteur and the problem of spontaneous generation, the point is made time and again that Pasteur recruited the microbes as allies and members of his supporting network, and that they contributed to its victory. Yet the microbes only came into being when that network had proven stronger than its rivals.

However, as we shall see later, Latour may not have expended all his resources for solving this problem. There may be means for its resolution at the deeper level of Latour's "fundamental ontology". In order to see more clearly where Latour stands on these issues, we must leave Latour the anthropologist and turn to Latour the philosopher. This happens in the next chapter.

10. In our overview of Latour's position, considered as an empirical programme within Science Studies, we recorded two major deviations from the Strong Programme. First, Latour reintroduces the material world as a factor in the process that generates sciences of nature, although only in the form of "actants". This might be described as an *intended* heresy against Edinburgh orthodoxy rather than a genuine one, since after all the Strong Programmers did officially grant a role to physical reality in the generation of scientific results (although their philosophical arguments undermined that tenet); yet there is little doubt that this role is enlarged in Latour's picture. Second, Latour adopts a constructivist stance with respect to ontology, seeing reality itself and not only our knowledge about it as a construct emerging from the activity of actants.

The first of these modifications was meant to repair a methodological flaw that caused considerable trouble for the Strong Programme, according to our assessment. Is Latour's move an effective counter to the problems? Does it help overcome the difficulties with respect to explanation that the Strong Programme faced, as examined in previous chapters?

At first glance, it might seem that Latour's theory neatly closes the gap that we pointed out in Chapter 3, and which was also criticized by Latour, as discussed above. Once nature is included among the factors explaining scientific results, the gap disappears. Put in more technical terms: Once the existence of the theoretical items with which science deals figures among the premises of an explanatory argument, it is a trivial logical exercise to make those items reappear in the conclusion of that argument, that is, in the *explanandum*. An example of how this could be achieved would be the quasi-Mertonian explanation of a veridical scientific result suggested a few pages ago.

Unfortunately, Latour's ontological constructivism de facto nullifies the benefits of his more liberal methodology. Since theoretical entities do not exist before there are strong networks supporting them, according to Latour, the task of accounting for the workings of science must perforce get by without invoking those *particular* actants that are posited by the theories under examination. To put it in technical terms once more: If reference to such theoretical entities is to occur in the conclusion of an explanatory argument, the terms by which we refer to them must derive from premises that describe the *other* actants involved in the scientific investigation in question. This brings us up against the same problem that bedevilled the Strong Programme: such a derivation is only possible if those terms somehow already appear in the vocabulary in which we describe those actants. And this, again, presupposes that the actant description already contains the relevant fragments of physics as one of its parts.

This criticism might appear misplaced because very few areas of science permit the deployment of Hempelian explanation, which means that an approach, especially within the social and human sciences, should not be evaluated according to its success on this score, but according to such standard virtues as fruitfulness, simplicity, and so on. And it is indeed noteworthy that Latour talks about the assessment of his own approach in the same terms.

This objection would launch us upon an argument that, in fact, we have already been through in Chapter 3 with Bloor as the protagonist. Thus we need only repeat its main steps: Latour's objective, like that of the Edinburgh School, is the naturalization of philosophy of science (and, in Latour's case, epistemology and metaphysics as well), that is, the attempt to replace the philosophy of science with empirical studies. The most elegant way of doing this – and indeed the only truly naturalistic way – would simply be to present a string of case studies offering airtight explanations of crucial turns in the history of science, using only categories culled from the social sciences (or the humanities), and with no appeal to notions employed by philosophy (such as "evidence" or "rationality"). This demonstration would show that the standard philosophical accounts are otiose (and presumably also ideological, self-serving and eurocentric). For, given the alternative accounts, there would be no room for the accounts provided by philosophy; there would be no space in which the standard philosophical categories could operate.

We remember that Latour (and Woolgar) adopted precisely this line of thought in *Laboratory Life* to indicate the level of ambition of their studies of science, using the expelling of "entelichies" from biology as a parallel. There are traces of the same exclusionary bent of mind in Latour's later work, where he reveals the ambition of driving psychological (cognitive) explanations out of science studies by showing that they are not needed for a full account of the scientific process. However, Latour does not seem confident that this will succeed, so he calls for a 10-year "moratorium" on psychological explanation. In other words, the police are called in to fill the gap in Latour's approach (op. cit., p. 280; also Latour 1987, p. 247).

The problem, however, is that this strategy will only work if one undertakes to deliver explanations – and indeed, not just any kind of explanation, but only those producing exhaustive, exclusive accounts. Regardless of their precise nature (i.e., regardless of whether they are causal, functional, or other), such explanations will satisfy the formal requirements of Hempelian explanation. If you have succeeded in providing an explanation of a phenomenon, in the sense of Hempel or a similar, very strict version of the notion, then you know that no other explanation of that same fact can be provided. Hempelian explanation is exclusive, both in the deductive-nomological and in the weaker, inductive-statistical version (at least as long as explanatory probabilities are kept above 50%).

By his decision to include nature among the actants that together shape science's course, Latour brings himself into a better position than the Strong Programmers to provide suitably tight explanations of that development. But this advantage is immediately nullified by Latour's insistence that this actant is in itself a construction. When we turn to the remaining actants – that is, the parts of the network remaining when we disregard the "theoretical entities" – Latour is in the very same predicament as the Strong Programmers. We will only be able to infer an *explanandum* couched in technical, scientific terms from a description of the actant network, if that description embodies, *per impossibile*, the relevant parts of natural science.

11. Perhaps Latour never wholeheartedly shared the Strong Programme's aspirations with respect to theoretical explanation in science studies, however. This stance never conformed well with his commitment, found already in his early work, to an ethnographic method. At any rate, what gradually emerges in his later work is an alternative way to bypass standard philosophical accounts of science in terms of rationality and so on – as well as rival empirical ones in terms of psychological factors. The strategy is simply to dismiss theoretical explanation altogether; thus, even the Edinburghers' attempted social explanations fall by the wayside. Instead, he favours a kind of descriptive, closely detailed study of the actual transactions, which conforms well with his ethnographic method. This has more the nature of historical case studies than of systematic explanation within a general theoretical framework.

Latour's adoption of this stance is motivated by the view that theoretical explanation inevitably reduces and falsifies the reality with which it deals, at least in social science. As Latour puts it, theoretical explanation replaces "mediators" with "intermediaries", where the former are concrete factors that leave their unique imprint on the outcome, while the latter are mere conduits of general causal forces (Latour 2005, p. 105). In more traditional terminology, we might say that in theoretical explanation, the description under which the *explanandum* appears is not the one under which it immediately presents itself and under which the agents know it, but instead a redescription, culled from a theory. Once it is so redescribed, the phenomenon may be smoothly integrated into a theoretical explanation; it is reduced to a mere token of a general type and its individuality is eliminated. In the process of theoretically explaining a phenomenon, we always deftly replace the original phenomenon with a stand-in that is designed to fit into the theory. Theoretical explanation thus always falsifies reality, or changes the topic (Latour 2005, p. 100 ff).

Like the argument of the Strong Programmers, Latour's account suffers from imprecision, largely due to the "material mode" in which it is couched (talking about causality rather than of explanation, as the "formal mode" would have it). It also confuses the point that theoretical explanation is only partial and never captures the full reality of the *explanandum*, with the entirely different point that there is normally a need for bridge principles to connect the *explanandum* with the description under which the event to be explained is known in everyday terms (cf. Chapter 3, Section 9).

In this quandary, Latour urges us to develop an account of society and of the scientific community in which no intermediaries occur, but all items are mediators.

What this means, in methodological terms, is that we must describe meticulously what happened on each particular occasion. In particular, we have to trace the network of influence, without resorting to glib explanation by invoking such multipurpose terms as “capitalism”, “Empire” or “norms”. We might say that Latour’s sociology is governed by the principle of “no action at a distance”: we have to show in detail the mechanism of the pushes and pulls of the social. This cannot be done in a general manner, and once and for all. Every scientific account deals with the individual case; there is science only of the particular (Latour 2005, p. 137).

Does this mean the abandonment of theory? In the traditional sense, yes; however, in Latour’s general metaphysical picture, theory once again assumes a role, but only as one more actant in the network. Abstract theory delivers “panoramas” that help the other actants in their networking efforts and thus aid them in the process of building a common world (ibid., p. 183 ff). In the next chapter, we shall get a closer look at this reconstrual of theory, knowledge and explanation, on the basis of Latour’s “primary philosophy”.

This is not the place to go into the details of Latour’s new approach. Whatever its merits, its implications with respect to our original problems are at least clear. If Actor Network Theory (ANT) no longer deals in explanation, and one of a suitably exacting kind, the exclusivity property of its accounts is now lost. This means that, just because ANT can provide a detailed picture of the scientific process, no one is committed to adopt the same description – or, indeed, to adopt the conceptual scheme in which it is couched. Any description highlights certain features and hides others. With the explanatory ambition gone, no particular description forces itself upon us; the question is only which features we want to highlight in the phenomenon in question. In this sense, it is indeed correct for Latour to characterize his own case studies as “fictions” (Latour and Woolgar 1979/1986, p. 252 ff).

12. If we were to ask the “old guard” of Strong Programmers whether Latour has furthered the cause of Science Studies, the verdict would be swift and harsh. To them, and to Bloor in particular, he is a traitor against the common cause. He gives up all that is dear to old Edinburgh, such as a strong notion of the social and the ambition to explain science in its terms. Bloor’s frustrations with Latour’s apostasy even led him to publish an article with the dramatic title, “Anti-Latour” (Bloor 1999a).

Still, there is considerable irony in this strong denunciation, since, in many respects, Latour simply applies to the social study of science the points that Edinburghers have been at pains to impress upon us from the start. For instance, Latour is a highly inventive user of metaphor and analogy in his account of the scientific process. We are offered such suggestive notions as the *network*, the *synopticon*, *translation*, *immovable movers*, *termite galleries* and *rhizomes*. From a more traditional point of view, there might be a suspicion that we are really never given a theory of the scientific research process, but merely a string of ever changing and endlessly multiplying metaphors. This complaint rings false, however, when it comes from Strong Programmers, to whom all of science is metaphorical, even at the best of times.

Similarly, an old-fashioned reader may feel that Latour scores some cheap points by virtue of his literary gifts which allow him to produce an uncommonly engaging prose, complete with all the classical figures of rhetoric, such as repetition, direct addresses to the reader, questions, charming little stories about the author and his past, and so on. Indeed, a cynical reader might feel that Latour's writings are all rhetoric and no substantial argument. Alas, here is another objection that cannot be raised by the Strong Programme, which prides itself on having eliminated the philosophers' mythical and self-serving distinction between logic, rationality and rhetoric once and for all.

Thus, to Strong Programmers, Latour's work represents a treacherous stab in the back delivered by a former ally, not an honest frontal attack, where the programme is strongly armoured to resist its designated enemy: mainstream philosophy of science. Latour outdoes the Strong Programme at its own game, carefully applying, to the anthropological study of science itself, the tools that Strong Programmers have found to be operative in natural science; thus, it is very difficult for Strong Programmers to get leverage against him. Yet Strong Programmers feel, with considerable justice, that Latour betrays everything that motivated Science Studies in the first place. This is very obvious in the two official and widely published debates between STS'ers and Latour, one being the "Chicken" debate, the other the exchange between Latour and Bloor (Bloor 1999a, b; Latour 1999a). The hurt and angry tone of the latter, in particular, betrays the high stakes of the debate.

Yet although the Strong Programmers may not be in a position to level this kind of criticism, their point is surely valid: with explanatory ambitions gone, what is the goal of Latour's project? What does he aim to accomplish and what are his criteria of success, or even just progress? Latour often mentions the superior "fruitfulness" of his approach, and indeed that this is the sole touchstone for the merits of his methodology (Callon and Latour 1992, p. 345). However, one of the lasting legacies of social studies of science is surely the insight that this notion can never again be used in an innocent fashion, at least not in the social sciences. From now on, we always have to ask, "Fruitful in terms of whose interests?", or, "Fruitful from whose point of view?" What is the point of view in terms of which the shift from Old Edinburgh to ANT can be seen as progress?

To answer this question, we must turn to an examination of Latour's *philosophia prima*, that is, the fundamental ontological principles that dictate the methodology of his science studies and the "empirical metaphysics" that ensues from them. It will transpire that Latour's goal is nothing less that a dissolution of the very foundations of modern philosophy.

Chapter 7
Latour's Metaphysics

1. In the previous chapter, we examined the merits of Latour's views on a purely methodological level. Such a reading is suggested by the author himself in numerous places (e.g. in Latour and Fuller 2003). However, there is no doubt that Latour also operates at a deeper, philosophical level. He holds that an ontologically neutral vocabulary is not just a methodological convenience in analyzing natural science and technology, but actually captures reality in the most fundamental way. Those two levels are inextricably intertwined in his thinking. The ultimate test of this ontology lies in its ability to provide coherent underpinnings to the results of science studies laid out above. This would include showing how to resolve the problems we have encountered earlier in Latour's position.

We may describe the two levels in an alternative manner: Latour's investigation of science represents what he sometimes calls "empirical metaphysics" (Latour 2005, p. 51). This is the attempt to discover what exists in the world, through a procedure that renounces any a priori preconceptions. So far, this might read like a definition of empirical science. But to Latour, science – at least, natural science – is itself an a priori framework of sorts, constituted by certain prefixed ontological ideals and methodological precepts, revolving around the related oppositions of mind versus matter and subjectivity versus objectivity. Instead, "empirical metaphysics" proceeds by letting the actants themselves define who they are, without any external epistemic authority imposing an essence upon them.

Still, this methodology points towards a deeper, purely philosophical level at which actants may be characterized. They are things capable of defining themselves, but which, being bereft of any intrinsic properties that could serve as the "raw material" of such definitions, can never accomplish this in isolation, but only through a process of interacting and convening with others.

We notice that Latour's attitude to philosophy is much more nuanced and complex than that of Bloor and the Edinburgh School. (As matter of fact, Latour expresses regret at Bloor's "philosopher-bashing", cf. Latour and Woolgar 1979/1986, p. 280.) Latour shares with Bloor the ambition of using Science Studies as a tool for a naturalization of the philosophy of science; the results of such studies will serve to solve certain traditional philosophical problems. As a matter of fact, however, Latour's ambitions with respect to the naturalization of philosophy are much grander than those of the Edinburgh School. Whereas the latter in effect only

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wanted to naturalize philosophy of science, Latour also wants to naturalize metaphysics, epistemology and philosophy of mind. This is to be achieved by means of an attack upon that cornerstone of classical epistemology, the subject-object dichotomy that has shaped most later Western philosophy and created notorious difficulties in epistemology. Lately, Latour's thought has even moved into the fields of political and ecological philosophy (Latour 2004).

Still, Latour is fully aware that empirical science studies cannot stand on their own. To choose the proper methodology, we have to invoke philosophical premises. According to Latour, these are metaphysical principles; right from the very beginning, he hinted that metaphysical, ontological premises are at the root of science studies (Latour and Woolgar 1979/1986, p. 280).

The deepest layer of Latour's philosophy consists of a *radical ontological monism*. Where the Edinburgh School adopted monism (symmetry) only for the explanation of science (i.e., for our cognition of nature), Latour extends it to include even the question of what exists. In his exchange with the Edinburgh School, he is propelled by an urge to push ontological monism to its limits. This means obliterating a whole series of connected dualisms – in fact, those dualisms that were listed at the beginning of Chapter 1. Prominent among them is the subject-object dualism, but also the actual-potential dualism, the real versus the ideal, the concrete versus the abstract, and the particular contra the general (the universal). All fall victim to Latour's monistic ban.

2. The subject-object dichotomy in European thought is an inheritance in particular from Descartes. This celebrated bifurcation has rendered later philosophy perennially susceptible to a scepticism that Descartes himself could overcome only through the invocation of divine providence. The empiricists inherited the bifurcation and the problems it brings with it in full force, leaving the existence of a real material world "behind" our ideas a precarious hypothesis, according to the philosophies of Locke and Hume, and, in Berkeley's philosophy, a problem to be resolved (once more) only by divine providence. There was also an ontological problem about the interaction between the two realms, which Descartes handled by the curious pseudo-solution of locating the interaction in the pineal gland.

The dualist position assumed a particularly sophisticated form in Kant. Kant tried to overcome the radical distinction between subject and object and the subsequent tendency towards scepticism by insisting that reality as we know it – the phenomenal world, in Kantian terminology – represents, as it were, an intercalation of subject and object. Features of this world, which we naively assume to be purely objective, are really projections from the subject; this is true of both causal links and of spatiality and temporality (Kant 1781/1929).

However, Kant's attempt at overcoming the problem only brought the unhealthy nature of the dichotomy into stark relief, for his "solution" inevitably pushes the original elements of the dichotomy towards a purely transcendent status. The object, untainted by any incursion of subjective elements, becomes the "Ding an sich", a purely unknowable surd. The mind, on the other hand, can now no longer be the mind we each know from our subjective experience, or see at work in the actions and utterances of our fellow men; for this mind is inherently tied to the body, which, as part of the phenomenal world, is it itself a creation of mind. It is also inherently temporal. Mind must perforce also withdraw into a transcendent realm, turning into the noumenal self that is the logical counterpart of the *Ding an sich*. To most of Kant's contemporaries (and to most philosophers of the subsequent era), this looked like a *reductio* of the position (Latour 2000, Chapter 1).

Latour is not content to recount this familiar episode in the history of Western philosophy, however. To him, the real story of dualism is played out on the larger, societal stage. It is the story, not so much about Mind versus Matter (*res cogitans* vs. *res extensa*) as about Society pitted against Nature. The theme of the plot is Modernism and its discontents, and he tells the story in a book with the intriguing title, *We Have Never Been Modern* (Latour 1993). Modernism effects a clear separation between nature and society and establishes entirely different vocabularies in which to describe them – although, significantly, the term "representation" may be used in both. The chief impetus of Modernity lies in the success of natural science and the enormously successful representations of reality – in the form of mathematically formulated theories – provided in particular in Newton's work.

Contemporaneously with the rise of science, a new political order gradually arises, where legitimation of government is not derived from divine providence, but from the people whose wills are represented in the body politic. So here we have a neat symmetry: Physical bodies are made up of atoms without wills, intentions and appetites, whereas the body politic – Hobbes's Leviathan – is made up of human atoms endowed with appetites, intentions and will, which, however, they choose to surrender to the collective will of the great Leviathan.

But this picture embodies a profound falsification, which to Latour is brilliantly dissected in Shapin and Schaffer's celebrated study, *Leviathan and the Air-Pump* (1985). Here, the authors show how the new, empirical and experimental conception of science, epitomized by Boyle and the Royal Society, emerged through largely societal processes and was propelled by political and ideological springs. Moreover, the concrete scientific results were carefully constructed in the laboratory, with the air pump and the vacuum it generated being the key tools of the enterprise. Thus, the conception of nature as independent emerged through a process that tacitly contradicted that very conception. Conversely, the conception of society as a purely normative system, a system of agreement and contract, only contingently embedded in the physical world, is belied by the fact that such a society is only viable and stable if it can weave parts of the material world into its fabric. It is dependent upon the persistent interaction with, and control of, physical nature in workshops, factories and, indeed, scientific laboratories, without which large-scale societies cannot reproduce themselves.

Here, according to Latour, we see the basic fraudulence of dualism. A dualist world order is only upheld by an interaction between nature and society, which that conception simultaneously denies. As a matter of fact, we may justly talk about two separations or divisions effected by modernism. The first is the separation between Nature and Society. This is a kind of purification, but it is not a purification effected in the world as much as in the way we talk about it. The second is the separation between the *products* of the first division – pure Nature and pure Society – and the underlying networking on the part of actants that *produced* the reality out of which these realms were abstracted in the first place. This separation takes the form of a repression of the existence of the latter. The institutionalization of this repression is precisely what we call Modernism.

In later works, Latour has traced the roots of certain elements of modernism further back into the history of ideas and drawn up even more dramatically its underlying motivation (e.g. Latour 2000, Chapter 8). That motivation is squarely a political one – that is, to suppress democracy, in its broadest sense – and its driving sentiment is a hatred of the *demos*. This stance can be traced all the way back to antiquity, and Latour finds it manifested with particular starkness in Plato's *Gorgias*. Here, Socrates and his sophist interlocutors Gorgias, Callicles and Polus are pitted against each other; and generations of readers have been taught to admire the way Socrates upholds the value of Truth against the conniving and manipulations of the sophists. But Latour points out that the antagonists share a fundamental premise, that is, a contempt for the unspeaking masses and an unwillingness to let them dispose their own affairs as they see fit. The sophists raise a particular ideal of aristocratic excellence to oppose the "mob rule" of the demos, whereas Socrates exalts an ideal of Truth, defined by the geometric method. The former attitude has proven incapable of curbing the masses and stemming the tide of democracy, but the second has proven more efficient, in particular since the coming of Modernity. As Latour sees things, the current Science Wars are the latest round of this battle. Philosophers and scientists have insisted that objectivistic conceptions of scientific truth and rationality are necessary to keep obscurantism and mob rule at bay.

In a recent work, *Politics of Nature* (Latour 2004), Latour takes this line of thought even further and in effect erects an entire political philosophy on the basis of his critique of the Modern settlement. His point of departure is the ecological movement and its endeavour to protect nature against the incursions of human society and human needs. Latour finds this agenda sympathetic, but, not surprisingly, rejects the manner in which it is built upon a sharp separation between nature and society. This weakness obscures the aim and prevents political ecology from ever reaching its goal. It is tantamount to a strict separation between science and politics, between rationality and power, and between fact and value, which leads to efforts to overcome the crisis by even more science and even more energetic politics. But the separation is part of the problem, not its solution, and more science is not going to solve it. The solution, according to Latour, is to dissolve the very separation between nature and society, replacing the two with the collective of actants. But this time around, this blending will take place at the level of political institutions. We will have to do away with a constitution that strictly separates a *political* system, where political interests are *represented*, from a *scientific* system where nature is *represented* and where the two modes of representation are allegedly of totally different kinds. Instead, we need to realize that in both cases, actants, whether human or non-human, are typically represented by spokesmen and that the processes are the same: discussion, compromising, tinkering and accommodation. Latour sketches an entire constitutional system, although described only in the most general terms, to flesh out these epistemologico-metaphysical insights.

Thus, the concerns propelling the latest transformation of Latour's philosophy are – if we were to put it in traditional terms – nothing less than the future of mankind: the conditions of human beings living together peacefully and in harmony with nature. Science Studies, which officially only aimed at examining science with science's own tools to overcome philosophical preconceptions, have indeed come a long way! But then there was all the time a tacit, more inclusive agenda of a political nature behind the overt one – as I have argued.

3. Thus, the purpose of Latour's work is not only to eliminate certain misconceptions in our way of understanding science, but even some deeply rooted maladies in our Western cast of mind. These are not merely pathologies of thought, but of our political life as well. They are caused by the way we dichotomise mind and matter, society and nature in Modernity. Overcoming Modernity and overcoming the dichotomies are one and the same thing.

To make things even more complicated, in the phase of late Modernity in which we currently live – some would even call it post-modernity – the basic dichotomy has transmuted into a trichotomy. The third element consists of *texts*, which are entities promoted to centre stage by the new science of semiotics. At the outset, semiotics was an attempt to overcome the dualist predicament, since semiotics reduces all the original oppositions to elements in a semiotic structure; they are in the end only "text effects". This attempt fails, however, which is no surprise since semiotics itself perpetuates the fateful dualist mode of thinking: it is based on a fundamental dichotomy, separating words (texts) from things. Admittedly, it would add that, on closer inspection, it turns out that the world contains only texts; but this is still a move within the original conceptual dualism and is hence as misguided, in Latour's view, as the Edinburgh School's reduction of everything to society, within the framework of an initial dualism of nature and society. Thus, inevitably, semiotics only manages to add a further entity to the furniture of the world – that is, texts – the relationship of which to other kinds of entities must now be examined. According to Latour, the solution lies once more in the elimination of binary contrasts. The very conceptual duality between things and texts must be overcome, along with that between Nature and Society. The very distinction between texts and non-texts melts away in the heat of Latour's all-encompassing push towards ontological monism (cf. Latour 1988a, pp. 166, 184–185, 2005, p. 122 ff). According to Latour, the problem of the construction of actants evaporates at the same time.

4. The move that Latour undertakes to achieve this is a reversal of the Kantian solution, but transposed to the larger arena of the social world. According to Kant, the real existents are the *Dinge an sich* and noumenal minds, respectively, which between them generate a derived, phenomenal reality; Latour, on the other hand, insists that both Nature and Society are derived from the primordial activities of something in the middle.

We have seen already in the previous chapter the picture resulting from this effort to overcome the dualism in the description of the scientific process. The fundamental notion is that of a network, also called the "collective", which is made up of actants. "Actant" is a neutral term designed to refer indiscriminately to all the factors shaping the scientific process: scientists, instruments, theoretical entities, texts, citations, institutions, grant money, professional reputations and so on. In the networks, actants are tied together in an interminable flux of ever-changing alliances. Thus, the world is best described in political terms.

Latour's network theory of science represents the naturalized aspect of his philosophy; it presents answers, springing from empirical studies of science, to venerable philosophical problems in the area of philosophy of science, epistemology and metaphysics. But, as Latour himself emphasizes, these empirical results emerge only on the basis of conceptual and methodological choices, which themselves derive from a metaphysical position and thus from philosophy (Latour and Woolgar 1979/1986, p. 280). Thus we need to get a grasp of the principles of Latour's "primary philosophy" underpinning the empirical investigation and, hence, the naturalized part of his philosophy.

The best source for the purely philosophical foundations of Latour's works is *Irreductions*, which constitutes part two of *The Pasteurization of France* (Latour 1988a). Here, we may also look for responses, at a deeper level, to some of the criticisms of Latour's empirical work, voiced above. The philosophical genre of *Irreductions* is that of metaphysics, which in itself represents a reversal of the trends of Modernity. Modern philosophy is defined by the move from metaphysics to epistemology and, later, to philosophy of language, in what has been called the "linguistic turn". With Latour's redefinition of the field, we are back to metaphysics, as the features of thought and language are derived from their ontological characteristics.

The most striking feature of Latour's ontology is the radicality of its monism. It is defined by the overcoming of all essential – and essentialist – distinctions. In certain places, distinctions are indeed drawn among actants, for example between humans and non-humans. But although this distinction is certainly genuine, according to Latour, it is not basic; for it turns out that the features that render humans special – such as intention and thought – are themselves "networking effects". They are the result of the association of actants. Indeed, all properties of actants are networking effects, aside from the very ability of actants to form networks. The actants are all of one type; whatever seems to set one particular kind essentially apart from another is itself the result of the interaction between actants, such as intentionality, or specific mental capacities (Latour 2005, p. 204 ff). Thus, Latour would reject any attempt to capture what the actants are intrinsically, as opposed to what they do: That distinction is constitutive of a substance metaphysics that Latour is precisely concerned to reject.[46]

Latour's metaphysics is radically actualist, nominalist and particularist. He rejects potentialities and possibilities (*Irreductions*, §§ 1.5.1, 1.5.1.1). To the potentialities belong logical powers, since drawing logical inferences means rendering explicit – deducing – what was already contained in the premises, implicitly and potentially. Consequently, Latour denies the reality of deduction (ibid., 2.1.2).

As there are no potentialities residing "inside" things, there is nothing "higher" than actual reality, either – no transcendent principles governing it and no meta-language in which to express such principles. Nor is there anything deeper, such as "underlying ontological structures", or other foundations (ibid., §§ 2.1.7.2, 2.1.7.3). It follows that there is no knowledge, since it would constitute a meta-level with respect to the reality known (§§ 1.1.5.3, 1.1.5.4). For the same reason, there are no theories, either (§ 2.1.7), nor any explanation of individual cases, which would have to be delivered by such theories.

Latour rejects essences or universals (§ 1.4.2), accepting only particulars and unique events: "Everything happens only once" (§ 4.4.4). This goes for meanings, too (§ 2.6.3). Latour's philosophy is thus radically nominalist.

5. What comes across so far is a radically reductionist, minimalist metaphysics. One might describe it succinctly as the reverse of Leibniz's monadology. Where Leibniz populated the world with items that have only abstract, formal and logical relations to each other, such as representation, but no actual contact, Latour's world holds items that have contact with each other but possess no abstract logical or cognitive interrelations.

Latour is adamant, however, that his theory is not reductive, as he announces already in the title of the essay (*Irreductions*). To him, all the epithets assigned to his position above would themselves reflect the kind of dichotomous thinking he is trying to overcome: the distinction between nominalism and Platonism, between concrete and abstract, between description and explanation and between object language and meta-language. A radical monism must do away with even these distinctions.

What the dissolution of these dichotomies amounts to in practice is this: Latour does, after all, allow back in all the things apparently expelled; only now they are reconstituted to fit into the ontology of actants. In this vein, Latour admits the existence of generality, of logical force, of possibility and potentiality; and he explicitly denies being a nominalist. But all these items are no longer their old selves, no longer things apart from the network, either hovering above it or serving as its foundations. They are themselves *parts* of the network, since they are themselves nothing but *actants*. There is no "action at a distance", not even the action of explanation, reference or deduction, but only exchange by contact between particulars. This explains Latour's seemingly ambiguous attitude to scientific explanation of science's developments. Explanation is indeed in order, but only as ontologically reconstrued – that is, only as one more actant that other actants can ally themselves with in the attempt to stabilize the network supporting a particular scientific development. The traditional kind of explanation – revealingly called "subsumptive explanation" – would represent an attempt to etablish a sort of hegemony, intellectual and social, over the area explained; it would subject that area to the discipline and institution that deliver the explanation (cf. Latour 1988a). We may note in passing that it was precisely this kind of hegemony that early Latour and Woolgar hoped to establish with respect to the traditional cognitive approach to science, if not its downright elimination; cf. Latour and Woolgar 1979/1986, p. 168.

The same thing applies to knowledge. When Latour declares that nobody knows anything, he means knowledge as traditionally conceived, that is, as something standing outside of a fact and "representing" it. But of course there is knowledge if we conceive it in the right way, that is as knowledge-how, a way of coping with reality. And to cope with reality means making it an ally; or more precisely, it means forming an alliance among actants and thereby constituting reality.

We may initially find it difficult to comprehend how such things as logical force, universality, knowledge and explanation might be actants. To grasp this, we should first remind ourselves that all these items are evidently part of the *discourse* going on in networks; they are part of the "semantics" developed by a particular class of actants, that is, humans. Next, we note that according to Latour's fundamental ontology, there is no essential difference between a text and other actants. For a text is not something apart from the reality to which it refers, but an element in the network through which such reference circulates. This means that there is no fundamental difference between invoking logical power in order to support one's argument, and actually making use of such power. (This does not mean, of course, that one can make one's argument logically compelling just by saying so. One needs to persuade a plurality of actants to rally behind that claim.)

Thus, in a sense, Latour's *philosophia prima* leaves everything as it is, just as does Wittgenstein's late philosophy (Wittgenstein 1953, § 124). But everything is changed at the same time: Everything is just demoted to the status of an actant in the network, in the same way that for Wittgenstein, everything is just a move in the language game.

6. The similarities between Latour's fundamental philosophy and that of the later Wittgenstein are indeed pervasive, and I shall pursue this topic at some length, as a foil for the further articulation of Latour's philosophy. The similarity is found already at the stylistic level, where Latour adopts the format of short paragraphs also used in Wittgenstein's main works. The format of Latour's text corresponds closely to that of Wittgenstein's *Tractatus*, with numbered paragraphs and sub-paragraphs according to a decimal system. However, as far as the contents go, the similarity to the *Philosophical Investigations* is far more striking. Indeed, if one were to give a one-sentence characterization of Latour's thinking, it might be that of Wittgenstein's late philosophy ontologicized and with a much higher sensitivity to the *dynamic* and *agonistic* character of human practices (which is very much underplayed in Wittgenstein's work).

Let us go back for a minute to our presentation of Wittgenstein's late philosophy in Chapter 2, deepening that account slightly. As we remember from this chapter, Wittgenstein's world, like Latour's, is "flat": there are only social practices, "language games" and "ways of life". He is at pains to get rid of the higher realms invoked in the *Tractatus*, in particular the logical world and its idealized "crystalline purity". Logic is just a projection of our human activities into a fictitious, transcendent realm and the "strength of the logical must" just the inflexibility of social practices. We have already looked at the way Wittgenstein reduces such logical terms to aspects of human practice, a move that played a significant role in Bloor and Collins.

Similarly, there are no higher epistemic levels, nor any deep epistemological foundations. In particular, Wittgenstein is concerned to be rid of philosophy as an epistemic meta-platform, from which all other human activities may be perused, judged and rejected if found wanting, or provided with foundations when such are needed. This rejection of meta-levels is extended to all other spheres; for instance, in the *Philosophical Investigations*, § 124, Wittgenstein declares that "mathematical logic" is not the name of a meta-activity relative to mathematics (often even referred to as "meta-mathematics"), but is just a part of the language game of mathematics.

Latour agrees with Wittgenstein on all these points, but whereas the latter considered the objects of his criticism to be specific *philosophical* abberations, Latour has a more permissive attitude. He considers these phenomena to be natural and ineliminable parts of human practices. Hence, we should not try to suppress them, but only put them in their right place; that is, show them for what they are – moves in a political process – and strip them of the magical trappings that account for some of their power.

The key to the difference between Wittgenstein and Latour lies in the latter's dynamic, agonistic conception of reality. Whereas Wittgenstein thought that human practices perpetuate themselves by the force of their own inertia, as it were – and certainly without the need for external guidance supplied by "logical rails" – Latour is insistent that human practices are only upheld by constant effort and activity, that is, the effort to keep allies aligned behind one.[47] In this endeavour, appeals to "rationality", "logic", "explanations" or "theories" are in order; they are not inherently philosophical phenomena.

Wittgenstein had no use for philosophical explanation or justification of social practices, since there is no way to get behind or above them. We just have to take them for granted; they are just "what we do". The activity of providing justifications and foundations was never a proper part of such practices, but a peculiarly philosophical perversion of the latter. Latour, on the other hand, is aware that there are always rival practices, and proffering a justification of one's own view may be a useful way of converting the opposition. It is not an illegitimate attempt to elevate oneself to a transcendent position, but an attempt to pursue political goals with other, subtler means. Similarly, explanation is always just a further, added aspect of the activity explained, typically a part of it meant to justify that element with respect to a particular audience; it is always part of an attempt to recruit them for the network. The same is true of appeals to "logic" or "rationality". In all these reconstruals, Latour manifests a strong sensitivity to the agonistic and political aspect of social life, a sensitivity that is absent in Wittgenstein's largely static and conservative picture.

Thus, Latour does not reject such cognitive meta-activities in themselves, nor condemn philosophy as the home of such activities. Still, something akin to the hostility with which Wittgenstein regarded philosophy is echoed in Latour when it comes to his rejection of Modernity. Modernity is the systematic obfuscation of the true nature of human practices, an attempt to control messy social reality by promulgation of a grand myth invoking supposedly universal transcendental forms like "scientific method", "truth", "rationality" and "logic". In the same way that Wittgenstein tried to bring certain key terms back to their everyday uses, Latour wants to strip these terms of their assumed transcendental status and show them for what they are: devices used for a political purpose. These items are not transcendentals, denizens of a "third world", but are only further actants belonging to the one and only world we all inhabit. What needs to be eliminated, however, is the mythical appearance that these things have assumed in Modernity.

A particularly interesting point of comparison pertains to the Wittgensteinian notion of rule-following, which played such a large part for both Bloor and Collins. Latour does not apply this term, but his attitude to the problematics it introduces may be easily gleaned from his remarks on deduction, the rationalist's favourite case of a transcendentally rule-governed activity. According to Latour, there is no such thing as extracting something from a set of premises, or from a sentence, that is already inherent in them. This means that we are not constrained by them in any way. Instead, what constrains us, according to Wittgenstein, is simply a social practice, learned through drill. Latour would agree, but he construes the practice rather as a political tug-of-war. What we call a "logical consequence" of a sentence is a modification and translation of the original sentence around which we can rally a majority of actants.

A crucial difference consists in the ontological implications drawn from this tenet. Wittgenstein, unlike Latour, is not an ontological constructivist. What are "constructed" according to him are human practices, meaning that they are determined only by factors immanent to those practices and exist in full independence of any authority superior to and transcending those same practices. There are no a priori logical rails along which human practices ride and that guide them; human practices lay the rails as they go. But the practices do not, as it were, produce the very ground on which the rails rest. This is where Wittgenstein and Latour part company. Interestingly, Latour, too, uses the rail (or rather *railroad*) metaphor to characterize human practices and, like Wittgenstein, is committed to denying the existence of pre-laid rails. He goes beyond Wittgenstein, however, in insisting that (if we may extend the metaphor a bit) the landscape that the railroad traverses is created along with the rails; the landscape and all the things comprised in it only spring into existence as the railway traffic gives them life (*Irreductions*, § 4.5.7.1). They are somehow intrinsic to the railway system. To illuminate this, Latour often uses a further metaphor: that of termite galleries. These blind insects never behold the world beyond their tunnel systems, but drag it into their systems and make them a part of it (Latour 1987, p. 232, 1988a, p. 171).

Note, incidentally, that this does not mean that reality exists only where scientific practices are adopted, according to Latour. There are other practices than scientific ones, such as religious or magical activities – or indeed our everyday non-scientific or pre-scientific practices. But reality only exists where practices exist, that is, as far as networks of actants extend.

7. This is not the place to engage in exhaustive criticism of Latour's philosophy. For that, it is too vague, too metaphorical and, above all, still too much in flux. What we witness is clearly work in progress and it would be highly inadvisable – and certainly very un-Latourian – to look for a determinate, fixed doctrine already existing underneath the somewhat tentative formulations that are currently available. It would be similarly perilous to second-guess the direction in which it will go. Either attempt would most likely be undercut by future twists and turns in Latour's development.

Let me just restrict myself to raising the question that I think forces itself upon any reader of *Irreductions*: How can actants generate the world through their interaction, if they are fundamentally propertyless, beyond their penchant for combining? In a sophisticated substance metaphysics, the dynamic and relationist nature of things is handled by including potentialities or propensities among the properties of things. If things are in constant flux and thus cannot be defined by the properties they display at any given time, we must define them by their propensities. Indeed, in a highly sophisticated substance metaphysics, all properties of things might precisely be potentialities. (An example might be the metaphysics of the *Tractatus*, where the properties of objects merely consist in their propensity to combine with each other to form *facts*.)[48] But Latour explicitly denounces potentialities; hence, he seems to relinquish any attempt at explaining why actants combine in the particular way they do. In Latour's system, reality emerges through some kind of boot-strapping process that is essentially beyond explanation.

Instead, I shall examine critically a few rather more technical points not pertaining to Latour's "primary philosophy" in itself, but to the way it is invoked to solve certain crucial problems with respect to the relationship between scientific theory and reality. The first is with respect to representation and truth, the second with the constructivist aspect.

8. Latour wants us to stop regarding scientific theories as representations of nature. Instead, they are ways the collective copes with what is outside of itself. Latour strengthens his attack upon the representational theory of knowledge by means of an attack upon the correspondence theory of truth, which he reads in a particularly strong sense, that is, as a picture or depiction theory (a representational theory).

Latour's criticism takes the form of a painstaking analysis of the field practice of a particular scientific discipline, namely *pedology*, the science of soil (Latour 1999b, Chapter 2). The site of the investigation is a small sector of rain forest in the Brazilian province of Boa Vista, in which pedologists try to establish whether the savannah encroaches upon the rain forest, or if it is the other way around. Latour offers a detailed account of the way in which (true) representations of the condition of the forest topsoil are constructed through a number of transformation devices. Main parameters of interest are the colour and texture of the soil. Samples of topsoil are compared for their colour by a standardized test and their hue is coded by number. That number, together with a score of similar numbers, is next transformed into a graph. Latour emphasizes how, at every stage, the relationship between sign and reality is not a question of likeness, but is rather that between links in a chain of transformations.

However, the theory set up by Latour for attack under the name of "correspondence theory" is quite different from what is normally understood by that term. As a matter of fact, what Latour puts forth is a theory of *reference* rather than a theory of truth, two things that most philosophers take some pains to distinguish (and indeed the text we are here examining, Chapter 2 of *Pandora's Hope*, bears the quite appropriate title, "Circulating Reference"). Few philosophers (if any) have ever held that referring terms work by means of similarity; the only examples of this in modern Western languages would be very rare cases of onomatopoieia, words that refer by means of an auditory likeness to the thing referred to (as in the term "cuckoo", which refers to the bird whose call it mimics). True, in other cultural traditions, such as the Chinese, there may be pictographic languages that function by likeness, but, for obvious reasons, these have played little role in Western thinking about the relationship between language and reality. The most prominent champion of a picture version of the correspondence theory, the Wittgenstein of the *Tractatus*, notably did not take singular terms to have any likeness to their object, although he took sentences to have an abstract *structural* similarity to the state of affairs expressed, which did indeed make them pictures of that reality (Wittgenstein 1923, §§ 2.1 – 2.17).

As a theory of reference rather than of truth, Latour's theory shows considerable affinity with a celebrated theory of naming developed by Kripke and Putnam (Kripke 1980; Putnam 1975). The key elements in naming, according to these thinkers, is, first, an initial causal engagement with a given object, amounting to a formal or informal naming ceremony. Second, starting out from this event, the name travels along a chain of communication to its later users. What Latour adds to Kripke and Putnam's story is a heightened sense of the fragility of the chain and of the efforts necessary to secure it. Broken chains of reference have indeed been discussed in the analytical literature, but typically based on simplified fictitious situations. There is no consideration of the added complexity that accrues when the "names" are numerical tags put upon soil samples, the identities of which might fall prey to such vicissitudes as decay, car accidents during transportation, or the like.

Latour backs up this criticism with reflections that may be read as a reconstruction, within the Latourian frame of thought, of a *coherentist* conception of truth – which evidently is highly germane to the coherentist construal of *justification* inherent in Latour's networking theory. Whereas on the standard coherentist conception truth is the imbeddedness of a *sentence* in a network of interconnected *sentences*, in Latour's version, of course, it is the linkage of an actant with an extensive network of other actants, be they human or non-human. A sentence establishes its connection with reality (establishes itself as *true*) not by mirroring or resembling the latter, but by means of "articulation", that is, by extending itself towards other sentences or other stable points in the network, thereby stabilizing itself as a firm node in the network. Translated back into a standard coherentist framework, the truth of a scientific sentence is established by operationalizing ("articulating") it, that is, drawing out the implications of that sentence with respect not only to experimental setups, but also to concrete practical situations to which it may apply. The richer this articulation, that is, the more extensive the set of such implications, the broader will be the evidential basis of the sentence if the tests fall out in its favour, or if it serves as an instrument for practical action.

Latour uses this analysis to express the sense in which truth is a construction. This description is appropriate, to the extent that the meaning of a sentence and, thus, its truth conditions, would not be fixed prior to such articulation. It is implausible to see the outcome of operationalizations of theoretical statements in science as somehow rigidly fixed in advance by some clearly defined logical core of the concept. It is a familiar point in the philosophy of science that there is no firm distinction between analytic and synthetic components in such terms. This was first captured in Quine's network metaphor of science, the transformations of which in the work of Hesse and Collins we have already traced. Now we find the same point repeated in Latour's ontological reconstrual of these logical or semantical points.

A standard objection to coherentism of truth, on is normal interpretation, points to the possibility of arbitrarily generating internally coherent sets of sentences, expanding them to the point where they exceed the existing set of sentences describing the world that we normally take to constitute reality. Imagine, for instance, Tolkien's works multiplied a billionfold, perhaps through the services of an army of Hollywood scriptwriters: suddenly, Mordor and Middle Earth become real.

Latour does not address this worry, but we may assume that he would dismiss it on the grounds that the nodes of the coherentist network, in his interpretation, are not texts, but actants, which are as real as anything. Thus, the networks are firmly rooted in reality from the outset. At the same time, Latour seems to accept cheerfully another consequence of coherentism that critics have focused on: the danger of *relativism,* if there is a draw between two (or more) frameworks with respect to their extension. Indeed, Latour seems to go further, being apparently ready to attribute truth even to inferior networks (and reality to the entities they posit), although a reduced truth (and reduced reality). Thus, Latour endorses not only a *relativism,* but a *gradualism* of truth (Latour 1999b, Chapter 5).

9. The second difficulty faced by Latour's metaphysics has to do with his constructivism. Latour's claim, that the "facts" generated in the laboratory extend only as far as the laboratory walls, seems simply wrong. Such facts extend further, both spatially and temporally.

Let us first look at the temporal dimension. The achievements of Louis Pasteur constitute Latour's main case study, and several of his works revolve around the question of their proper metaphysical interpretation. Did the microbes exist before Pasteur proved their presence? In an early article (Latour 1989), Latour adopts a rather lame halfway position: The microbes did not exist prior to Pasteur's "discovery", because what is referred to by the term "microbe" today, post-Pasteur, is different than its reference prior to Pasteur's arrival on the scene. Pasteur *happened to* the microbes, and they have not been the same since.

In so arguing, Latour draws upon the processual, non-substantialist ontology suggested by Whitehead (1929). We should not see the world as made up of substances, that is, enduring entities defined by a set of permanent essential properties, but at the same time carriers of a further set of transient, contingent properties. Rather, we should construe them as basically processual – as foci of constant flux and, especially, as constantly being modified through their interaction with other such foci. Thus, the tuberculosis bacillus of today is different from the bacillus that killed the pharaoh thousands of years ago, since it has been reshaped endlessly by the processes and interactions in which it has been involved since then. The most significant of those "interactions" is its discovery by Robert Koch in 1882.

In a later publication (Latour 1999b, Chapter 5), Latour introduces a much subtler position (yet apparently without abandoning the former view). Put in terms slightly different than his own, Latour suggests that any temporal assertion (temporal sentence) is doubly time-indexed, with one index indicating the date at which a certain event took place, the other indicating the date at which the assertion about it was made. Such sentences may sport temporally variable truth values. When a sentence pertains to a state of affairs of the past, it may transpire that, at the later time at which the sentence is asserted, the sentence in question has recruited additional members for its supporting network (in traditional parlance, additional evidence in favour of it may have been gathered), thus strengthening its epistemic authority and ontological import. For instance, the sentence, "Microorganisms existed in 1830_{1830}" is false: it was false in 1830 that microorganisms existed in 1830. However, the sentence, "Microorganisms existed in 1830_{1865}", is true, since in the interceding period Pasteur managed to assemble a network supporting that statement. It has remained true to the day Latour wrote his book. The sentence, "Microorganisms existed in 1830_{1999}", is true; but its truth value may possibly change sometime in the future.

This theory makes it possible to admit that Ramses II did indeed die from tuberculosis (and avoid the feeble position that he died from a "tuberculosis-like" disease); cf. Latour 2000. He did indeed do so, if we can establish, on the basis of evidence available to us today, that this hypothesis provides the best explanation of his death.

This suggestion shows a highly interesting similarity to Dummett's work on the status of past sentences within the framework of an "anti-realist" (constructivist) semantics for temporal sentences.[49] Dummett's work is relevant, since it would seem that only the general arguments in favour of a global anti-realist position provided here would suffice to support the view of the past held by Latour. Indeed, Latour could derive some additional support for his general constructivist position from a later work by Dummett.[50] It should be noted, however, that in his most recent writings on the topic, Dummett has largely retracted his constructivist conception of the past (2004). Moreover, even according to the position adumbrated in Dummett's earlier work, Latour's position is not coherent. From a constructivist point of view, lack of positive evidence for a statement at any given time does not imply the falsity of that statement, but merely that no determinate truth value may be attributed to it. Only conclusive disconfirmation allows the determinate value of "false" to be assigned to a statement. Thus, since in the nature of the case there could not have been conclusive disproof of the existence of the tuberculosis bacillus in 1830, the truth value of the sentence, "The tuberculosis bacillus exist in 1830 $_{1830}$", is not false, but indeterminate. The correct verdict at this time would not have been to declare the bacillus to be non-existent, but to declare the matter undecided and refuse to attribute a determinate truth value to the sentence. Moreover (and more importantly), Latour overlooks the "presentist" bias of the anti-realist position, which means that for any (doubly) time-indexed sentence S_{t1t2}, where t1 is the time referred to and t2 is the time of assertion, S_{t1now} takes precedence over all other differently time-indexed versions. That is, the truth value is always assigned from the perspective of the present moment, the *now* (where "now", of course, changes whenever the matter is addressed). Hence, "The tuberculosis bacillus exist in 1830_{1830}", with its indeterminate truth value, yields to, "Tuberculosis is caused by a bacillus existing in 1830_{now}", which has the truth value *true*. Any other conclusion would produce a genuine temporal relativism, which Dummett rejects on the grounds that it conflates a realist and an anti-realist (constructivist) position. It illicitly combines the anti-realist conception that truth value varies with time (but only in the sense that indeterminate may change to determinately true, or determinately false) with the realist idea that all times are equal and may be perused, as it were, from a point itself outside of time from which absolute truth values may be assigned. But the anti-realist holds that the truth value assigned at the *present* time takes precedence, which does away with relativism (cf. Dummett 1969).

Latour's attempt to explain science "in the making" (Latour 1987) according to principles appropriate to that dynamic perspective, while at the same time routinely invoking items that only exist within the framework of a "finished" science, precisely exhibits this illegitimate combination of two temporal perspectives. In explaining the making of science, Latour expressly urges us to adopt the perspective upon the world as it was prior to the "stabilization" of a theory and the ontology it brings with it. In his third methodological principle, he makes it clear that, within that context, scientific posits may not be invoked for explanatory purposes (op. cit., p. 258). This amounts to an anti-realist position with respect to a point of view prior to the stabilization of the discipline in question. Yet, in describing Pasteur's network as being stabilized by Pasteur's alliance with the microbes, he adopts a position *ex post* the stabilization of the network (e.g. Latour 1988a, p. 35 ff, 1989, p. 107). The two standpoints cannot be combined. Either Latour remains true to his methodological principle, in which case microbes cannot be invoked, or he invokes the microbes, at the cost of rejecting Principle Three and with it the very distinction between explaining science in the making and as a finished product.

In any case, Latour's temporal constructivism fails to improve his position with respect to the major problem we have pointed to several times: A member of a given network cannot simultaneously be a product of the stabilization of that network, and a contributor to its stabilization. The microbes cannot be what stabilizes the network, and at the same time a product of that stabilization. Of course, once created, they may further stabilize the network, but that is an effect over and beyond the stabilization that brought them into existence in the first place. The problem is not solved by Latour's insistence that things and texts are fundamentally of the same kind, i.e. actants, and that there is thus no fundamental difference between the status of micro-organisms, and the status of the word "micro-organism" as featured in texts arguing the existence of such things. The question is not how many ontological *kinds* of things stand behind a given claim, but how many *individual* actants do so. It is obvious that the claim that micro-organisms exist gains drastically increased support from the myriad of micro-organisms that are encountered every day in laboratories all around the world, over and above the support the receive from texts referring to such organisms.

10. The weakness of Latour's temporal constructivism is further demonstrated by the parallel case of space. Consider an astronomical datum: When the fragments of the comet Shoemaker-Levy crashed into the surface of Jupiter in July 1994, this event had been predicted months in advance based upon Newtonian mechanics, with an error of only a few seconds. This remarkable prediction constituted an extension of the range of Newtonian mechanics into the depths of our solar system, but without a similar extension of the reach of our laboratory practices. The event took place in a setting that was in no way controlled by human action.

It might be argued that this result only exists, as a scientific fact, because we can direct our telescopes and observe the crash; in this way, we somehow extend our laboratories to the surface of the faraway planet (cf. *Irreductions*, § 4.5.4). But it is a meaningless stretching of terms to say that our laboratories are somehow extended to the surface of Jupiter, just because we can observe that surface through telescopes. This is an extension of the term "laboratory", not an extension of laboratories.

At this point, however, we should recall Latour's other metaphor for network practices, that of termite galleries. Termites make things parts of their network of tunnels by dragging them into their tunnel system and incorporating them into it – literally by eating those things and building the tunnel walls out of their excreted remains. Thus, rather than networks reaching out to things, things are dragged into networks. Formally, this suggests a solution equivalent to the one used for the temporal case, introducing an extra spatial index to indicate the location of the laboratory into which the object is incorporated. It is indeed true that the comet crashed into the surface of Jupiter, millions of miles outside the reach of even the remotest laboratory. But here we must remember the dual indexes: What is true is that the comet fell on the planet *as registered in a observatory on earth*. Thus, the discussion concerns a dually indexed sentence, such as, "The comet crashed into the surface of Jupiter$_{\text{Mount Palomar}}$".[51]

But now problems begin to multiply. The truth value Latour attributes to the above sentence is, of course, relative to his own geographical location from which he assesses the evidence; in other words, we are discussing the sentence, "The comet crashed into the surface of Jupiter$_{\text{Mount Palomar, Paris}}$". Or, since the generation of the latter sentence has a geographical location too, the real sentence we are discussing reads, "The comet crashed into the surface of Jupiter$_{\text{Mount Palomar, Paris, Copenhagen}}$". And, of course, the sentence on which my reader reflects will have a further geographical index. We may justly feel that our indexing device has spun out of control. The proliferation of indexes runs amok and we quickly lose track of the content of our discussion.

The problem facing Latour here is both ontological and semantic. On the ontological side, it is hard to fathom a reality of such infinite, or indefinite, multiplicity. It consists not of such things as a comet and the planet Jupiter, but the comet and planet from-the-point-of-view-of-Mount-Palomar-from-the-point-of view-of Paris-from-the-point-of-view-of-Copenhagen-from-the-point-of-view of... from-the-point-of-view-of-your-armchair. This is heavy stuff.

On the semantic side, there is the question of the meanings of scientific sentences thus embedded in their nested networks of actants. One of the problems that grounded Dummett's anti-realist programme was that of providing a viable anti-realist semantics. Semantic meaning seemed to dissolve in endlessly branching links between the *analysans* and an ever-growing set of sentences that serve as evidence for it. Dummettian semantics is even conservative in linking sentences only to other sentences; the situation is much worse for Latour, who links scientific sentences to an unruly crowd of actants. Latour impresses upon us that we are dealing with a different microorganism after it has been tied to the network as an ally; translated into semantic mode, this says that the meaning of the term "microorganism" has changed after its inclusion in the network. But how can we construct a plausible model of this kind of meaning and its constant changes? Meaning itself is lost when sentences are turned into "immutable mobiles". Words may maintain their *reference* to individual things, in the way that Latour has shown, through a concrete trail of "transformations". But *meaning* seems to get lost in the process: scientific networks do not harbour contentful thoughts about reality, but only interact with it, just as mycelium does not think about the substrate upon which it feeds.

Thus, the metaphysical underpinnings of Latour's "actant network theory" of science do not suffice to save it from the fundamental problems we touched upon earlier. In particular, a contradiction remains between Latour's constructivism and his claim that, contrary to the Strong Programme, actor network theory recognizes the theoretical entities of natural science as bona fide members of the network. No amount of playing around with multiply time-indexed sentences can change the fact that such entities cannot contribute to the stabilization of the network, if the stabilization of the network is a precondition for their existence.

11. A final question remains: How has Latour managed to combine the two aspects of his work, that of the empirical student of science and that of the radical philosopher? More precisely put, how has he managed to become a highly influential representative of technology studies (indeed, probably the leading figure in the field), with a large following in Europe and the USA, when his work is based upon a highly abstruse and highly speculative metaphysics?

The explanation does not seem to be that his admirers simply ignore the metaphysics. On the contrary, there seems to be some intellectual prestige connected to the radicality of Latour's thought. If one were to invoke a more explicitly sociological framework to explain Latour's success, one might point out that his message is ideally suited to the interests of the new "Triple Helix", the politico-administrative-industrial complex that is emerging, especially in Europe, as a result of the continent's efforts to catch up, in the global economy, with USA and the emerging powers of China and India. The catchword is Mode 2 production of science. This is the idea (and ideal) of a kind of science that dissolves the classical divisions between basic science, applied science and technology, both in manner of generation and in mode of function. Where the traditional, Mode 1 model has it that science arises out of purely theoretical concerns and within the framework of institutions dedicated to this purpose (i.e., universities), only later to trickle down to contexts of application, Mode 2 science is designed from the very start for practical purposes. Correspondingly, it emerges in contexts where theoreticians and customers interact from the start. Among the latter are both representatives of business and of the political system that partially finance the research activities.

Latour's work is highly critical of the ideology of Mode 1 science, and extremely flattering to the practitioners of Mode 2. In such works as *The Politics of Nature*, Latour tells us how Mode 1 springs form a hatred of the *demos*. It is designed to render the masses powerless by confining the production of scientific knowledge to narrow coteries of specialists, far removed from the common man and his practical concerns. Mode 2 production, on the other hand, is generated in the all-inclusive and egalitarian community of actants. Its democratic mode of organization extends not only to the common man, but to all of nature. The aim of the whole enterprise is empowerment – not only of the human masses, but even of things.[52]

This kind of thinking is perfectly designed to cast aspersions on those areas of research that still offer some resistance to the total mobilization of science as a resource in the global economic competition. They are depicted as inhabitants of an ivory tower from which they peruse, with fear and disdain, the antics of the *demos*. Instead, we should hasten the transformation of the old universities into "entrepreneurial" institutions that would collaborate with your friendly neighbourhood multinational in "connecting people", a motto that captures Latour's ideal of science and that also happens to be the slogan of a leading international information technology corporation.

Latour also sings the praises of politicians in a manner that, as he himself points out, makes him a very rare thing in the history of philosophy. Time and again, he makes the point that the political way of resolving issues – the tinkering, bargaining, compromise-making and coalition-forming – is not just the best way, but the only way. He berates the ambition of scientists to elevate certain issues out of the morass of political bargaining, transforming them into topics of cool scientific calculation by a group of experts (Latour 1988a, pp. 210, 225, 232). This is not just an impossible dream, but also a manifestation of elitist arrogance and, in the final analysis, hatred of the *demos*.

It would be a blatant case of ad hominem argument to dismiss Latour's thought as simply reflecting a desire to align himself with powerful current trends in politics and business. So, let us just say that Latour the actant has been remarkably successful in translating the original Science Studies programme into a form that would inspire other actants to connect with it. He has built up a firm alliance that makes his thought an "obligatory passage point" for many activities within modern science and technology work. Lately, he has tried to translate and transform the concerns of a large neighbouring network, that of the ecological movement, in such a manner that a fusion of networks might be possible, which would turn his agenda into a veritable global mass movement. All this is quite an achievement for something that, officially, started out as a narrow scholarly effort to end the dominance of *épistémologie* in French philosophy of science, using the Strong Programme as its weapon.

Chapter 8
Andrew Pickering and the Mangle of Practice

1. When Andrew Pickering published his main theoretical contribution to science studies, *The Mangle of Practice* (1995), this research specialty was already a familiar item within academia. Thus, there was no need for him to make a case for the very viability of a sociological investigation of science as such. Numerous different positions had already been articulated, providing a platform from which he could launch his own position. His particular approach could conveniently be defined by selective endorsement of – or opposition to – previous positions in the field. Along with the authors we have already examined, Pickering took his point of departure within the Strong Programme and, in early work, had actually adopted at least the phraseology of this approach (cf. Pickering 1980).[53] Like those other authors, however, he has gradually come to distance himself from certain crucial elements in the Strong Programme and has eventually moved quite close to the Actor Network Theory. In Pickering's recent work, discussing the finer points distinguishing him from ANT seems to be just as important to him as elaborating on the larger issues that separate him from the Strong Programme.

Somewhat mischievously, one might say that what primarily separates Pickering from Actor Network Theory is not a point of doctrine – although there are some, such as the status of the distinction between human and non-human agency – but merely a preference for a different metaphor. Where Latour talks about science as the outcome of the Actor Network, Pickering sees it as generated by a *mangle of practice* (Pickering 1995). Lately, the metaphor of science as *alchemy* has become prominent in his works (Pickering 2001). But when we spell out these metaphors, the differences are fairly negligible.

Pickering had already made a name for himself within Science Studies as the author of a comprehensive case study of the emergence of quark theories, *The Social Construction of Quarks* (Pickering 1984). The subtitle is *A Sociological History of Particle Physics*, but it is fair to say that the theoretical, sociological parts are rather undeveloped; the work shows Pickering's original training in theoretical high energy physics to good effect but also reveals that he was just a newcomer to social science at this stage. A coherent, original sociological position would only emerge gradually and piecemeal in later work, to be finally presented in its entirety in *The Mangle of Practice*. This study will form the focus of the present chapter, with occasional forays into other texts.

F. Collin, *Science Studies as Naturalized Philosophy*, Synthese Library 348,
DOI 10.1007/978-90-481-9741-5_8,

2. Pickering's chief grudge against the Strong Programme is that it underestimates the role of the material world in the production of science. We encountered a similar criticism in Latour, who urged that nature must be reintroduced to the equation; Pickering follows him in this. The resources of the social sciences are not strong enough to explain the historical trajectory of science (Pickering 1995, p. 9 ff, 1992, p. 5). As we saw in Chapter 5, this more inclusive methodology was criticized by Strong Programmers and their allies – in particular Collins – for risking trivialization by explaining the emergence of a scientific discipline in the very same terms that representatives of that discipline would employ. Such accounts would thus belong to science rather than to sociology; more specifically, they would resemble the lore that a discipline will often generate in order to explain its own emergence, and justify its status. (In the standard terminology, they would be "Whiggish" accounts.) This was why Merton defined such accounts as lying outside the compass of the sociology of science – at least as long as we accept the factual validity of the science in question.

Latour and the Actor Network Theory attempted to meet this challenge by moving to a deeper level of analysis, that is, the actant level. Pickering adopts another policy, which is initially introduced at the level of theory of explanation, but which turns out in the final analysis to reflect a metaphysical position. We should adopt what he calls "real-time" accounts, which only employ resources that were available to the agents involved as the events explained unfolded (1995, p. 14 f) – to which are added, of course, the resources supplied by the investigating sociologist. In this way, we avoid the illusion (as Pickering sees it) of a vantage point, defined by currently accepted science, from which scientists retrospective reinterpret the historical events to make them appear as leading inexorably towards the establishment of that very platform. Real-time explanations, by contrast, highlight the uncertainties, false starts, controversies and alternative interpretations characteristic of the development of a scientific theory as a contingent historical process. (This corresponds to the distinction drawn by Latour between "science in the making" and science as finished product.) In this way, social studies avoid handing over their disciplinary autonomy to current natural science; still, nature is allowed to play a role, since the account is a history of the encounters through which we come face to face with nature's own agency (Pickering 1995, p. 14, 1989).

At a deeper level, it turns out that such real-time explanations are the only ones that can be given, according to Pickering, if we want to stay true to the fundamental character of reality. This follows from the strong *instrumentalism* to which Pickering subscribes. I have argued that instrumentalism is a fundamental and common, but not always explicitly articulated, premise of Science Studies. In Pickering, this premise is stated quite explicitly and is moved to centre stage. He prefers to refer to it as a *performative* perspective upon science, though, as opposed to a representational one (Pickering 1995, p. 5 ff); here, however, I shall stay with the standard term. Instrumentalism implies that the retrospective story of the emergence of a scientific theory that can be told within the framework of that very theory is valid only in this particular perspective; alternative, equally good accounts could be given within alternative instrumental frameworks. There is no such thing as a picture showing how nature *really* is and how this came to be discovered, since this would call for us to concede that one of the instrumentally equivalent pictures is somehow uniquely adequate and hence *true*. But we have no rational grounds for such a concession (Pickering 1995, p. 186 ff, 1989).

The point that Pickering makes here is an epistemic one, in the first place, but he goes on to draw an ontological implication from it, thereby giving a strong, metaphysical interpretation to his instrumentalism. It is not merely that we cannot confidently single out a privileged description of reality, for lack of discriminative power in our standards of theory choice: We cannot even make sense of there being a uniquely privileged description of reality, other than and beyond those that are supplied by the various alternative ways in which we describe it for practical purposes, including those of science. There is no description of reality as a pure *noumenon*.

The only thing we can say about reality in itself is that it manifests *agency*, which we encounter in our contacts with the material world. Thus, Pickering agrees with Latour that the basic defining feature of reality is its active, dynamic character. This was captured in Latour's choice of the semiotic term *actants* to name his basic items. However, while Latour tried to tell us something more about this fundamental nature, Pickering is more taciturn, making no attempt to address such traditional issues of metaphysics as the status of universals or potentialities, such as those we find in Latour.

Pickering's ontology is not quite as radically monistic as Latour's. As we saw in the previous chapter, Latour basically sees the world as made up of entities belonging to one and the same class, that is, actants. We may, for various practical purposes, draw distinctions in this homogeneous class, such as that between humans and non-humans, but the features in terms of which such distinctions are drawn are themselves products of the activity of actants; they are "networking effects". Pickering, on the other hand, takes the distinction between humans and non-humans to be basic and irreducible – or rather, he takes the related distinction between *intentional* and *non-intentional* to be basic (Pickering 1995, p. 17 f). On this point, he explicitly takes exception to Latour's position. Still, we shall see that his analysis leaves the contents of this distinction somewhat modified.

3. When the basic conception of reality as a dynamic field is brought to bear on science, the latter turns into what Pickering calls a *dance of agency* between material nature, measuring instruments, theoretical frameworks and human beings and their intentions (Pickering 1995, p. 21). Pickering refers to the three last mentioned factors as *culture*, in a broad sense that makes it roughly coextensive with *manmade*; thus, they are set against Nature, which is the non-manmade contributor to the process. Science is an attempt to achieve what Pickering calls "machinic capture" of nature's agency by means of such cultural tools and resources. Capture is a dynamic process through which these cultural resources are deployed in what Pickering refers to as *practice*, in the plural. Practice is a creative and unpredictable *extension* of cultural resources, not a rigid and predetermined application of preexistent intellectual and material tools. Pickering refers to this trait as *emergence*.

The metaphor of a "dance of agency" is meant to capture the way that the parties to the process alternate in taking the lead, assuming an active and a passive role by turns. Less metaphorically, Pickering describes the process as a matter of alternating phases of *resistance* and *accommodation* (ibid., p. 22). An obstacle is encountered to the effort to make a measuring device function reliably, or to establish a conceptual interpretation of its output. In response, tinkering is resorted to everywhere, both at the machinic level, where the setting of the machine may be modified, or in the theoretical, interpretive apparatus. There is no telling in advance which kind of tinkering will succeed, that is, will achieve an equilibrium between the machinic and theoretical aspects of the entire setup. Pickering refers to the same process as "the mangle".

We noted above that Pickering considers the distinction between human, intentional participants in the scientific process and their non-intentional, material interactants to be fundamental and irreducible. Still, intentions are not fully formed independently of the scientific enterprise, controlling it from the outside, as it were, but are immanent in that very process and only become fully defined and specific as result of the process. They, too, are shaped by the mangle rather than shaping it.

The same thing is true for the social factors that, according to orthodox STS, shape the dynamics of science. There are no enduring, strictly demarcated social factors such as interests, social structure or *Weltanschauungen* that determine the development of science while themselves remaining unchanged in the process. They all become transformed as they pass through the "mangle of practice".

Thus, overall, Pickering depicts science as a dynamic process through which certain cultural, manmade elements – ideas, machines, social structures – are deployed in the material and cognitive "capture" of nature. Science is not a matter of generating veridical representations of reality, but of achieving reliable practical interaction with it. The process is thoroughly emergent; it is not just unpredictable from a human perspective, but is inherently undetermined. Not only are the cultural factors involved emergently transformed through their mutual interaction; they are all, severally and collectively, mangled through their confrontation with a further interactant, nature, which has no determinate features beyond that of offering resistance to the agency of the other factors. These factors are all mangled through practical application, that is, the actual human activities through which the factors are brought into play, while at the same time being transformed. In moving to this level, Pickering also undertakes the shift from a macro- to a micro-approach that we found in Latour, leaving society at large behind in order to delve into the details of laboratory practices.

4. So much by way of a brief, abstract statement of Pickering's position. As he himself emphasizes, his argument can hardly be understood, nor can its merits be appreciated, as long as it is considered independently of concrete examples. (Thus we might say that it is "inductively based", like the Strong Programme.) So, let us turn to some of Pickering's detailed exemplifications. The mangling has many aspects, most of which are at play in the example of the American physicist Donald Glaser's work on the *bubble chamber* (op. cit., p. 37 ff). Here we encounter the mangling of material agency, of human agency and, finally, of social relations.

First, Pickering considers the mangling of *material agency*. Setting out to develop a detector for cosmic rays with much higher yield than the familiar cloud chamber, Donald Glaser went through numerous experimental setups; the only one that eventually worked was what came to be known as the bubble chamber. Here, the gaseous medium of the cloud chamber is replaced by a liquid that, because of its higher density, promises a much higher number of interactions with incoming particles. Drawing upon a study by Peter Galison (1985), Pickering describes the various stages and elements in the process of tinkering that finally led Glaser toward a reliable instrument. For instance, Glaser put a great deal of effort into developing devices that could trigger the expansion of the bubble chamber – the process through which tracks are formed – at the very instant it was hit by cosmic radiation, an effort that would eventually prove futile. He also tested out numerous different kinds of fluid to fill the expansion chamber, among others liquid hydrogen and xenon. Xenon with an addition of ethylene finally emerged as the preferred choice. The dimensions of the chamber were also varied, as well as the material used in the gaskets and diaphragm of the device, to avoid interference with the processes inside the chamber. Pickering emphasizes that these efforts had the character of practical tinkering; there was no firm knowledge on the basis of which the outcome could be calculated in advance. Thus, the eventual successful capture of material agency – that is, the production of a stable, predictable result – was an emergent phenomenon.

Next, Pickering turns to the *intentional* aspect. This, too, is not a case of fixed and strictly defined factors serving as inputs to the scientific process, which they shape while being themselves left unchanged. Rather, we have a feedback process (or, as Pickering prefers to describe it, a *mangle*). In the bubble chamber case, this is illustrated by the frustration of Glaser's original intention of using the bubble chamber in the investigation of cosmic rays and the subsequent transformation of this intention. Glaser's plan ran into a "resistance": the failure to construct triggering devices that would set off the expansion process at the exact moment when a shower of incoming cosmic rays hit the apparatus. Instead, Glaser had to switch to artificially constructed sources of radiation that could be fully controlled by the experimenters. This represented a major transformation of Glaser's aims, since such devices involved the activities of a large staff of scientific and technical personnel and thus implied a form of "industrial style", Big Science undertaking that Glaser had fought to avoid.

This leads to the final aspect of the mangle illustrated by the example, that is, the transformation of *social structure*. In turning from cosmic rays to accelerator work, Glaser was inexorably moved from a small-scale operation – just he and a graduate student – to a relatively larger group that, at its peak, involved the collaboration of 14 people. This happened, despite the fact that Glaser tried several tricks at the purely material, machinic level to counteract the unavoidable organizational implications of the switch from cosmic rays to artificially generated rays. For instance, the change from liquid hydrogen to xenon was motivated by the larger density of the latter that would allow a smaller apparatus for a given yield and thus a *reduction* of technical staff.

In all these cases, Pickering stresses the temporally emergent nature of the process, both in an epistemic and an ontological sense. There was no way in which the twists and turns of these processes could have been predicted by the agents ahead of time; there are not even precisely defined preexisting factors that could have allowed such an inference, even in theory.

5. A special challenge to the idea of science as a matter of resistance and accommodation is posed by the role of theories and the concepts of which they are composed. Pickering puts the issue as follows:

> Thus, while it is easy to appreciate that dialectics of resistance and accommodation can arise in our dealing with machines – I have argued already that the contours of material agency emerge only in practice – it is hard to see how the same could be said of our dealings with concepts. And this being the case, the question arises of why concepts are not mere putty in our hands.
>
> (Pickering 1995, p. 113)

Although Pickering does not explicitly say so, the background assumptions generating this puzzle are clearly Wittgenstein's rule-following considerations. We have witnessed the importance of this argument to Bloor's and Collins's thought; indeed, it played a role even in Latour. Bloor and Collins invoked those considerations to break the hold upon us of Platonic realism with respect to concepts. This is the idea that concepts are concretely existing entities that we somehow run our heads against in our intellectual practice, if we make false moves. In this way, concepts nudge us along in our intellectual practices – or, to change the metaphor in a direction familiar from previous chapters, they provide the rails that guide us along in those practices.

Bloor and Collins are at pains to demonstrate that we have no such aids in our conceptual practice. To the question what then determines those practices, they have a ready answer: *social factors,* such as interests. Pickering has explicitly dismissed this solution and follows his fellow post-Strong Programmers in adopting a micro-level analysis instead. Rather than social interests, we have the mangle as the sole determinant of scientific development. In light of the rule-following considerations, however, can we make sense of the idea that concepts, too, somehow add determination to the mangling process? How can they influence it at all? Why are concepts not mere putty in our hands, as Pickering puts it?

Fundamentally, the answer that Pickering returns is identical to Wittgenstein's: conceptual rigidity is supplied by the drill that human beings undergo in learning language and other rule-governed practices. A given, routinized conceptual practice possesses a power of its own that constrains free human creativity or stipulation. Pickering refers to this as *disciplinary agency* (op. cit., p. 115). This term must be understood correctly. It is not a matter of a discipline in the sense of an abstract unit of the academic curriculum exercising agency here, but the *disciplined* human action, which sustains and defines such a unit.

However, this answer is too general to serve Pickering's interests. He needs to give a more detailed analysis demonstrating not only the fact of such resistance, but also how its mode of operation displays that "dance of agency" – the alternating phases of resistance and accommodation – that is central to his analysis. He proceeds by means of an example picked from mathematics, involving the development of quaternions by the Irish mathematician William Rowan Hamilton (ibid., p. 126 ff; Pickering and Stephanides 1992).

Hamilton's work grew out of the problems besetting the foundations of algebra in early 19th century, caused by difficulties in interpreting "absurd" quantities (i.e., the square root of negative numbers). A main line of effort centered around modelling such numbers in terms of geometrical properties. Hamilton adopted this line and so faced the challenge of extending complex algebra to accommodate the "absurd" quantities while still retaining the possibility of their geometrical representation. We might also call this the problem of establishing a comprehensive *structural analogy* between the two fields; Pickering, like Bloor and Collins, views scientific thinking as basically analogical in its modus operandi.

Analogical thought typically starts with one or more points of positive analogy between a source and a target; the fruitfulness of the exercise depends upon the extent to which the analogy can later be expanded and disanalogies put aside or otherwise overcome. Pickering divides the process of conceptual development by analogy into three different aspects, or rather temporal phases. In our concrete case, Hamilton first established a tentative, positive analogy between the algebraic system and a particular analogue in the geometrical system. The novelty of Hamilton's approach was his use of a three-dimensional system, instead of the common two-dimensional modellings. Pickering refers to this as establishing a *bridgehead* for the algebra in the three-dimensional geometrical world. He stresses that this is a free, unconstrained process, as nothing dictates which points of analogy should be selected.

However, once a bridgehead is chosen, a number of moves by which the analogy must henceforth be developed now impose themselves upon us. Pickering refers to this as the *transcribing* of formal features of the source – the algebraic system – into the conceptual framework of the target area (in this case, geometrical systems). This forced process may lead us into areas where the target offers resistance to the transcription. True propositions or valid principles of calculation in the source system may fail to be mapped into truths or valid principles in the target; the extension of the initial point of departure of the analogies can bring us face to face with striking disanalogies. This is where resistance makes itself felt and we shall return to the point in a moment.

Finally, there is *filling*. Certain aspects of the source may have no clear analogical counterparts in the target, without, however, being contradicted by direct disanalogy. This invites an operation of filling-out, which may be construed as the arbitrary establishment of supplementary analogies between further elements of the source and target, conjoined with, but not related to, the first one. (Remember that the initial analogy and its corresponding bridgehead were freely chosen among an indefinite number of possible alternatives.) The procedure is justified as long as the new compounded analogy turns out to be a fruitful tool for exploring the source, or the target, or both.

Now, let us return to resistance. In working out the model relationship, Hamilton came across a resistance in transferring – or, in Pickering's preferred terminology, transcribing – the standard rules of general algebra onto the geometrical model. Hamilton tinkered with various ways of handling this difficulty, one being the arbitrary assignment of the value of zero to one product in the equation. Hamilton eventually chose a more radical and consequential way out: to give up the principle of commutation inherent in the source. This turned out to be a very fruitful move. At the same time, a modification also had to be accepted in the bridgehead, since the original bridgehead in a three-dimensional geometrical space had to be replaced with a four-dimensional one (hence, the name of "quaternions" for Hamilton's discovery).

In his analysis of Hamilton's work, Pickering is at pains to establish several points. There are countless ways in which a conceptual system may be extended and developed with another conceptual system as a resource for comparison and contrast. Not only is the point of departure (the bridgehead) arbitrarily chosen, it may even be given up later. There are even countless ways to accommodate the inevitable disanalogies (resistances) that arise as the analogical process moves ahead. Fillings are also arbitrary. On the other hand, transcription is compulsory. Considered as a sequence of phases in an overall process, the phenomenon of conceptual extension thus manifests the traits of a "dance of agency", where human agency is at play in establishing the bridgehead, only to yield to "disciplinary agency" in the phase of transcription, where the principles inherent in the analogy dictate our steps, and finally stepping in again in the third and final phase of "filling".

6. In all these concrete examples, Pickering holds that something more is at stake than just the realms of nature, machines, intentions and theoretical structures interacting and influencing each other. He hints that there is also a mangling at the conceptual level, since the very notions of material agency, human agency and disciplinary agency are transformed in the process.

Perhaps the point is clearest in the case of material agency. This is traditionally conceived in terms of the notion of causality, embodying the idea of a number of preexisting conditions; a triggering event, the cause; and a resultant event, the effect, which follows from the combination of the first mentioned factors in accordance with general laws. Now, Pickering wants to dispose of the notion of preexisting determining conditions, which leads to a reformed notion of material agency.

The situation is somewhat different when it comes to the mangling of human agency. In this case, there is no denying the pre-existence of human aims and desires prior to the inception of the "dance of agency"; this is a obvious fact of everyday experience, which Science Studies could deny only at their peril. Still, even here, Pickering claims that a remoulding of the idea of human agency, at the conceptual level, is taking place. This is signalled in his adoption of the term "posthumanism" for his approach to the researchers' contribution to the process. This is supposed somehow to take us beyond a traditional humanist account that represents humans as subjects of determinate thoughts and intentions.

Still, it is a little unclear how Pickering's position diverges from the received view. He makes much of the point that scientists' intentions develop and change during their work. But although this may contradict very simpleminded, Whiggish accounts of the scientific process, which would represent Newton as setting out to discover Newton's Laws from the very start, it is hardly news to anyone who has studied the history of science in any detail. The serendipitous nature of the process has been often documented and is readily granted by everyone.

Occasionally, Pickering expresses his point by saying that the joint mangling of human and natural agency forms an interim zone, where neither pure human agency nor natural agency are at work; rather, the very agency involved is somehow transformed into a third form different from either (Pickering 1995, pp. 53–54). This is shown by the fact that the resistance encountered in this sphere may be indifferently reconstrued either as pertaining to human or to machinic agency. That is, a given resistance to machinic capture may be handled either by a modification of the machine – the physical experimental setup – or of the experimenter's aims in conducting the experiments. However, it would seem that this joint mangling zone is just an artefact of an analysis undertaken with insufficient temporal resolution: In terms of a slow-motion micro-analysis, the blur would again resolve itself into a "dance of agency", where human intentionality and agency and machinic agency remain (conceptually) distinct, but take turns. The strands of human and machinic agency may be intimately intertwined all the way down in the scientific process, but they still remain separate, even at the finest micro-level.

There is another aspect to the modification of human agency involved in Pickering's analysis. To capture nature's agency, man produces machines such as bubble chambers or microscopes. But he also turns himself into a kind of machine; that is, in his interaction with the machine, he makes the manifestations of his own agency more machinic, imposing a discipline upon his spontaneous actions without which the dance of agency will not reach a stable point. As Pickering puts it, the dance of agency needs a choreography to impose structure upon it (Pickering 1995, p. 101 f).

Finally, there is a conceptual mangling of the *social* aspect. In the bubble chamber episode, this is illustrated by the way Glaser's work and the material mangling involved pushed him towards a different scale of organization than that originally envisaged (ibid., p. 58 f). Glaser's original goal (intention) was that of conducting small-scale, individually based research, as was the norm in the field of cosmic radiation in which he was originally trained; he abhorred the large-scale, "industrial" style of doing research that was becoming prevalent. However, he failed to stabilize his bubble chamber in a setting where it would detect randomly incoming cosmic rays and was forced to associate it with accelerator-based research, which inevitably runs on a larger organizational scale. He would end up heading a laboratory of 14 people, much against his wishes. Pickering implies that a conceptual mangling takes place here, as well. However, there is little here that could not be captured by a traditional micro-sociological or organizational analysis – such as the one just hinted at. It is unclear in what sense a modification of the very *concept* of organizational structure is at play.

7. Although Pickering adopts an overall performative, non-representational view of science, he does not deny that representation plays a role and, indeed, that

representations, when true, may be said to state *facts*. His point is that this aspect is dependent upon the performative aspect, however. To illustrate this, we may look at a further example, this time one in which all the different factors involved in the scientific process are at play, that is, human agency, material agency – further subdivisible into machinic agency and objectual agency – and conceptual agency. The last mentioned factor has a particular significance in this context (Pickering 1995, p. 68 ff).

Machinic capture, in a broad sense, has two stages. The first involves building a piece of material apparatus – a machine – and getting it to behave regularly and predictably. The second, referred to by Pickering as *framing*, is the deployment of this machine as a tool for investigating further aspects of nature, for example as a measuring apparatus. Elaborating a little upon Pickering's metaphor of capture, we might say that the former is a matter of capturing and taming a wild beast, while the latter is the process of making the newly domesticated animal carry out productive work. In the case of laboratory apparatus, the aim is to make it produce results that can be interpreted as measurements of further aspects of material reality.

Metaphor aside, what is added when we turn from the capture of material agency to its framing are chiefly *conceptual structures* in terms of which to effect the framing. This does not mean that we now ascend to a transcendent realm of Third World entities, in Popper's sense. Concepts are seen as part of a cultural practice, which is illustrated by the "disciplinary agency" that we examined above.

Pickering illustrates the phenomenon with a detailed analysis of the Italian physicist Giacomo Morpurgo's research on quarks in the 1970s (op. cit., p. 71 ff). The theoretical background of the research was work published by Nee'man and Gellman in the late 1960s, indicating that underneath the rapidly proliferating family of "elementary" particles known then, there was a group of even simpler particles that might explain the fecundity of the former; thus, hopes were that this level, referred to as "quarks", would finally present us with the fundamental building blocks of the universe.

Physically, this stratum of entities would be identified by possession of fractional electrical charges, more precisely one-third (or multiples thereof) of the elementary charge. Setting out to look for such charges, Morpurgo constructed a measuring apparatus that was basically a latter-day version of Millikan's famous device for discovering the elementary charge, but much more sophisticated and with a sensibility many orders of magnitude larger than the latter, thanks to the fact that this apparatus was capable of handling much larger quantities of matter in a single measurement.

Pickering tells us a fascinating story of how Morpurgo and his crew eventually managed to achieve "machinic capture" with this piece of apparatus. Periods of apparent stability were interrupted by phases in which the apparatus behaved erratically, for reasons that were not immediately transparent. Sometimes, stability was reestablished by mere physical adjustments of the apparatus, but occasionally it could only be achieved by an adjustment of the theoretical interpretation of what went on in the apparatus, followed by adjustments in accordance with the new understanding; sometimes this called for considerable modification of standard conceptions of the workings of such machines. Thus, a docile and reliable machinery can not be got by mere material tinkering, but calls for theoretical reinterpretation; the two enter the kind of dance of agency that Pickering refers to as the mangle.

The role of conceptual, theoretical elements turns from being important to being essential when we take the further step of interpreting the output of the entire setup as the production of *facts*, for facts are linguistically formulated, conceptually informed items, and require conceptual structures to make up the framework within which they exist. Once we have introduced conceptual elements and established an interpretive equilibrium, our setup consists of the following series of elements. First, we have a sample of physical nature embedded in the setup, with respect to which machinic capture has been achieved; that is, certain manipulations of the machine produce a predictable and stable outcome. Next, we have drawn upon established physical theory to get a conceptual grasp of this connection; Pickering refers to this as our *interpretative theory*. In Morpurgo's experiments, electrodynamics served in this role. Finally, by means of this grasp, we may translate the output of his instrumental setup into what Pickering calls *phenomenal accounts*. These are the theoretical, conceptual interpretations of the machine's outputs that give the latter the status of *measurements*, the contents of which henceforth count as scientific *facts*. In the quark experiments, the fact established was to the effect that no fractional charges were found and that limits could be put on the likelihood of finding them.

8. We saw in the previous section that Pickering grants science a conceptual, representational side, too, and that, within this perspective, science may be described as a producer of *truths* about nature. Still, the performative aspect is predominant, since, in the final analysis, the representational aspect is only to be understood on the basis of the former; the representations are mere contributions to the performance. This defines Pickering's "performativism" as a version of orthodox instrumentalism that readily admits that science embodies conceptual, representational elements that are prima facie different from material tools; it insists, however, that such elements turn out, on closer inspection, to be mere auxiliaries to the function of science as an instrument for the practical commerce with reality. They are not "pictures" of reality in a literal sense, but merely thought models facilitating the intellectual manipulation of the mathematical apparatus that serves the core function of science.

In Chapter 6 of *Mangle of Practice*, Pickering adopts a closely similar stance. He grants that the experiments undertaken by Morpurgo established scientific facts (such as, say, the fact that the samples did not exhibit fractional charges). Still, such facts exist only within one particular way of achieving "machinic capture" of reality. The experiments did not show that the world as it is in itself has this property. The reason is that there is an indefinite number of alternative ways in which machinic capture of reality might be achieved, each married to a different conceptual framing of the outcome. None of these captures is privileged; none of them can be taken to show us what the world is *really* like.

In substantiating this point, Pickering adopts a version of the celebrated *argument from incommensurability* that has dominated the debate (op. cit., p. 186 f). The argument reflects the assumption that, if any scientific theory is to have a legitimate claim to privilege, thus expressing what the world is really like, this theory must be singled out by rational standards of theory choice. Next, it is argued that no rational algorithm is strong enough to point to any particular version as uniquely superior. In the classical debate, the notion of incommensurability, introduced by Kuhn and Feyerabend, has played a central role. Rationalist models in the Popperian tradition assess the merits of rival theories in terms of the number of experimental predictions that speak in their favour as compared to the number that contradict them. This proposal requires a metric for counting the number of positive and negative instances, which, in the final analysis, presupposes that facts relevant to different theories, or paradigms, are couched in languages that are at least commensurable. But, according to the incommensurability claim, this is not the case.

Given Pickering's downplaying of the conceptual, representational aspect of science, this argument has somewhat less appeal to him. Instead, he transposes it into his preferred performative idiom, producing an allegedly deeper version of incommensurability that he refers to as *machinic* incommensurability. This is the argument that various machinic set-ups will produce different captures of the world (ibid., p. 188). Since these may be different in every aspect they are incommensurable, and hence block any rational comparative assessment that might allow us to consider one as giving a more adequate picture of the world than another.

Pickering's presentation is quite sketchy here and not very clear, but we may try to derive its more precise import from yet another historical case with which he illustrates it. The case provides further details of Morpurgo's experiments with fractional electrical charges, as reported by Pickering in a previous chapter (op. cit., p. 210). The crux of the story is that Morpurgo failed to capture fractional charges with his experiment and took this to disprove their existence; at approximately the same time, however, the Stanford physicist William Fairbank claimed to have found such charges and thus to have detected free quarks. This looks like a straightforward contradiction, but, according to Pickering, is no such thing, since the two results are incommensurable. Morpurgo achieved a negative verdict concerning a sentence that we may express as, "There are fractional charges-as-captured-by-the-Morpurgo-apparatus". Fairbank got a positive verdict with respect to a different sentence, "There are fractional charges-as-captured-by-the-Fairbank-apparatus". These two sentences pertain to entirely different setups and have no logical connections whatsoever, hence, no contradiction ensues from their simultaneous endorsement.

Pickering leaves the discussion here, satisfied that he has delivered a final blow to the philosophers' search for rational models of science. It is striking that he tells us nothing about what attitude working scientists should adopt to the apparent opposition between Morpurgo's and Hamilton's results, according to the "performative" stance. If the answer is that we just leave the two of them alone, each happily pursuing his own agenda, without trying to decide who is right and who is wrong, it would seem that Pickering has inadvertently exhibited the feature to which enemies of instrumentalism chiefly object. Instrumentalism, they claim, will leave us with a totally fractured science, since there is no inherent push towards a unified, all-encompassing picture of the world (cf. Popper 1963d). Apparent contradictions between theories can always be handled with the technique demonstrated by Pickering in the above example: by treating them as equal, but separate. It is surprising that Pickering apparently sees no problems in such a scientific policy.

9. According to Pickering, science is thus not a rational enterprise, since it is marred by incommensurabilities. It is also inherently *relative*, since its development is inescapably path-dependent, reflecting the way that contingencies stacked up during the process of achieving “machinic capture” of reality (Pickering 1995, p. 201 ff). These two features would normally be taken also to compromise the objectivity of science. Pickering does not accept this conclusion; to him, the scientific investigation of reality is objective in the sense of not being determined solely by subjective features of the experiments, nor even of social features of the laboratory setting or the societal background in which it is embedded. Instead, it is co-determined, in a mangled way, by reality itself. Hence, it is *objective* in an ontological, rather than in an epistemic sense (op. cit., p. 194 f). In conformity with this labelling, Pickering even classifies his position as a kind of realism, named *pragmatic realism* (ibid., p. 183). At the same time, it amounts to anti-realism from an epistemological point of view although, as Pickering stresses, an anti-realism of a special, non-sceptical kind (ibid., p. 190).

Corresponding to this picture of science, Pickering offers us a view of science studies as largely a *historical* enterprise. It is a micro-history, however, where developments are traced at the level of individual actors and machines and their mutual “dance of agency”. There is no attempt at large-scale macro-historical explanation, such as is found in Marxism, which would be met with the same kind of scepticism as large-scale sociological explanation. This is not to say that the mangle is incapable of operating at the macro-level; indeed, Pickering insists that his theoretical apparatus is “scale invariant” and may be applied at the micro-, meso- and macro levels (ibid., p. 234 f). But, at all levels, he repudiates the possibility of explanation in the sense of a deduction of events as flowing inexorably from a set of enduring parameters.

10. In his writings since *The Mangle*, Pickering has increasingly focused on the *posthumanism* that supposedly follows from his analysis (Pickering 2006, 2008). The idea that human agency and intentionality are radically insulated from the agency of things is repudiated. This goes together with a rejection of metaphysical dualism of a Cartesian kind. Pickering’s thought on this point shows some influence from a currently influential anti-Cartesian position, that is, Heidegger’s dismissal of the notion of an abstract human intentionality that is only related to actual external things by accident (Heidegger 1927). What emerges instead is a view of man as a “being-in-the-world”, a being ineluctably embedded in the material world. Pickering goes further in the direction of elevating material reality to ontological parity with man, however. Where Heidegger views material reality, in its primary ontological manifestation, as revealing itself in the mode of being-at-hand (i.e., as a tool for human action), Pickering rather sees it as a partner with whom man interacts as an peer in the mutual “dance of agency”.

It is noticeable that Pickering does not draw upon more recent philosophical developments, in particular within the philosophy of mind, to support his antihumanism; its basis is solely in the study of science and technology. Daniel Dennett's highly influential intentional instrumentalism might have been particularly useful (Dennett 1971, 1987), and recent work on "externalism" in the description of human intentionality might have been helpful as well. But they never receive any mention.

Pickering goes on to draw quite strong normative implications from this view. These are particularly clear in the empirical field of investigation to which he has increasingly moved, that is, technology studies. A crucial element in this emerging stance is a call for human beings to respect the autonomy of nature as our equal when dealing with it in large-scale engineering projects. For instance, he urges the U.S. federal authorities to finally let the Mississippi run its natural course and stop hemming it in, as generations of engineering projects for the Mississippi basin have attempted. This recommendation may involve telling New Orleans to drop dead, since the city will lose its status as a major port and economical centre when the lower run of the river is reduced to a trickle, but this is a consequence he is apparently willing to take in his stride (Pickering 2008). Thus, Pickering's thought has developed in the direction of a political, ecological agenda strongly reminiscent of the recent turn in Bruno Latour's work (2004). At the same time, Pickering's thought takes on an increasingly philosophical character; it would not be misleading to call it a *philosophy* of technology, rather than a *sociology* of technology. This philosophy consists of a metaphysical part, championing an anti-dualist ontology, and a normative part, which flows from the former and urges us to treat nature as our equal. We may note in passing that Pickering's stance offers little consolation for those who worry over technology, not because it offends against nature, but rather because it encroaches upon human existence. Among these worries are information technologies that threaten our privacy and erode the division between work and leisure time. Pickering's anti-humanism seems to aggravate, rather than lessen, this threat.

11. In this book, I have assessed positions within science studies, mainly with respect to two questions that emerged from the examination of the doctrines of the Edinburgh School. (1) What are its ambitions with respect to explaining the course of science and how does it handle the challenges that the Strong Programme faced? (2) How does it cope with the challenge of reflexivity? Let us put Pickering's account to the same test.

With respect to explanation, Pickering explicitly renounces explanation of the development of science in anything like the stringent sense of "explanation" that was adopted by the Strong Programme. This is an explanation that attempts to give an exclusive account of the *explanandum*, which involves dictating the terms in which it is described. We saw that Pickering repudiates the possibility of such accounts, since they are inherently "non-emergent", that is, they presuppose the pre-existence of enduring determining factors. But, according to Pickering, the picture thus presupposed has been discredited due to the effects of the "mangle".

Instead, "explanation" in social studies inevitably takes the form of historical narratives operating mainly at the microscopic, individual level. Such narratives will document how, at every turn of the scientific process, scientists faced choices that were fundamentally open and underdetermined. Their choice of one particular option must, in the final analysis, be understood as the establishment of a social convention for the interpretation and handling of nature. Other choices would have worked equally well, given adjustments elsewhere in the choreography of the continued "dance of agency" with nature and would have resulted in an equally successful material practice. Hence the kind of retrospective Whiggish accounts that are traditionally provided by scientists and philosophers, even of professional historians of science, constitute not only a falsification of the historical events, but also an obfuscation of the fundamental metaphysical state of affairs. They are based upon a realist metaphysics, with a "ready-made" (in Putnam's apt phrase, cf. Putnam 1983), determinate reality waiting for us to discover it, or even nudging us towards a correct depiction, through the experimental encounters in which nature graciously displays itself.

But, it might be objected, does not this argument mark a departure from a consistent instrumentalist stance? A consistent instrumentalist will surely have to stick to his reconstrual of scientific fact, and of scientific explanation, even when we move up to a meta-level where the object of explanation is scientific discovery, not some ground-level scientific phenomenon? Of course, the explanation provided will have the "as-if" character of all instrumentalist explanation, but will be no worse for that, by instrumentalist standards.

Pickering's response to such a challenge would run along the following lines, I suppose: the instrumentalist, "performative" analysis does indeed dictate a differential attitude to explanation in the sciences and in the social studies of science. Explanation invoking theoretical physical notions is permissible in a purely physical context, since such explanations are chief among the intellectual tools that theories are meant to provide us. Imposing a particular theoretical interpretation upon a set of phenomenal events gives us leads with respect to how we can cope with them and handles with which to manipulate them.

No such purpose is served, however, if instrumentalist posits are invoked in the context of explaining the *genesis of scientific beliefs*, which is the concern of the sociology of science. Here, instrumentalist posits are not aids in the control of physical reality. There will be other purposes at play, of course, but these could well be such from which the sociology of science would want to distance itself. Thus, as Kuhn pointed out, a given theory or "paradigm" often develops a particular Whiggish historiography designed to show how this theory expresses the final truth of the matter. This point of view is, of course, inimical to that adopted in science studies, which maintains that an entirely different theory could have prevailed if another set of machines had been available, bringing with them a different "machinic capture" of reality.

True, Pickering does envisage the possibility of over-all explanations of the development of science, beyond detailed historical accounts of its micro-workings. He sometimes refers to this as "pattern explanation", which provides illumination of science by pointing to overall recurrent features in its historical genesis (Pickering 1995, pp. 24, 146–147). Those features, of course, are those presented in his analysis as laid out above (i.e., the dance of agency, the mangle, the transformation of instruments, theories and other "cultural" elements, and so on). It is obvious, however, that this is not an explanation in a strict, deductivist sense. Cognitively, "pattern" explanation rather works by showing that an unfamiliar phenomenon displays, on closer inspection, familiar general traits that it shares with a whole group of other phenomena. It is explanation by reducing the unfamiliar to the familiar.[54] This mode of explanation lacks the property of exclusiveness that is needed if STS accounts are to claim hegemony over more traditional accounts of the progress of science.

Notice in this context that Pickering's position is different from Latour's more classically anti-realist view. Latour grants the existence of entities, but only once they have been discovered, although they may be retrospectively projected back in time. Pickering does not grant the real existence of theoretical entities, even when the praxis of which they form a part is firmly established; they remain instrumentalist fictions.

12. How do things stand with respect to *reflexivity*? Are Pickering's own conclusions undermined if we recast them in the framework that he himself recommends, that is, as extensions of elements of scientific culture, emergently processed through the mangle? A particular worry is Pickering's insistence that the products of scientific investigation do not count as a representation of reality, but are merely a performativity through which we cope with the world. As a matter of fact, Pickering's instrumentalism is of a particularly radical kind, since theories are not depicted as instruments for the intellectual handling of a class of "hard", independently specifiable facts, but rather of facts that themselves emerge, in part, though interaction with the instruments in terms of which we try to capture them. This was the import of the thesis of the "mangled" character of "machinic framing", which is the process though which facts are generated. This thesis is a crucial element in Pickering's claim that theory-building in natural science is essentially *historically relative*. Does this relativity spill over to his own studies and, if so, is it detrimental to them?

Pickering himself is rather silent on the issue of whether or not his analysis of science transfers to his own investigations. In any case, the answer might appear obvious: Pickering's own theoretical apparatus is blatantly metaphorical: He is not inviting us to believe that there is literally a mangle grinding out scientific results somewhere in the basement of research institutions. Nor would anyone take his talk about a "dance of agency" to be anything but metaphor. Thus, we are apparently forced to construe Pickering's own concepts as metaphorical, or analogical, and his conclusions as instrumentalistic. In other words, they would have the same status as knowledge in natural science, as depicted by Pickering.

Yet this conclusion might be rash. In a later response to critics, Pickering seems to back-paddle a bit from the celebration of the mangle metaphor in the 1995 text. Instead, Pickering urges us to treat "the mangle" simply as an abbreviation of the phrase "the dialectic of resistance and accommodation in fields of agency" (Pickering 1999, p. 168). This move invites a discussion as to whether the latter phase possesses a literal content, or whether it itself is metaphorical (or analogical), but this is not the place for such an analysis. Instead, let me simply present the upshot as a dilemma for Pickering. *Either* he may insist that his own theoretical apparatus is not metaphorical and hence not instrumentalist, in which case the universality of Pickering's "performative" analysis of science is lost; moreover, we would want to know how Pickering blocks the argument that led to the mangle analysis for natural science from applying to his own investigation. *Or*, Pickering grants that the mangle analysis applies even to his own studies and suffers the consequence of relativism: Pickering's own studies must represent a particular, contingent "stabilization" between reality – that is, the events unfolding in the scientific communities investigated – and his theoretical apparatus, chiefly the notion of a "the dialectic of resistance and accommodation in fields of agency". The facts that Pickering reports are "framings" of this part of human history. As such, they are as "path-dependent" and hence as *relative* as the facts of natural science.

Epistemic relativism of this or some other kind may indeed just be a part of the human condition; still, formulation of this predicament is a matter of some delicacy. Everything depends upon the precise wording offered and upon the implications drawn from the relativist position. With a little carelessness on this count, relativism may turn into something that threatens science as a responsible cognitive enterprise, and at the same time undermines the "naturalistic" argument on which that relativism is based. We have a case of pragmatic inconsistency. It seems that Pickering occasionally brings a consequence of this kind down upon himself. Consider this quote, which occurs towards the very end of his 400-page analysis of the new quark physics:

> On the view advocated in this chapter, there is no obligation upon anyone framing a view of the world to take account of what twentieth-century science has to say. The particle physicists of the late 1970s were themselves quite happy to abandon most of the phenomenal world and much of the explanatory framework which they had constructed in the previous decade. There is no reason for outsiders to show the present HEP [High Energy Physics] world-view any more respect. In certain contexts, such as foundational studies in the philosophy of science, it may be profitable to pay close attention to contemporary scientific beliefs. In other contexts, to listen too closely to scientists my be simply to stifle the imagination.
>
> (Pickering 1984, pp. 413–414)

What are the repercussions for Pickering's project if we replace "twentieth century science" with "Pickering's analysis of the progress of science", or "the world" with "the development of modern physics", in this quote? The implication seems inevitable that his body of work is something of which we may justly refuse to take account, if we happen to have a different agenda than its author. We are free to dismiss Pickering's historical "facts" as just products of the mangle constituted by Pickering's theoretical apparatus.

In the article "Editing and Epistemology" (Pickering 1989), Pickering offers a number of reflections relevant to this issue. In the first place, he observes that all historical accounts involve an element of *editing*, since the history of even the most trivial incident cannot be told in full (op. cit., p. 218). We may note that the term of "editing" represents a subtle retreat from Pickering's standard doctrine of the mangled and constructed nature of "facts", since it suggests a process of selection and foregrounding rather than brute production. Still, it is not obvious that a genuine change of doctrine is announced here. Next, Pickering presents an argument designed to give preference to his own instrumentalist-constructivist accounts over traditional realist-rationalist rivals, which might also be thought to deflect the charge of relativism. Realist accounts of science are inherently unstable, since they undermine themselves by inevitably turning up evidence showing that alternative construals were possible. Thus, they point towards an instrumentalist conception. This is not the case for constructivist accounts: they do not, from within themselves, generate evidence in favour of realist accounts and, hence, are stable. Such stability might be thought to provide constructivist accounts with a certain non-relativity, or a pragmatic simulacrum thereof.

But this argument suffers from a confusion of levels. Surely the instrumentalist construal of the accounts provided by science studies implies that alternative (albeit equally instrumentalist) accounts of scientific developments were possible. In other words, Pickering's concrete accounts will necessarily have (constructivist) rivals. This shows that any *particular* such study is unstable; acceptance of it must necessarily go together with the recognition that alternative accounts are equally valid. From this, critics might surely be tempted to infer that the constructivist approach *as such* is unstable.

13. At the beginning of this chapter, I made the point that Pickering's late arrival as a theoretician on the scene of Science Studies allowed him to define his position relative to a disciplinary field that was already well-articulated. We may add the observation that is has also made it possible for him to achieve prominence in this field with a position that, had it been launched in the mid-1970s, would hardly have made a mark at all. The claim that the progress of science cannot be explained in deterministic ways would not have shocked philosophical or sociological orthodoxy in the 1970s which was perfectly willing to admit – indeed would insist – that the growth of science is inherently beyond reach of deterministic explanation. This is captured in the notion of the "context of discovery" that leaves ample room for the "emergent cultural extension" involved in research or, as traditionalists might prefer to call it, the creative genius of great scientists. What can be explained rationally is only the subsequent process of assessment. Pickering has critical ideas pertaining to the latter issue, of course, which render problematic the notion of a predefined set of rationality principles guiding the development of science. But Pickering's contribution to the argument that even the context of justification constitutes an emergent mangle – notably, his doctrine of "machinic incommensurability" – would have been thought to add little to the challenge from *representational* incommensurability that already had philosophers quite worried at the time.

Pickering's ideas appear truly revolutionary only against the background of the STS credo that science can be explained in social terms. Thus, somewhat paradoxically, Pickering has managed to create a platform for his ideas within the Science Studies community by means of an agenda which basically subverts most of its original ideas and aims. Pickering's conclusions imply the impossibility of Science

Studies, in the sense of systematic, explanatory studies of science, adopting stringent standards of theory-building and explanation. No explanatory accounts can be provided. This totally controverts the original ambitions of the Strong Programme and one may wonder how Pickering has been able to avoid censure of the kind that has been administered to Latour, seeing that his doctrines are really every bit as antithetical to the Strong Programme as are Latour's.[55]

Part of the explanation may lie in the gradual shift in Pickering's interests from science to technology, which has made him somewhat marginal as far as orthodox Science Studies are concerned while also causing him to disappear from the radar screens of science warriors in the scientific community (although, of course, to Pickering, the division between the two areas is perfectly artificial). In step with this move, Pickering's work has generated increasing interest among technology and engineering communities. It is easy to see the appeal of Pickering's recent work in this context. While Pickering is critical of the tendency of orthodox engineering to overlook the human aspect of technological solutions ("stabilizations"), his "post-humanism" is perfectly suited as an ideology of a reformed and politically conscious engineering community, eager to improve the public image of its profession. They would be happy to represent their technical solutions as springing from an enjoyable "dance of agency" with nature, in which the latter's autonomy is fully respected. Pickering's thought is equally suitable as the ideology for Mode 2 science, since it shows such activities as autonomous and not derivative from basic science, as Mode 1 would represent them. Like Latour's thought, it may be viewed as an ideology particularly suited to a period where huge resources are being put into technologies and enterprises located midway between technology and science, such as nano science/technology, supplying a metaphysical stance which could enhance the standing of the players in this field.[56]

Chapter 9
Steve Fuller and Social Epistemology

1. The science studies programme that is the topic of this chapter differs from those we have examined so far in this book. A philosopher and historian of science by training, Steve Fuller operates at a meta-level in relation to the rest of STS's main figures. Rather than illuminating the development of natural science by means of empirical case-studies of his own making, Fuller undertakes a historico-critical survey of the development of STS itself and offers advice concerning its future development. To some extent, his project overlaps with that of the present text; indeed many of my own analytical and critical points are inspired by Fuller's work. On the other hand, he is not a mere outside observer of the field, but very much a player and inside critic of developments in science studies.

The platform from which Fuller surveys the area is one that he himself has been instrumental in establishing. It goes by the name of *Social Epistemology* and constitutes a more general, interdisciplinary enterprise comprising the sociology of knowledge, or elements thereof, as a proper subpart (Fuller 1988/2002, Chapter 1). The points on which Social Epistemology goes beyond typical STS positions are the object of some controversy between Fuller and STS representatives. We shall dwell on some of these disputes, which have a clear temporal dimension: gradually, the gap between Fuller and orthodox STS opens up wider, in step with internal shifts of position on both sides.

2. Social Epistemology carries on the *normative* agenda of classical epistemology (Fuller 1988/2002, Chapter 1, 1993, Chapter 1). To put it in the most general terms, it is the attempt to develop standards of right reasoning and to eliminate sources of error in human thinking. Traditionally, these efforts have been epitomized in the epistemologist's struggle against that perennial foe, the sceptic, and the attempt to show that we are not, cognitively speaking, forever locked up in our individual, solitary minds. This struggle betrays the Cartesian, individualist and subjectivist bias of traditional epistemology. Social epistemology, as the name indicates, attempts to overcome this bias by granting at the outset that cognition not only puts us in contact with external reality, but places us squarely in the middle of a world that is inherently supra-individual and social.[57]

The normative stance of Social Epistemology is of a particularly strong kind (Fuller 1988/2002, p. 24 ff). It is a practical, prescriptive normativity, not a merely

F. Collin, *Science Studies as Naturalized Philosophy*, Synthese Library 348,
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reflective and retrospective one. In other words, it aims at shaping the future course of human cognition and of science in particular, not merely at evaluating it after the fact. Moreover, Social epistemology is *naturalistic*, as the standards of right reasoning are not thought to be demonstrable by a priori means, but are to be discovered empirically. These two features of Social Epistemology are intimately connected. We do indeed have a priori intuitions about how our individual cognition should be undertaken to secure its validity – although such intuitions may offer nothing more useful that the Cartesian demand for "clear and distinct ideas" and similar metaphors, derived mainly from the visual register. Yet we hardly have comparably vivid intuitions about the proper organization of societal institutions of knowledge production; or rather, to the extent we do, they tend to be simplistic projections of our intuitions about the individual case, which may lack any likelihood of implementation in the real world. Our intuitions are typically incapable of divining the effects of material constraints upon knowledge production, in the broadest sense; they reflect a very idealist conception of knowledge, where material restrictions are absent and the cognitive processes proceed without friction. The task of Social Epistmology, as defined by Fuller, is precisely to supply a faithful picture of the social generation of knowledge as constrained by organizational, economic and other material conditions of production (Fuller 1988/2002, Chapter 1).

While it is easy to give a succinct statement of Fuller's aims at a purely programmatic level, it is much harder to pin down his detailed positions with any precision. Most of his writings are somewhat kaleidoscopic; he himself describes his first book, *Social Epistemology*, as "not the usual monolithic monograph", but rather a "parcel of provocations, a sourcebook of ideas, and directions for further research" (Fuller 1988/2002, p. xxx). This aptly characterizes the work in question, but applies equally well to most of his books. This often makes it difficult to establish exactly to what position Fuller sees himself as committed. Contributing to the problem is Fuller's preferred mode of presentation, which proceeds by means of polemics with other figures in the field. Moreover, Fuller's abstract and systematic points are typically only made after a detour through a "potted" rendering of the history of ideas; the issues to be analyzed are depicted as the latest installment of debates with an ancient history. These historical perspectives are often fascinating and betray an impressive learning on the part of the author, but sometimes have the effect of obscuring his principal systematic points.

3. As was the case with the authors we examined in previous chapters, the issue of *realism* in natural science looms large with Fuller. According to a standard conception, scientific realism is intimately tied to the idea of a particular way in which the world *is*, independent of the many divergent ways rival theories have historically depicted it. This "God's-eye view" of reality is typically defined as the terminus towards which science is moving, or, in a hypothetical or even counterfactual version, the direction in which it would move (would have moved) if the strictest methodological precepts were heeded; this is the doctrine of *convergent realism*. The belief in such a uniform direction of development, in its turn, depends among other things upon the assumption that science is cumulative and does not change

course with every major scientific breakthrough. We are not surprised, then, to find Fuller launching an energetic attack upon the idea of the cumulativity of science (Fuller 1988/2002, Part Two).

The Kuhnian doctrine of the incommensurability of consecutive paradigms is usually thought to be the bane of cumulativity and approximation in science and, hence, of convergent realism. The challenge is that scientists working within different paradigms cannot strictly speaking be taken to be talking about the same objects, or to be saying the same things about them, even though they may accidentally employ the same terminology. This is a consequence of the holist semantics for scientific terms embraced by Kuhn, which has it that the meaning of any term in a theory complex is coloured by the theoretical assumptions of that complex and the additional terms in which they are couched. Since these parameters by definition vary between paradigms, identity of meaning across paradigms is ruled out. Thus, scientists are always talking past each other when they address each other from within different paradigms. This predicament excludes long-term cumulativity of scientific results, which presupposes a shared frame of reference.

Philosophers of science have developed a counter to this argument, however, namely, the referential semantics for theoretical scientific terms. This would at least leave scientists before and after a paradigm shift talking about the same things. Referential semantics allows us to disregard the variability and transience of theorizing with respect to a given sector of reality, as long as our linguistic practice is tied to that sector by robust referential links of a causal-communicative nature. Thus, we may grant that the phlogiston theory and the oxygen theory of combustion embed that phenomenon in different networks of background assumptions; yet both are still tied to the very same familiar phenomenon, exemplarily exhibited by a piece of wood going up in flames, by robust ties of referential discourse. Hence, they may be compared with reference to the same reality, and their relative explanatory merits assessed. Or so referential semantics has it.

Fuller sets out to discredit this semantics. His arguments are strictly philosophical and the one upon which he puts most store has it that the knowledge required to establish referential links between a given temporal stretch of discourse and a given entity is often not forthcoming. We may note that the immediate response of the referential theorists to this objection is of no avail here. This is the observation that, since the Kripke–Putnam theory of reference is an "externalist" one, placing the constituents of reference in circumstances external to the minds of the discourse participants, a person may refer to an object although he has no adequate knowledge of that object, or of the communicative ties linking him to it. Indeed, he may do so even though nobody in the society has this knowledge. But the philosophical realist needs to give us more, of course. He needs to show not only that a person *may* refer, but that he actually *does* so refer. He needs to make it at least plausible that the linguistic community in question shows sufficient synchronic coherency and diacronic (historical) continuity to sustain such referential links, which are, after all, quite sophisticated social micro-institutions. It is precisely such continuity that Fuller disputes in a generalized version of the critical objection to referential semantics, to which we turn in the next section.

Fuller is careful to restrict his anti-realism to the entities of natural science, while explicitly excluding history – including the history of science – and social science from its scope (Fuller 1988/2002, p. 65, 1989/1993, p. xiv). We shall see later that reserving a special status for these disciplines involves great challenges, since theorizing in social science seems vulnerable to the same arguments Fuller deploys against realism in natural science. As for history, its status as a low-theoretical discipline might at first seem to save it. However, Fuller himself will later argue that history as normally conducted is methodologically naive and must draw on social theory to fulfill its disciplinary aims (Fuller 1993, Chapter 6). Thus, he gets entangled in problems of reflexivity, since his own theoretical platform is precisely built upon the findings of social science and the history of science.

4. The problems bedevilling a referential semantics are only a special case of a more general problem of diachronic transfer of cognitive content, according to Fuller. This is what ultimately does realism in. It presupposes the existence of a fairly stable, semantically encapsulated "content" to scientific theories, which gets confirmed (or disconfirmed) through history. But this is not the case, according to Fuller. The mechanisms of learning a theoretical language, and the transfer of its cognitive content between generations, are simply too weak to transmit the full contents of a theoretical structure as embodied in the practice surrounding the theory – even disregarding the special vicissitudes of reference (Fuller 1993, Part Two, 1989/1993, Chapter 2). Vaguely recognizing this problem, some theorists of human practice have turned to the story, made popular by Michael Polanyi, that members of a scientific practice are party to an intricate body of *tacit knowledge* animating that practice, a knowledge handed down across generations by virtue of a robust, broadband channel of information transfer sometimes referred to as "apprenticeship" (Polanyi 1951, 1966). Fuller rejects the doctrine of tacit knowledge as largely mythical, especially the notion that such knowledge is uniformly shared in a scientific community. The superficial unity of scientific practice is largely a product of the stable environmental conditions under which the practice is normally carried on. When the environment changes, the consensus will often dissolve, revealing that no uniform tacit principles sustained it (Fuller 1988/2002, p. 216 ff).

A key factor contributing to the fiction of a uniform cognitive basis is the use of a shared linguistic apparatus. Conformity to a particular idiom may hide considerable diversity along several dimensions, according to Fuller, creating what he terms "the elusiveness of consensus" (Fuller 1988/2002, p. 216 ff). In the first place, one and the same idiom may be used to perform very different linguistic acts within science. Philosophers of science recognize only one such kind, that is, *representation*. But there are others, such as asserting instrumental utility or reproducing social values. Moreover, a shared idiom may conceal large differences of methodological orientation in a scientific community that may remain hidden unless external events force them out into the open. Fuller illustrates all these aspects of heterogeneity hiding beneath a homogeneous surface with an analysis of the way in which the surface agreement concerning Newtonian mechanics, with a shared idiom to go with it, covered over immense differences in methodology and metaphysics between Newton's

British and Continental followers. For instance, the French Newtonians would adopt a purely instrumentalist reading of his laws, thus undercutting a realist reading of the overall movement.

Ultimately, the viability of a realist picture depends upon a sufficiently robust sense in which the statements of our scientific progenitors may be translated into our current idiom. Fuller doubts that this is possible. There is certainly translation, but not *one* uniquely correct translation. Any translation must be judged against a number of different desiderata, which may not all be satisfiable at the same time, thus generating a plurality of translations. And none of them may be particularly conducive to the goal of maintaining historical homogeneity. But then, according to Fuller, this is not the function that language evolved to fulfill.

Of course, one might make it the supreme desideratum of a translation that it safeguard the continuity of reference between shifting theories and paradigms. But this smacks of circularity, if invoked as an argument in support of the cumulativity of science. The principle needs an independent grounding. Fuller is willing to concede the force of a transcendental argument to the effect that this policy is a precondition for our understanding the history of science at all; yet this remains an argument about "history" in the sense of historiography, not as referring to the historical events themselves. Narrating history in this Whiggish manner may well be a precondition for our ability to grasp what is told. But the historical events themselves may be supremely unaccommodating with respect to our cognitive needs. History may make no sense to us: it is just "one damn thing after another", in Arnold Toynbee's famous phrase.

An attempt might be made to ground the desideratum upon a deeper, truly transcendental principle, namely, the Principle of Charity, celebrated by Donald Davidson and Willard Van Quine. The connection is established by the consideration that if our scientific ancestors are construed as truly rational, then their reaction to being confronted, in a counterfactual thought experiment, with the evidence that has persuaded us of the falsehood of their theories, would be to admit to error and accept our point of view. This would allow us to construe their theories as antecedents to our own and the concepts used in them as failed attempts to designate the same items in reality; hence, referential continuity would be safeguarded. But even here, Fuller leans towards the position that this principle has no particular epistemic status, but is just one desideratum, to be balanced off against others. To drive home the point, he briefly delves into the sitation in German hermeneutics at the time of Wilhelm Dilthey, where the strangeness of a translation was presicley taken as a sign of its verity, since only such translation would capture the true and genuine strangeness of the foreigners (Fuller 1988/2002, p. 121 f).

Fuller adds another novel twist to this argument by pointing to a so far overlooked source of indeterminacy in translation. This is the role of *silence* in hermeneutics (Fuller 1988/2002, Chapter 6). Should the lack of deployment of a particular concept, or mention of some fact, be taken to indicate that the author did not possess that concept or know that fact? Or, contrariwise, does it indicate that the concept or fact was so much taken for granted that it could be left unmentioned? Very different interpretations will obviously result from either choice. Fuller is sceptical with respect to a final resolution of this issue, reconstruing it, in his preferred fashion, as a matter of different methodological options, each reflecting a different (but legitimate) cognitive concern. An interpretation of a historical text, such as Aristotle's *Physics*, from the perspective of the history of science, would adopt a different methodology than one undertaken within the humanities, such as philosophy. The former will require that the translation support the continuity of scientific development, hence stress similarities, while the latter will stress the aspects revealing that and how Aristotle's project differed from ours. Neither of these approaches can be demonstrated to be uniquely correct. Although the natural science approach has an intuitive plausibility to it – which is the reason for its elevation into a transcendental principle under the name of Charity – it suffers from an internal tension. The less effort it takes to reinterpret an ancient author's project as being really a confused version of our own, the more puzzling it becomes that the author did not cleary and explicitly express the truth he allegedly shares with us. The humanist interpreter tries to capture this difference, which precisely constitutes the distinctive quality of the humanistic approach.

The fact that there is no uniquely best choice between these two translational strategies (or further ones, motivated by yet other cognitive interests) invites another, supplementary interpretation of this issue. Incommensurability is really the methodological aspect of the phenomenon of *indeterminacy of meaning*. It is not as if our object of interpretation – Aristotle or whomever – had a distinctive meaning, either the one reconstructed by the natural science interpreter or by the historian of philosophy, but that his meaning was indeterminate between those two alternatives. Our problem as interpreters is that, looking at the matter with a hindsight sharpened by conceptual distinctions unavailable to Aristotle, we feel compelled to force him in one direction or the other. Our choice will reflect the interests that motivate our efforts at translation in the first place (Fuller 1988/2002, p. 130 ff).

Fuller goes on to tie the discussion of the inscrutability of silence to another interesting phenomenon: *burden of proof* (Fuller 1988/2002, p. 105 ff). To the extent that silence betokens the utterer's (the author's) conviction that explicit mention or argument is not needed, it would reflect the governing societal preconceptions as to which side of a dispute bears the burden of proof. According to Fuller, this constitutes a further source of incommensurability between the belief systems of two communities. For although those communities might comprise identical belief systems as defined solely in terms of the network of inferential and evidential relations between beliefs (their "assertibility conditions"), there might still be a difference in the frequency with which utterers were called upon to actually present that evidence (present the "proof").

The over-all conclusion Fuller draws from this complex argument is that the development of science displays much less consensuality and agreement than is normally assumed, since the theories around which agreement ostensibly accumulates are crucially ambiguous, thus making consensus largely spurious. This fatally undermines a crucial prop of a realist construal of science, that is, the notion that science converges towards a "final theory".

5. Fuller takes no great pains to specify the precise import of his anti-realist position, despite the importance of this issue given the many different positions hiding under that label. He seems to suggest that the existence of an objective physical reality, described in the theoretical lingo of whatever happens to be cutting-edge physics at the moment, is an "objectivity effect" brought about by certain mechanisms inherent in the way science is produced. Objective reality at the level of theoretical physics (in contrast to the one we encounter in everyday experience, we may assume) is a projection from our linguistic and material practices. The fundamental source of this projection – and hence of the objectivity of reality, in general terms – is the resistance we face in carrying out our actions and the difficulties we encounter in trying to predict events. In the case of science in particular, this feature is compounded by the fact that the former is essentially a collective enterprise. Hence, the accidental contributions of other people to our mutual interactions, and their idiosyncratic ways of using the results of our common cognitive efforts, add to this unpredictability.

Fuller draws parallels to the Marxist analysis of *reification* in the economic sphere (Fuller 1988/2002, p. 235 ff). The economy is indeed constituted by human interactions; hence should be transparent to man as a product of his own efforts and intentions. However, due to the ideologically charged nature of economic transactions in the capitalist economy, certain features of it take on an objectivistic appearance. This goes in particular for the category of the commodity. Moreover, Fuller adds a Foucauldian twist to the story, which moves language to the forefront of the argument. The objectivistic illusion is primarily a reflection onto reality of the objectivism with which we tend to regard linguistic meaning, reifying it in a spirit of naive Platonism (Fuller 1988/2002, p. 246). This tricks us into projecting these meanings onto nature as somehow reflecting an objective structure of fundamental entities and laws existing "out there". (Cf. Also Fuller 1989/1993, p. 213, where realism is identified with "hyperrealism", that is, the mistaken projection of a hidden reality behind the pluriform environment with which we are directly confronted.) This is a very radical argument, which actually resembles the argument used by Bloor, on the basis of Wittgenstein's rule-following argument.

Notice that Fuller does not call for the wholesale dispelling of the objectivity illusion. Rather, he wants us to adopt a more enlightened and less naive attitude to it. Like all metaphysical, philosophical concepts, this one has to be translated into a proper sociological vocabulary before we can evaluate its true merit. Once this has happened, we can decide if the concept thus reconstructed is worth saving. In the case of realism, the answer is positive. This emerges from Fuller 1989/1993 (p. 82 ff), which seems to indicate that objectivity and realism are notions with a rightful place, even within a sociological reconstructed conception of science. They serve to articulate a long-term perspective on scientific development, in contrast to its local and short-term assessment. Fuller's over-all concern is precisely to safeguard such long-term and global perspectives upon science, protecting it against exploitation by more opportunistic short-term interests.

This attitude is apparent from the fact that when, in the *Postscript* to *Philosophy, Rhetoric and the End of Science*, he sketches out a future Utopia in which the lessons of Social Epistemology will have been heeded, there is still room for something akin to "realism", properly reconstrued: "Those aspects of tomorrow's world that will be called 'external' [i.e., will be construed realistically] refer to cognitive liabilities, namely, whatever we cannot predict, recall, or otherwise structurally incorporate without great effort" (op. cit., p. 378). I read this as a radicalized version of Peirce's definition of reality as that which resists our efforts at grasping it cognitively, by occasionally saying "no" to the theories we project upon it.

That the resulting stance remains firmly anti-realist in the normal metaphysical sense is made clear from what immediately follows: "The love affair that Western thought has had with the idea of truth as something that us "discovered" or "revealed" finally comes to an end in the world of tomorrow" (ibid.).

6. According to mainstream philosophy of science, the notion of *rationality* is closely linked with that of scientific realism, since reality is defined as the ontological double of the world picture generated by sustained use of rational procedures. Like realism, Fuller does not simply reject this notion; equally like realism, he accepts it only in a sociologically reconstructed version. The traditional conception of rationality is dismissed as intuitions that reflect nothing more solid than innate cognitive heuristics. Rationality, properly reconstructed, can only mean the practical, means-end efficacy of science in producing the results we demand from it. These results Fuller conceives more broadly than traditional epistemology, which considered the attainment of truth to be the sole goal. Fuller's conception of rationality, however, does not imply that the normative stance of Social Epistemology is limited to assessing the instrumental efficacy of science with respect to pre-given goals. On the contrary, the deepest ambitions of Social Epistemology is to critically scrutinize the very goals of science (Fuller 1989/1993, p. 211 f).

With rationality and realism out of the way, one of the other favoured conceptions of classical normative philosophy of science falls as well: this is the notion of the *internal history of science* (Fuller 1989/1993, Chapter 2). This is precisely a highlighting of those episodes through which science allegedly effected its ever closer approximation to the truth. According to the traditional story, those episodes result from the successful shielding of science from external, societal influences.

It follows from Fuller's argument that the idea of an internal history is a myth. The dubious nature of the enterprise has been covered up by philosophers by a number of rhetorical tricks, the most efficient of which is the clever oscillation between a genuinely descriptive, historical approach and a purely normative one. When the philosopher fails to discern his preferred pattern of rational thought in a sequence of historical events in which he expected to find it – that is, in developments that all are agreed to regard as progressive – he resorts to normative sermonizing, covering over the gap in the actual train of events with a little story about how science should properly have been conducted to achieve the goal which was, fortuitously, a progressive one, as we realize with the benefit of hindsight.

There is a particularly devious version of this strategy, devised by Lakatos, who resorted to *counterfactual* history (Lakatos 1971). Lakatos not only prescribed how history ought to have developed, but actually postulates that history would have taken that route, had it not been for the interference of particular, corrupting influences of a social nature. In this way, the rationality of science is safeguarded at the same time as the integrity of philosophy of science as an autonomous discipline is upheld.

7. Fuller sees a crucial role for the sociology of science and other branches of science studies as contributors to Social Epistemology. This does not mean, however, that he is uncritical of what goes on under this label. On the contrary, he subjects it to the kind of rebuke reserved for those we love. As we shall see, this love would later gradually turn sour.

Fuller sees the sociology of science as having moved forward in a sequence of waves (Fuller 1988/2002, Chapter 1). The first wave, encompassing developments transpiring between the World Wars, represented the production of scientific knowledge as tightly controlled by social interests. In so doing, it failed to draw a crucial distinction between the *motivators*, *benefitters* and *users* of knowledge and to recognize that the last-mentioned have considerable freedom with respect to the conditions that constrained the two first-mentioned classes of agents. Failure to appreciate this point leaves the sociology of science vulnerable to what Fuller calls the *Strong Objection to the Sociology of Science* (Fuller 1988/2002, p. 239): its inability to account for the way in which scientific results are happily deployed by "consumers" who have widely divergent social interests. The overly strong focus upon the producers is falsified by the ease with which scientific knowledge is appropriated by people with radically different concerns (op. cit., p. 12 f). This weakness has been partly rectified by the New Wave of sociologists of scientific knowledge, but they still overestimate the degree to which knowledge is under the control of its producers, thus still leaving the New Wavers open to the Strong Objection to the Sociology of Science.

Fuller's own response to the Strong Objection is derived from his analysis of science as much more loosely coupled than is normally taken for granted. This points to the crucial mistake in classical sociology of science and, at the same time, to the way in which the Strong Objection can be countered. Classical sociology of science, and its followers within the Strong Programme, adopted a far too monolithic view of scientific theories, taking their homogeneous rhetorical surface to betoken a genuine underlying unity. Once the hidden heterogeneity of scientific theories is recognized, it is no surprise that widely different groups of clients could find something of interest in them. Adopting a biological metaphor, we may compare disciplinary knowledge to a biological species, whose phenotypes show considerable surface similarity. However, this surface likeness may hide considerable genotypical variety, making the species adaptable to other, different econological niches than the one in which it originally evolved. The same goes for scientific knowledge. Its surface form hides a heterogeneity that makes it easily adaptable to uses not envisaged by its original producers. (Fuller makes a similar comparison in Fuller 1989/1993, p. 105.)

The post-Edinburgh STS faction has come much closer to seeing through this facade of science as merely rhetoric and to do justice to the multiplicity of interests involved in knowledge circulation. This is primarily the theory of ANT, from which early Fuller shows a considerable influence. Here, a scientific theory is presented as the surface of a highly complex network of stakeholders – or, elaborating upon a favourite metaphor of Latour's, the clearly delineated mushroom that, however, is just the surface manifestation of a vast and not clearly defined mycelium. This network embodies the true underlying multifariousness of a scientific theory, since each node in the network interprets the latter in a slightly different manner, in light of its particular interests.

8. Fuller also refuses to follow current sociology of science in its hostility to the psychology of science. We found the latter attitude manifested in the Stong Programme's neglect of such research and saw its strongest – although somewhat lighthearted – expression in Latour's proposed "moratorium upon psychology of science". Fuller, on the contrary, allows a place for the psychology of cognition. In particular, for dialectical purposes, he has a use for psychology, to demonstrate the shortcomings of man as a solitary reasoner. Indeed, *Philosophy of Science and its Discontents* is a sustained attempt to find a strategy for integrating various naturalist approaches to science. It is clear that psychology has a secure place, although only as partly transformed by a sociological perspective (cf. op. cit., p. 213 f; cf. also Fuller's contributions to Shadish and Fuller 1994). Fuller has even sketched out a particular research project in the psychology of science (Fuller 1989/1993, p. 112 f). He makes it clear, however, that he prefers to reduce psychological findings to sociological ones (ibid., p. 5).

Leaders among cognitive psychologists who in particular have examined man's proneness to error are Tversky and Kahneman (1983). The results have been summed up by Ross (1977), and Fuller offers a brief abstract of the list, which comprises the following errors: Subjects confirm where they should falsify; they ignore the base-rate probabilities essential for Bayesian inference; they fail to see how sample size affects statistical reasoning in general; they cannot conceptualize causes whose interaction brings about an effect; they erroneously take the ease with which they remember something as an indicator of the extent to which it represents their experience; they do not make consistent expected utility assignments (Fuller 1989/1993, p. 106).

Fuller makes the astute observation that these results tell equally against those versions of the sociology of science that depict science as shaped by participants' interests (ibid., p. 106). Or rather, they tell against the possibility of refining such accounts into theoretical explanations with strong explanatory power, based upon rational choice reasoning. Such reasoning presupposes the expert calculation of probabilities as inputs to the expected utility models that supposedly drive the selection of theories (cf. ibid., p. 148); but such complex calculations are simply beyond the cognitive powers of the agents in question.

Sociologists of cognition have not taken kindly to this line of psychological research. The over-all theme of the sociologists' response to the psychological findings is clear. It is that the analysis of human thinking through the lens of the individual, isolated thinker is artificial: Man is primarily a social thinker, who does his cogitations in collaboration with others and always in a concrete social setting.

This setting has to be supplied in experimental research, if we want to do justice to "man the cognizer". Somewhat ironically, philosophers and sociologists have joined forces in their rejection of these psychological experiments. In particular, Fuller presents the criticism directed by L. Jonathan Cohen (Cohen 1986) against the realism of these psychological results, criticisms designed to save the dignity of man as a reasoner.

9. The philosophers' reaction to these subversive psychological findings inspires a brief digression from Fuller's story at this point. The demonstration that man's cognitive rationality is much more fragile than we like to think, and that he is altogether rather flawed as an individual cognizer, is grist to the mill of a more socialized conception of human thinking and was so taken by many sociologists. Yet although these findings provoked a reaction from a few philosophers, as we have seen, they did not generate anything like the recriminations unleashed in the Science Wars and in the accompanying debates between philosophers and the STS faction. (We shall return to these events in the next chapter.) I take this to support the interpretation of the underlying dynamics of these debates that inspires the present work: The difference is that the subversive psychological findings were not deployed to support the call for a radical reconstrual of science in the direction of a more "socialized" conception, along the lines recommended by STS. The entire research effort was undertaken within the framework of a perfectly conservative understanding of science; no attempt was made to draw conclusions from it that would undermine the mainstream conception. Hence, the ensuing debates between philosophers and the agents of this particular exercise in "naturalized epistemology" could be handled as a local controversy; there was no interest on either side in elevating them to the status of a Science War fought out in a global academic arena.[58]

10. Now, back to Fuller. Fuller considers all the mentioned criticisms of the subversive psychological results to be misguided. The psychological findings are not the outcome of faulty experimental design and should not be patched up to sustain our cherished conception that man is rational after all, alhough only in his natural social setting. The proper conclusion is rather that the individual is not the producer and subject of scientific knowledge at all: the collective is. The proper subject of scientific knowledge is not the individual, but a social entitity of sorts, such as the research collective; ultimately it is society in its entirety. This is a conclusion that has already been accepted by some philosophers (for instance, Popper) for partly overlapping reasons.

Yet this move poses the following crucial question, according to Fuller (1989/1993, p. 128): If society is the real subject of knowledge, what then is its "knowledge display"? How can one determine what society knows? As long as we are dealing with an individual cognizer, the answer is easy: we just ask that individual; or, if we mistrust his words, we observe his behaviour. But how do we establish what a society knows, as different from what is members individually know? Fuller argues that the best option is probably the citation and reference patterns of scientific articles. But he stresses that such patterns are also shaped by the rhetorical aim of marshalling support for scientists' positions, and would probably not very adequately reflect what would be considered established knowledge in the smaller cognitive arenas of which society is made up. Thus, there turns out to be no good answer to this question.

This shifts focus back upon the individual cognizer again and reinstates a role for psychology. Remember that, for Fuller, the normative assessment of science, including its rationality, is not primarily a matter of handing down marks for past efforts, but of establishing principles for an improved organization of science in the future. This shift of perspective is crucial, according to Fuller, since it makes it possible to draw a distinction that deflects the potentially undermining effects upon science of the rationality studies. This is the distinction betweeen the *ecologial* and *external* validity of experimental studies (Fuller 1989/1993, p. 176). Ecological validity is a matter of the *representativity* of the experimental studies with respect to situations outside the laboratory, while external validity is a matter of the *reproducibility* of experimental effects in relevant situations outside the laboratory.

This distinction makes it possible for Fuller to concede that these experiments may have little ecological validity, but insist on their external validity instead. That is, the experiments show us how we may rearrange the conditions under which scientific work is carried out so as to safeguard its rationality. The sociological transformation of psychology advocated by Fuller will make it a tool for the modification of the social and other environmental conditions under which man carries out his scientific thinking, thus improving his performance. According to this formula, psychology would take on the logical structure of "action research", in which the researcher actively modifies reality and records how interventions create new phenomena; or, more precisely, he checks the efficiency of interventions with respect to generating specific desired outcomes. (This is *intervention* rather than *representation*, in Hacking's terminology, cf. Hacking 1983). Fuller succinctly expresses his strategy in the motto that he sees the experimental method "more as a means for *macro*-reproducing the lab in the world than for *micro*-reproducing the world in the lab" (Fuller 1993, p. 190).

We may note in passing that Fuller has sufficient respect for a purely cognitive approach to science to envisage the possibility that theory-generating computers may someday assume the same role that other prosthetic devices possess today, as a means of extending the range of the human senses. These instruments would be trusted to the same extent that we trust what microscopes and radio telescopes tell us – or, perhaps as a closer analogy, what our computers tell us to be the result of complicated mathematical calculations (Fuller 1989/1993, p. 142).

We have now drifted into an examination of the instruments by means of which science could be improved. This is, indeed, a major concern for Fuller, as we have seen. Before we turn to examine his preferred instrument for this endeavour, we must round out our discussion of Fuller's disagreements with current STS.

11. I have argued that STS always had an implicit normative agenda, namely that of rendering science more democratic and of establishing a fairer accommodation between science and society, but that this normative goal has gradually moved to the forefront in step with the demise of the original "neutralist" strategy

for pursuing it – that is, revealing science's all-to-social nature through empirical science studies. For reasons both intrinsic and rhetorical, the early Edinburgh School took this strategy to imply the adoption of a stringent methodology, including a strongly upheld scientific neutrality. But, of late, STS has turned explicitly normative and, indeed, openly political.

Fuller construes the situation slightly differently, in a manner that I would argue overlooks the strategic nature of STS's original methodological neutrality. For Fuller's deepest grudge against the sociology of knowledge, at least as it appeared in 1988 when *Social Epistemology* was published, was its rejection of the normative agenda of classical epistemology. STS staunchly declined to adopt a critical attitude to science, refusing to use the knowledge it had aquired to change science. But with its growing insights into the mechanisms of the knowledge-making process, STS'ers faced ever more strongly this natural challenge: How should it conduct itself in light of what it learned about science? (Cf. Fuller 1993, p. 9.)

There was clearly a strong incentive within the original Strong Programme to suppress this issue, however. The academic credentials of the work undertaken within the programme, and hence the credibility of the picture of science emanating from it, were critically dependent upon the assurance that the programme was undertaken according to the most stringent criteria of scientific procedure. Open commitment to a concrete political agenda aiming at the transformation of science might easily compromise this assurance. This predicament could only be aggravated, of course, by the Strong Programmers' insistence that scientific results are inherently shaped by underlying interests. Hence, any claim from Strong Programmers to the effect that they had achieved a strict insulation of their theoretical work against biasing influences from their practical agenda would immediately make that programme "a standing refutation of its own principles", to quote a key phrase from the Reflexivity Condition. I have argued that this is the locus of an ineliminable tension in the Edinburgh approach; no wonder that Strong Programmers did not want to direct attention to it by promulgating an explicit normative agenda.

In later STS work, the neutralist stance was typically strengthened by the adoption of epistemological relativism; yet the underlying argument was already inherent in the "philosophical turn" later undertaken by the Strong Programme. A main source was the Wittgensteinian doctrine that science is generated in isolated enclaves of human practice, each of which defines its own "way of life". Social science, like philosophy, has no business adjudicating these different ways or preferring one to another; for there is no Archimedian point from which such judgements could be made. Thus the sociology of science, like philosophy, leaves the world as it is. The arch-villain according to this attitude is the kind of elitist, a priorist epistemology represented by Plato, which claims to occupy a privileged epistemic platform outside of space and time from which to assess all the different historical ways in which societal knowledge has been generated. This leads to a general wariness of expertise and scientific authority, which leaves little scope for a prescriptive stance with respect to science, beyond a general call for it to be more permeable to societal interests.

Although Fuller is entirely sympathetic to this anti-elitist stance, he is still critical of the relativist, neutralist attitude that is taken to follow from it. While this attitude prides itself on its sociologically sophistication, it actually embodies an outdated sociology that is inadequate to the modern world. Fuller agrees with STS that science is produced under conditions of only local rationality, but he directs attention to the fact that, in our new global world, it is increasingly being "consumed" in a much more extensive market, where non-local concerns must be satisfied. This calls for the adoption of a more general perspective for the assessment of science.

Fuller concludes that STS has no excuse for shirking its responsibility to use the knowledge acquired to improve the conduct of science, and the goods that flow from it. The neutralist attitude is impossible to justify; among other things, it overlooks (or suppresses) the insight that doing nothing is itself an act with morally relevant consequences.

12. The stage is now set for Fuller's own suggestions as to how the normative epistemic enterprise should be conducted. These diverge radically from what we get from traditional epistemology. In the first place, the approach is *naturalistic*, to an extent to which the inchoate naturalistic efforts within analytic philosophy have not yet approached. Next, traditional epistemologists have proceeded on the assumption that they knew beforehand what the goal of knowledge is. The idealization under which classical epistemology has operated extended even to the nature of the goal of human cognition. That goal is *truth*, typically construed as correspondence with reality. But Fuller insists that we cannot take this goal for granted. We cannot assume, prior to examining each concrete case, that truth is really the goal. Or rather, we cannot assume that the "truth" ostensibly sought is what we understand, today and in our culture, by that term. An examination of the criteria that counted as conclusive grounds for accepting something as "true" in a given culture, in a given epoch – in which, for example, fidelity to holy scripture carried a decisive weight – should suggest to us that knowledge may have served other functions than it fulfils in our current society. Rather than dismissing our ancestors as poor reasoners, we should conclude, or at least entertain the possibility, that they were after other game than we are.

Fuller comprises these points about knowledge in what he terms the "panglossian" conception of knowledge (Fuller 1988/2002, p. 26), which might also be called a construal of (communal) knowledge observing the Charity Principle to an extreme. According to this conception, we live in the best of all possible worlds, which means that apparent incoherencies in past or otherwise foreign methods of knowledge production are merely the sign of our failure properly to grasp the nature of the goals that were pursued.

This liberal, contextualist construal of the concept of knowledge is tied to a criticism of a core element of our received conception, namely that knowledge, especially scientific knowledge, is a body of doctrine that human beings "possess" and carry around in their heads. Here, Fuller shows traces of Rorty and his criticism of the idea of the mind as a "Mirror of Nature" (Rorty 1979), and of the goal of cognition as sheer representation. As we saw in Sections 2 and 3, Fuller finds fatal flaws in this conception – flaws grave enough to push him to the conclusion that the idea of a homogeneous, yet socially distributed, content of scientific theories is a myth. This criticism opens up for a broader, socialized conception of (scientific) knowledge, according to which having knowledge is simply a socially ascribed status, to be earned by a person in the course of participating in various epistemic social practices. This definition of knowledge obviously also ties in well with the demonstration that man is highly flawed as an individual cognizer.

Fuller articulates his alternative conception of knowledge in the following principle:

> A producer "has knowledge" if enough of his fellow producers either devote their resources to following up his research or cite his results as background material for their own. . . . Thus, having knowledge is ultimately a matter of having *credibility*.
>
> (Fuller 1988/2002, p. 30)

We notice the Latourian flavour of this definition, which is of a piece with (early) Latour's analysis of science as an institution in which academic credit in constantly generated and circulated. Science is the ultimate manifestation of knowledge as credit, with its elaborate system of accreditation, such as degrees, prizes and citation counts.

13. Fuller's normative approach is particularly ambitious, as we observed in the first section, since its goal is to govern science (i.e., to change its course). This calls for reflection on the instruments available for this project, an issue that is particularly pressing when seen against the background of the determinist picture of science presented by mainstream STS'ers. They see science as shaped by either class interests (the Marxist tradition), or deep-seated cognitive frameworks (the Durkheimian tradition).

In our examination of the Strong Programme, we observed that this deterministic macro-level picture was softened by occasional indications that interests of a more varied and local nature were also at play. Scientists' concern for their reputations was allowed among the interests forming science. This might make science manipulable in terms of the judicious distribution of funds to selected projects, with the prospect of academic credit motivating the beneficiaries.

As it happens, Fuller does not recommend this strategy, at least not as his primary tool. True, he puts great store on the economic infrastructure of science, but a policy of centrally controlled apportioning of funds is apparently too top-down for his taste.[59] He does, however, sketch out a model for the reorganization of university research that uses budgetary instruments as its chief tool, but still leaves uncertain whether or not he would actually try to implement this model with its strongly top-down structure (Fuller 1993, p. 52).[60] Instead, he advocates *rhetoric* as the primary tool for the direction and reorganization of science, making it the topic of an entire monograph (Fuller 1993). The rhetoric he has in mind is not the one that has become influential in recent times and which focuses upon the ornamental, expressive side of speech and writing. To Fuller, this is just a decadent form of a old tradition, best represented by Socrates and his critical questioning of received wisdom in the Athenian agora. The rhetoric he has in mind distinguishes itself from pure logic, used as a critical instrument of argumentation, mainly by its endeavour to establish an *ethos* – that is, to adopt a platform on which a *rapport* with the intended audience can be established. In so doing, the rhetoretician distances himself on one hand from the philosopher, who elevates himself to a transcendent position from which he addresses people in monologue and on the other hand from the post-modernist, who insists that all points of view that pretend to more than local validity are bogus. The point of view to be promoted by rhetoric is thus *global* without being *transcendent*. Fuller coins a term for the kind of science that is to emerge from this policy: It is *prolescience*, the result of the "proletarianization" of the process through which science is produced – that is, its democratic transformation (Fuller 1993, p. xviii).

Fuller's confidence in the possibilities inherent in a rhetoric of science reflects two pivotal aspects of his understanding of science. The first is captured by a distinction between what he calls "Deep Science" and "Shallow Science" (op. cit., p. 12). The former depicts science as being produced by experts drawing on specialized knowledge, making extensive use of instruments and relying on a disciplinary tradition internalized through training. The latter sees scientific knowledge as a more distributed phenomenon, gaining its authority from extensive networks of users who find it useful in terms of their own widely divergent purposes (there are echoes of Actor Network Theory here).

These two conceptions of science go together with corresponding conceptions of the language in which scientific discourse is conducted. Somewhat paradoxically, the Deep Conception of science goes together with a Thin Conception of scientific discourse, since language is thought to be incapable of encapsulating the knowledge that constitutes scientific method (a knowledge that is hence tacit). Conversely, the Shallow Conception of science is tied to a Thick Conception of language, which attributes to language a genuinely constitutive role as the medium consolidating the social relations (the network) that constitute, or produce, science. What makes this language "thick", according to Fuller, is precisely rhetoric, which marshalls the tools for this social process. Thus, his book on the rhetoric of science is an exercise in the "thickening" of language.

Fuller seeks to develop a rhetoric that does not suppress the voicing of disagreement; indeed, rhetoric should encourage the articulation of disagreement about goals, thus counteracting the tendency of all epistemic discussions to be flattened into discussions about means. We need an *axiology* of science, not merely a *methodology* (Fuller 1993, p. xviii). Considered in a more narrowly scientific context, this is a matter of establishing interpenetration, that is, to dissolve disciplinary boundaries and democratize knowledge production. All "philosophically deep" problems generated by the sciences are the result of dysfunctional communication habits (ibid., p. xx).

If one's goal is to expand the networks in which science is produced and assessed, as it is Fuller's, then the tendency towards *disciplinarity* constitutes a major obstacle. Fuller believes that the scientific enterprise naturally tends towards isolating the practitioners of science in tribes that claim a special prerogative with respect to a particular segment of reality, made up of its specific natural kinds. But in Fuller's opinon, these apparently objective (meaning "out-there") sectors of reality are really just the effects of, that is, projections of, communicative borders. They largely reflect lines across which no communication takes place. Thus, the keyword for his normative approach is *interdisciplinarity*. Fuller believes in the virtues of interdisciplinarity – not as the incidental intellectual overflow, as it were, of activities that are securely grounded in their own disciplines, but as the very *modus operandi* of science (Fuller 1993, p. 33).

The drift towards disciplinary insulation is a predicament that calls for the rhetorician to intercede, since rhetoric, properly understood, is the art of facilitating communication (ibid., p. 19). Fuller sets the rhetorician's distinctive approach to language and communication against the philosopher's and that of the late-generation constructivist STS'er. Philosophers dismiss the very problem that rhetoricians attempt to solve, that is, the failure of people to communicate. In their idealized realm, a universal basic language is available in which all meaningful issues can be discussed and resolved, independently of the interests of the parties. STS'ers, on the other hand, hold that members of diffent paradigms (or "ways of life") are so deeply embedded in their respective languages and conceptual structures that communication is ruled out beforehand. The rhetorician, finally, recognizes the challenge posed by different basic standpoints, but still tries to overcome it in order to join in a common cause (ibid., p. 19).

Fuller does not have much faith in the possibility of changing scientists' ways by merely telling them to do so. This approach would presuppose that scientists have a reflective insight into their actual practices; such is normally absent, however (cf. Fuller 1989/1993, p. 191 ff). Still, Fuller is optimistic with respect to the scope for rhetorical efficacy, an optimism based upon a feature of his social construal of knowledge that we have mentioned already: knowledge is not so much an intrinsic state of the individual, a state of "belief", as a matter of the credibility and authority publicly attributed to that individual. This state will be more amenable to rhetorical manipulation than a state of mind. The status of knowledge as a public phenomenon is closely tied to the phenomenon of *orthodoxy*, which in its turn is tied to the notion of burden of proof. Fuller points out that public belief (i.e., orthodoxy) is different from the preponderance of private beliefs; indeed, the two may diverge radically. This possibility is well-illustated by the hypothetical situation in which a majority of scientists privately harbour religious beliefs, while still maintaining an orthodoxy according to which such beliefs could never be the official doctrine of the scientific community; the strictures of scientific method condemn them to being forever private and non-scientific. In accordance with this insight, Fuller sketches out a rhetorical strategy by which one does not move by direct persuasion, but by changing presuppositions of thought. These serve to shift the burden of proof that is needed to present and defend a thesis in the public arena of science.

Fuller goes into some detail concerning the role of the rhetorically oriented STS'er in the process of furthering interpenetration and interdisciplinarity in science. A preferred role is that of the *facilitator*, which is defined in contrast to the *negotiator* and the *arbitrator* by its modesty (Fuller 1993, p. 312). This feature is captured in two principles, which according to Fuller should govern the facilitator's efforts:

> 1: *The Principle of Reusability*: When trying to get someone to change her ways, avoid tactics that are nonreusable, or are likely to wear thin over time. . .
> 2: *The Principle of Humility*: The person whose ways you are trying to change may have good reasons to resist your efforts, which, given the opportunity, she could tell you and which would perhaps even change *your* mind. . .
>
> (Op. cit., p. 316)

14. The presentation of Fuller's position so far has been based upon three foundational early works (although with later editions). These show considerable homogeneity, apart from smaller discrepancies that I have passed over here. Yet, in later works, Fuller gradually moves away from his original positions on certain points. In particular, rifts have opened up between Fuller and authors who played a large and positive role in his earlier work. This is epitomized in Fuller's attitude to Thomas Kuhn and Bruno Latour.

We saw that the Kuhnian notion of incommensurability formed a crucial prop of the anti-realist, constructivist platform, from which Fuller launches his Social Epistemology. True, Fuller offered a sociologico-rhetorical reinterpretation of incommensurability, to replace Kuhn's own internalist-semantical one; moreover, from the outset, Fuller saw the lack of communication associated with incommensurability as a problem. Still, incommensurability played a major constructive role as a premise in Fuller's edifice and his attitude to Kuhn was correspondingly favourable, on the whole. Thus, we read that "Kuhn's overall impact on the academy has been more liberating than inhibiting" (Fuller 1988/2002, p. 9).

Fuller soon came to see the balance between pros and cons as tilting in the opposite direction, however. And to the consternation of most of his earlier readers, Fuller launched a harsh attack on Kuhn in the book *Thomas Kuhn. A Philosophical History for Our Times* (Fuller 2000b). Rather than just the discoverer of incommensurablity, Kuhn is now exposed as a major contributor to its prevalence. Moreover, he is seen as someone who, through his doctrine of paradigms, helped pre-empt a critical attitude in academia towards Big Science. The doctrine of paradigms and normal science with their depiction of science as largely insulated from influence from society at large is easily co-opted for the promotion of a policy whereby science and society abstain from interfering in each others' affairs. A *quid pro quo* is tacitly established, whereby society respects science's autonomy in return for science turning a blind eye to the sometimes questionable uses to which scientific results are put. Thereby, an opportunity was missed to alert the community to the dangers inherent in the emergence of Big Science and of the military-industrial complex. Instead, social science – and the sociology of science, in particular – adopted a strictly neutral, professionalized attitude to the phenomenon of Big Science. Fuller even describes Kuhn as an icon of the Cold War.

Fuller's criticism extends to aspects of Kuhn's historiography. Kuhn is normally celebrated as the main instigator of the "historical turn" in the study of science and is praised for his detailed historical case studies. While respecting Kuhn's purely technical work in the history of science, Fuller insists that the historiography of *Structure* is deeply flawed. Kuhn mixes up different periods. His celebrated model of scientific development is jerry-built out of cases culled from different periods, in such a manner that certain aspects are found only in one period, whereas others are only found in other periods. The result is that no single historical example can be found of a sequence of events showing Kuhn's full cycle of normal science-revolution-normal science. Kuhn's celebrated model is empirically inadequate.

A related flaw in Kuhn's analysis of science is its striking failure to apply to the period in which it was written. Kuhn's model is mainly internalistic (although not in the philosopher's rationalistic sense), and hence fails to capture our current era of Big Science, where the course of science is mainly dictated by powerful external stakeholders. Fuller construes this as discretion with regard to science's foremost financer, that is, the military.

Perhaps worst of all, Kuhn advocated a doctrine of "double truth", the historical roots of which reach all the way back to that arch-conservative elitist, Plato. Kuhn recommended that the members of a scientific discipline be fed a carefully reconstructed, Whiggish version of their discipline's past, which made it look as if all of prehistory inexorably led up to its present stage. All the false starts and contingencies in the history of the discipline were to be carefully censored out.

15. With regard to Fuller's attitude to Latour, there are numerous points of agreement between the two, and Fuller's early work shows considerable Latourian influence. Indeed, the "Postscript" to *Philosophy, Rhetoric and the End of Knowledge* reads like a veritable Latourian manifesto. We are told that the special sciences represent a spatial social structure, with the validity of their results extending only as far as their spatial reach; that science is stronger the more intimately it is embedded in a social network, where conventional philosophical wisdom puts its strength in its assumed isolation; that any scientific truth can be upheld, if sufficient resources are mobilized on its behalf, and that the proper task of the sociology of science is to compute a balance of the costs of such mobilization and who bear them; that the conception of humanity will change as the distinction between human and non-human producers of science becomes blurred; and that science as it actually happens can only be captured at the micro-level of detailed social studies, whereas all abstract explanation falsifies the phenomenon. Fuller also shares Latour's hostile attitude towards Platonism, which is seen as the ultimate elitist and hegemonic stance within epistemology. He also originally shared Latour's idea that "representation" means much the same thing in science and politics (Fuller 1988/2002, pp. 36–45).

But a gradual fall-out has taken place. We saw previously how Fuller criticized STS for its neutralistic attitude towards the study of science. Already in his earlier works, Fuller occasionally hinted that this attitude might reflect opportunism: it was a marketing strategy, highlighting the product's usefulness to all potential buyers (Fuller 1993, p. 301). In time, Fuller has increasingly come to see Latour's work as motivated by precisely this aim, or at least as assuming such a role. In step with this, he has adopted an ever more critical stance towards Latour (Fuller 2000b, p. 365 ff).

Fuller diagnoses ANT as an accommodation to the neo-liberal world order that has emerged during the three decades in which STS has existed. The first generation of STS'ers received formative impressions from the spectacle of science as a servant to the "military-industrial complex". This was the classical epoch of "Big Science", in which science appeared very much a part of the Establishment and, hence, a proper object of censure. Military pursuits favoured investment in basic research, the results of which were supposed afterwards to trickle down to practical (i.e., military) applications, as we saw in the analysis of Kuhn, above. This organization of science protected basic research, at the price of silencing any reservations scientists might harbour concerning the applications to which their research led.

With the advent of the neo-liberal world order, this organization of science, previously seen as ideal, has been cast as really a passed-over stage, tagged with the label of "Mode 1"-science. The thing of the future – but a future already at our doorstep – is that of "Mode 2". Here, science is not produced according to the so-called linear model, which has basic science come first and later trickle down to serve practical applications (hence, the "cascade model" might be a better metaphor), but is produced in close collaboration with – and at the instigation of – interested clients. Latour's ANT-model is well suited to capture this feature. Moreover, with the passing of the "military-industrial complex" (at least from the newspaper headlines), the need for a moral stance with respect to the products of science appears much less pressing; indeed, with the modern economy geared to satisfying consumer demand on a global scale, it becomes much easier to see growth in production as an unqualified good, or at least as something on which there is little point in passing judgement. Mode 2 is thus associated not only with a democratization of the creation of scientific results, but also with a wider distribution of the fruits of scientific labour.

Thus, ANT is ideally adapted to a neo-liberal world order, in which universities are increasingly run like businesses: the widely heralded "entrepreneurial universities". More precisely, it is the perfect ideology for people who work under the labour conditions generated by this new world order. Here, research jobs are increasingly short-term and researchers must increasingly go hunting for new private contracts or short-term government grants. Hence, there is need for a "user-friendly" approach carefully purged of critical elements that might offend potential patrons. Fuller repeatedly presses the point that, in failing to own up to this situation, STS has carefully avoided applying to itself the tools with which it has so successfully revealed the interests behind other scientific programmes.

Fuller has also distanced himself from the political implications Latour draws from the ontological premises of his argument. At the outset, Fuller and Latour were agreed that from the point of view of analyzing and explaining knowledge and its production, human beings have no privileged role. Latour expressed this point in his celebrated theory of actants, while Fuller followed suit with an analysis of knowledge that explicitly undercut the traditional mentalistic tenor of epistemology which construes knowledge as a state of mind ("belief", in the traditional three-part definition of knowledge as "justified true belief"). According to Fuller, knowledge is not essentially a matter of belief, but of action. Moreover, it is essentially distributed, not only among a plurality of persons, but even in texts, computer programmes, and instruments (Fuller 1988/2002, p. x f).

We saw in previous chapters that Latour (and Pickering) went on to draw quite strong conclusions from this ontological premise, reaching into the domain of political philosophy. Indeed, Latour's latest writings look like a bid to revolutionize our political institutions. Fuller seemed to think along similar lines in earlier works (cf. Fuller 1993, p. 342, 1989/1993, p. 134). However, he has increasingly come to reject those conclusions, in the face of the potential anti-humanist consequences of this attitude, especially when combined with STS's lack of scruples with respect to whom it picks as its allies. He also objects to the lack of an explicit argument to get from the ontological foundations of Science Studies to the normative-political implications drawn by Latour. The traditional, strict separation between human and non-human is indeed ripe for elimination, according to Fuller, and the ethical implications of such a move must be faced. Among current philosophers, Peter Singer has taken on this task. Fuller's complaint is that Latour fails to take such work into consideration, or to develop his own thoughts along the same lines (Fuller 2007, p. 217).

Through Fuller's criticism of Latour rings a tone of disappointment that we also encountered in Bloor, a sentiment only worsened by Latour's immense impact both inside and outside the academy. As Fuller makes it clear in his book *The Intellectual* (2005), he himself aspires to being influential outside his own specialized field, and even in the broader society as a "public intellectual". Indeed, this ambition follows directly from the ideal of the rhetorician, as delineated in *Philosophy, Rhetoric and the End of Science*. Yet in this respect, his own impact has been overshadowed by Latour's supremely efficient rhetoric which, in the eyes of many academics, has established him as an heir to the illustrious line of French intellectuals such as Zola and Sartre. As Fuller sees things, Latour's efforts actually run counter to the humanism inspiring those figures; moreover, he works mainly with rhetorical strategies that Fuller would consider illegitimate, including liberal use of jokes and puns. But some of his tools would actually have to be considered legitimate by Fuller's lights and one of them would even be especially commendable: the shifting of the burden of proof from one side of a debate to the other. This happens, for instance, when Latour challenges our intuitive and unreflective presumption of a strong difference between humans and non-humans, forcing us to present an argument that the difference is indeed real, and significant.

With his latest ventures into politics, such as in *The Politics of Nature*, with its rather blunt attempt to ally ANT with the popular ecological movement, Latour has even taken a step in the direction which Fuller recommends for science, that is, to transform itself into a social movement (Fuller 2000b, Chapter 8); Fuller has earlier urged us to see philosophical schools as social movements (Fuller 1993, p. xvi). This is the opposite of the Kuhnian model of science as a highly confined, professionalized activity taking place in isolation from broader social concerns. A social movement, on the other hand, is socially highly permeable and is a direct expression of such concerns.

The rift between Latour and Fuller also reverberates in a shift in Fuller's attitude to the universities with their celebrated autonomy. Earlier, Fuller held this autonomy to be largely mythical, and, to the extent that it was real, mainly harmful; it was essentially a guild privilege of academics. With the increasing pressure upon universities to transform themselves into "entrepreneurial" institutions promoting industrial innovation, Fuller has come to appreciate the value of upholding certain barriers between the universities and society at large (e.g. Fuller 2001). This, of course, is an effort that goes directly counter to STS'ers' attempt to dissolve the boundaries of all (knowledge-producing) social institutions and make them directly responsive to the interests of all actants. Unfortunately, the latter agenda is easily high-jacked by societal powers intent upon enrolling the university in a top-down knowledge policy. Fuller finds that people educated in STS have shown themselves as too-willing instruments, in particular for the highly centralized, top-down knowledge policy regimen that has emerged in Europe. The European Union science policy establishment is narrowly focused upon catching up with the USA, and upon not being overtaken by China and India, and has found academics with a STS background to be very useful allies in the process.

Fuller's re-evaluation of the partial social insulation of the university does not imply any general weakening of his strong democratic, bordering on populistic, impulse with respect to issues of knowledge production and distribution, however. Thus, he has lately acted in defence of educational schemes in which Intelligent Design is given equal space on the school curriculum as Darwinism if this is in accordance with local interests (cf. Fuller 2008).[61]

16. We next move on to evaluate Fuller's Social Epistemology in terms of the same issues with which we have confronted the other STS schools.[62]

I. What are Fuller's ambitions with respect to explanation? What is his concept of explanation? Here, we first need to emphasize the fact, which has transpired from our earlier discussion, that Fuller is not primarily interested in *explaining* the development of science, but in *changing* it. Hence, he is concerned with explanation mainly as a tool for change. Still, he seems to accept the overall picture of science presented in STS as generated by, and hence explicable by reference to, social interests (in the broadest possible sense of the term). He even defends this picture against what he himself calls "the Strong Argument against the Sociology of Knowledge".

This observation does little to clarify Fuller's notion of explanation, however. His comments upon that notion are brief and perfunctory; many of them are mere asides, made in the process of discussing other topics. But, as far as one can tell from the textual evidence, Fuller subscribes to a quite orthodox notion of explanation that involves the tracing of causal ties. Since such ties are seen as reflecting general causal laws, explanation comes to be construed as the subsumption of explananda under such laws, as the traditional, Hempelian conception has it. There is even an apparent commitment to the classical, but widely rejected, Hempelian assertion of the structural identity of explanation and prediction – see Fuller 1988/2002, p. 252, where explanation is characterized as a set of causes, knowledge of which would also allow us to predict the phenomenon in question. The Hempelian conception of explanation coheres well with Fuller's idea of science studies as aiming at a "unified social science, which in its search for regularities and causal mechanisms will provide the basis for a science policy" (Fuller 1989/1993, p. xiii). This reads very much like the motto of the Edinburgh School.

We may note in passing that the above reading is not contradicted by Fuller's critical reflections on the "politics of explanation" (cf. Fuller 1993, p. 152, p. 386 f), where explanation is represented as a tool of power, a sentiment Fuller shares with late Latour. Indeed, this critical stance corroborates the reading, since such misgivings are pertinent precisely with respect to Hempelian, "subsumptive" explanation, but beside the point with respect to "softer" kinds.

Fuller is as discreet as are the rest of STS'ers, however, in explicitly invoking the Hempelian model and pondering its implications for the magnitude of the task STS'ers take upon themselves. True, alone among the major figures, Fuller does mention Hempel and Hempelian explanation, but in a slightly different context and without relating it to his own explanatory practice (Fuller 1988/2002, p. 148).

So much for the very concept of explanation. Next, we need to examine what kind of factors Fuller would want to invoke to explain the course of scientific development. His theoretical statements are again a bit perfunctory, so we need to supplement them with reflections on his practice.

On the explicit side, Fuller repeatedly recognizes an influence from Marx, which implies that scientific developments are to be explained, among other things, by reference to interests. But Fuller makes the obvious reservation that Marxism was tailored to the situation of 19th-century European society, not to modern society, where the notion of socio-economic classes (and hence an orthodox Marxist notion of class interests) has lost its usefulness. Instead, social interests are distributed across a much broader and much more heterogeneous set of carriers (Fuller 1989/1993, p. 58). As a matter of fact, the things that most often occur in this role in Fuller's writings are *disciplines* and their representatives. A standard stratagem of his is to explain scientific developments as a result of disciplines, or professions, fighting for resources, for legitimacy, or for academic turf.

This quasi-Marxist mode of explanation is supplemented by another, which plays a significant role in Fuller's presentation, but which he apparently never acknowledges. We may call it *historico-narrative*, since it consists in explanation-by-embedment-in-a-historical-narrative, where a person or a discipline is presented as the heir to, descendant of, or representative of, a historical lineage of thinkers who are committed to a particular constellation of ideas. We saw a prime example of this in Fuller's treatment of Kuhn, who was seen as belonging to the historical lineage of Plato. Often the two explanatory modes are combined, with the historico-narrative one accounting for the overall intellectual content of a position, while the interest-based one explains the particular twists and turns this content undergoes under the influence of current contingencies.

We may note that Fuller typically does not offer these historical accounts as part of historico-sociological case studies in the natural sciences; rather, they serve as weapons in his dialectics with opponents within STS, or within philosophy.[63] A favoured dialectical manoeuvre in Fuller is to show his opponent to be heir to some repugnant political or ideological tradition. The most striking example is once more the discussion of Kuhn, who is made out as an heir to the Platonic doctrine of "double truth". This strategic use might raise a doubt as to the intended import of such emplotment, which may either be seen as a genuine attempt to establish a causal nexus, hence as a genuine explanation, or rather as ideological criticism by means of placing a person in incriminating historical company. Fuller apparently intends both uses at the same time. Specifically, he makes it clear that such emplotments are meant to indicate a causal connection between a current person's thinking and a historical lineage of ideas; hence, the former will be causally explacable in terms of the latter (Fuller 2004, pp. 25–26).

We encounter a third major factor that Fuller sees as shaping the development of science, namely *rhetoric*; but this, too, fails to be accommodated in the explanatory models which Fuller adopts – if only for the reason that these are so implicit. At any rate, it is clear that, according to Fuller, the explanation why some scientists adopted a certain position will sometimes be that they were swayed by rhetoric – perhaps expressly designed for that purpose by a clever Knowledge Manager. It is not clear, however, whether or not Fuller would reconstrue this as an interest explanation, in the final analysis. His insistence that the rhetorician establish a platform of mutual understanding with his addressee within the framework of an *ethos* might be taken to imply that an appeal must be made to the latter's interests. On the other hand, if we look at Fuller's own rhetorical practice, we see that it offers numerous examples of reasoning showing very little effort in the way of establishing *ethos*; it looks exactly like standard reasoning in the analytic tradition. (A good example is Fuller's sustained discussion of the intricacies of incommensurability, indeterminacy of meaning and referential semantics in *Social Epistemology*.) Clearly, it is primarily addressed to adherents of the latter tradition; but if such communality of argumentative style is enough to establish *ethos*, then Fuller's concept of rhetoric covers large parts of what philosophers would call dialectical argumentation. An explanation of a scientist's change of heart as a result of such argumentation, and his modified subsequent behaviour, would hardly count as an interest explanation in the ordinary sense, but would rather correspond to what some analytic philosophers are fond of calling a "reason explanation".

It transpires from the above that Fuller is not a social macro-determinist with respect to the formation of scientific knowledge. He believes that people can be swayed from the course that their professional or individual interests would dictate by such means as rhetoric – that is, abstract arguments. Thus, he avoids the pragmatic inconsistency that befell Bloor and the Strong Programmers as soon as they tried to persuade opponents by abstract arguments not appealing to the latters' interests.

17. In our original discussion of explanation in the context of the Edinburgh School, we distinguished between Type I and Type II accounts, the former explaining the genesis of theories, the latter only their reception. Fuller never indicates very clearly which kind he endorses, but we may infer from other discussions that he sees explanation to be of the latter type. I base this upon his quasi-neodarwinian model of scientific evolution. In the biological model, the emergence of new geno-typical

traits is not explained, but figures as a background parameter (explained in the final analysis by chemical processes in the gene), leaving only the selection of traits to be explained. In Fuller's model, similarly, the emergence of new ideas is not explained sociologically, but is construed as the effect of an infra-sociological phenomenon – that is, the inherent unreliability of the processes by which intellectual traditions reproduce themselves. Just as, in the biological model, mutations amount to mere "noise" from the point of view of faithful transmission of biological information, so novel scientific ideas appear as "noise" in the transmission of the semantic content of scientific theories from one person – or generation – to another. Although there is indeed a sociological component to this disturbance, reflecting the frailness of the institutional mechanisms by which knowledge transfer takes place, it also reflects the inherent complexity of advanced scientific theories and is thus not fully socially explicable (Fuller 1988/2002, p. xxix).

Restriction to Type II explanation, and abstention from the ambition to explain the genesis of scientific theories, absolves Fuller of the charges we directed at the Edinburgh School. He does not have to shoulder the excessive explanatory burden of having to somehow derive the features of scientific theories from social parameters. However, this does not quite resolve all possible worries over Fuller's position. The fundamental problem at issue in our discussion of the Strong Programme was what explanatory role was left for reality to play; this question has not been answered as far as Fuller is concerned. However, it is better discussed in the context of the problem of relativism, to which we now turn.

II. Fuller rejects relativism and, true to form, his main objection to it is sociological. We live today in a global world, as Fuller reminds us; hence, relativism embodies an outdated sociological stance wedded to the assumption that societies or cultures form isolated enclaves (Fuller 1993, p. 319). Fuller is highly wary of such balkanization, in science as well as in politics at large. This attitude is natural, in light of his urge to dissolve the internal barriers within science (i.e., disciplinarity). But Fuller takes care to distinguish globalism, which is a naturalistic, sociological conception of inclusiveness, from that of universalism, with its transcendental overtones. Fuller rejects the Kantian fiction of universal standards of thought which would command the assent of any rational cognizer.

In *Philosophy, Rhetoric and the End of Knowledge* (1993, p. 91 f), Fuller offers us a slightly different elaboration upon the anti-relativist stance adopted by Social Epistemology. Different scientific disciplines and different branches of science are animated by different aims and values, which we may well call social, such as survival, welfare or utility for various technical ends. The Social Epistemologist's goal is to help us raise ourselves above such particular concerns and pursue the ends of science in a more inclusive manner, that is, as a weighted sum of particular concerns. This satisfies the classical epistemologist's yearning for an epistemic standpoint that is not just one among a number of equals, but one that is somehow elevated above the rest. In terms of such a conception, the Social Epistemologist may pursue the classical epistemologist's business of critiqueing particular ways of doing science from a point of view that is different from any of them. The difference between the Social Epistemologist and the classicist is that the latter believes himself to occupy a transcendental platform, while the former's platform is avowedly naturalistic – that is, defined in sociological terms. (See also Fuller 1993, p. 319 ff.)

This argument will go some way towards averting the classical epistemologist's critique. Still, although Fuller may be safe from the kind of criticism we directed against the Strong Programme, some of the underlying worries carry over to his position as well. One worry was that Bloor's relativism leaves scientific knowledge tracking social interests rather than non-social reality. Now, while Fuller's position may not have science tracking shifting and locally variable social interests, it is hard to see that he provides any means for science to track *nature*. Fuller's conception leaves science to track a specific combination of the societal ends listed above – survival, welfare, practical efficacy – which he himself describes as being various "surrogates" for truth (Fuller 1993, p. 91). But we would precisely want science to track *truth*, not its surrogates.

As it happens, Fuller himself states and addresses a somewhat similar worry (Fuller 1989/1993, p. 82, 1993, p. 91). His rejoinder is that the distinction between arguments appealing to various conventional, socially valued proxies for truth and arguments invoking genuinely evidential, truth-conducive grounds, must itself be reconstrued sociologically, that is, as a difference between short-term and long-term perspectives. When theories that have initially received high marks on the proxy scales fail to maintain their standing in the long run, we conceptualize this as a contrast between merely pragmatic considerations and truly evidentiary (truth-conducive) ones. On the other hand, if a theory continues to accumulate positive marks, as measured by those same proxy standards, we eventually redescribe this as a situation in which the theory is genuinely in touch with reality (or, actually "tracks" truth).

Fuller would no doubt support this anti-realist reconstrual with the observation that science could not possibly track truth, in the realist's sense. Truth is unavailable as a systematically achievable goal for science (cf. Fuller 1989/1993, p. 213 where truth-seeking is construed as the fictive search for Hyperreality. But see also ibid., p. 83). Hence, there is no option for science but to pursue truth's "surrogates". This position would be supported by his general anti-realist stance, which, as we saw, reflected his conviction that science does not show the requisite approximation towards a "final theory".[64] But here a troublesome question appears. Fuller declares himself to be a realist with respect to social science, but how does he prevent the above argument from generalizing from natural science to social science? If anything, there would seem to be even more reason to suspect the integrity of intellectual traditions in the humanities and social science. If so, his own findings in sociology and psychology would also seem to fall by the wayside. They, too, fail to track the reality with which they ostensibly deal.

The problem extends even to historiography as a source of data for STS. In *Social Epistemology*, Fuller stated that a robust realism is required for a historian of science (op. cit., p. 65). However, in a later text, he undercuts this position by stressing that the idea of a simple recounting of historical events or developments "as they actually happened" is a naive superstition. Instead, historical research must make the transition from the humanities to the social sciences (Fuller 1993, p. 192 f); that is, it must strengthen itself with theories borrowed from the social sciences, and from psychology. But such theories are as vulnerable to Fuller's anti-realist arguments as are those of natural science.

In some of his writings, Fuller seems to attempt to turn the tables on the reflexivity objection by appealing to the practical agenda of STS. He accepts that anti-realism and constructivism extend to psychology, too, but extracts an answer to the critic from precisely this fact. The failure of psychology to track truth would, indeed, matter if the business of psychology were representation. But it is not; it is intervention and construction. More precisely, it is the fostering of habits of thought and action that are deemed congenial to certain societal interests.

This argument, however, exploits a hidden ambiguity in the term "constructivism". Fuller champions constructivism, meaning the view that science (materially) generates phenomena that did not exist before the advent of science and its artificial laboratory settings and procedures. In the case of psychology, this involves, for instance, the use of drill and suitably regimented frameworks to help man's scientific thinking reach levels of rationality – in the sense of means-end efficiency – that were never achieved before. This is different, however, from "constructivism" as the tenet that the abstract, conceptual models in terms of which we grasp reality are generated by a process that fails to track reality and, hence, does not produce "representations" of the latter.

Fuller needs *truth-tracking* models of the "constructed" (artifactual) psychological processes resulting from the suggested manipulations of human cognizers, because only such models offer advice about the scope and robustness of his practical manipulations. After all, the successful installation of good reasoning habits in a particular group of people on a particular occasion might be an accidental effect of the personalities involved, what people had eaten for breakfast, or the quality of the lighting (remember the Hawthorne effect!)[65] Fuller needs a device that will allow him to project beyond the original setting, in order to generate similar successes elsewhere, with other protagonists. Only a truth-tracking theory can deliver this.

The point is strengthened if we take into consideration those further proxies for truth that, according to Fuller, serve as the basis for selection of scientific theories, such as ability to improve human welfare. We need to keep in mind that the constructivist wants these concerns to determine theory choice by ranking theories in proportion as they *genuinely serve* those interests. He is not in any way helped by the fact that interests may tacitly sway theory choice in a direction that is intuitively flattering to those interests, but perhaps in reality is detrimental to them. But how could we assess if a given modification of man's cognitive habits would genuinely enhance human survivability on a societal scale, save on the basis of general sociological *knowledge* – real, truth-tracking knowledge – about how such thought processes inform various societal processes? (Cf. our discussion on p. 59.)

It might be objected that the constructivist is really not in a worse position in this regard than the realist. The realist, too, must invoke devices to take up the slack between theory and observation, known as the underdetermination of theory. Despite valiant efforts on the parts of philosophers, it has never been demonstrated to everybody's satisfaction that the favoured criteria of theory choice – simplicity, fecundity and others – are inherently truth-conducive. So, the difference might seem to boil down to the constructivist's preference for truth proxies that serve blatantly practical, non-epistemic ends, as opposed to the realist's preference for more abstract, "aesthetic" values.

However, this is to underestimate the difference between the realist and the constructivist (instrumentalist). The realist constantly endeavours to eliminate recognized slack by replacing appeals to abstract epistemic standards with hard empirical evidence; and although it is surely true that there is no such thing as a logically pure "crucial experiment", it is equally true that accumulating empirical evidence ultimately will often fatally discredit a contender. Moreover, even if he had, against all odds, obtained a perfect local match between a given theory and the data, the realist scientist still would not be satisfied. He would start speculating about possible ways of extending the theory and devise possible experiments to check these hunches; his aim is to find a maximally inclusive theory. The true constructivist (instrumentalist) has no incentive to do any of this; as long as we possess a theory that allows us to handle reality satisfactorily, there is no reason why we should try to decide between that theory and another one of equal usability. Nor is there any point in trying to extend or generalize our theory to cover neighbouring areas, as long as those areas are covered by a (pragmatically satisfactory) theory of their own; not even if that theory is inconsistent with the former. Such an endeavour could only be motivated by the idea that reality possesses an objective underlying structure not reflected in our local "captures" of it, a stance which Fuller rejects (cf. Popper 1963d, p. 111 f).

Finally, note that the objection directed against Fuller here does not spring from the insistence that epistemology, including Social Epistemology, must provide an a priori assurance that our cognition establishes contact with reality in the first place, thus ruling out radical scepticism. Like other STS'ers, Fuller routinely frames philosophers' worries about the coherency of STS as having precisely this source. As Fuller correctly observes, that project is a famous dead-end and typically ends up giving in to scepticism (Fuller 1988/2002, p. xxviii). But this is not the criticism raised here. The point made here is rather that Fuller's system possesses features that positively preclude human thought from ever establishing reliable contact with, or "tracking", reality.

18. The roots of all these problems, of course, are in Fuller's constructivism. This is an unfortunate feature of his system, since it prevents him from forging a natural alliance with the programme that is, no doubt, his closest kin in the intellectual landscape, even to the extent of sharing the same name: the social epistemology of Alvin Goldman and collaborators. The latter is typically based upon a realist metaphysics. Fuller's constructivism is hardly essential to his project. True, there are certain advantages to Fuller in a constructivist position, since it provides more leeway for the effort to change the conduct of science though organizational means; there is no independent reality to constrain this effort. On the other hand, it gives less freedom to manoeuvre in the intellectual sphere, since it locks Fuller into a

position where he is not taken seriously by people whom he might want to recruit as allies. Constructivism clearly appeals to Fuller's activist instincts, which makes STS a tempting alliance partner for him. But he might get further by means of an alliance with mainstream social epistemologists. From Fuller's perspective, which judges normative proposals by their ability to reach implementation, this is a very serious problem indeed.

We may add the final observation that in adopting his favourite argument against realism, based upon the alleged failure of theoretical content to be transmitted reliably over time, Fuller momentarily forgets his own meliorative and activist attitude to the shortcomings of human thinking. Surely Fuller is right, at a purely historical level, that long-term theoretical endeavours within academia have often been hampered by tacit shifts in basic assumptions and inconstancies of conceptual definition. It is no doubt true, as a historical observation, that such vices have often kept academic debates from producing any robust truths and, even, from going anywhere at all. But, consistent with his basic attitudes, Fuller might have chosen to rectify this shortcoming, instead of giving in to it. He might have devised a strategy by which we further the progress of science – as measured by any reasonable criterion – by strengthening the instruments by which scientific content is transmitted, thus enhancing the possibility of subjecting research traditions to that consistent criticism which he allegedly considers the most valuable aspect of science.[66]

19. Fuller's work epitomizes the transformation of STS from an effort officially dedicated to a purely theoretical agenda to an explicitly normative one. In Fuller's writings, this stance was clearly articulated from the start; however, it took a while for the rest of STS to catch up with it. Orthodox STS shared an implicit normative agenda that would stress the responsibility of science in relation to society and the need for a democratization of the way science is conducted. The hope was to achieve this goal by exposing the essential entanglement of natural science with underlying political interests and societal forces, thus dispelling the myth of science's elevated, unbiased status, which had hitherto protected it against calls for increased societal control. The tool was to be detailed explanation of crucial turns in the history of science in purely social terms. It was gradually realized, however, that this strategy had no chance of success, for reasons I have tried to spell out in detail. With this recognition, the underlying normative agenda of STS was allowed to come to the surface and to be pursued by more explicit means.

Yet at the same time, the target of the original criticism (i.e., research undertaken according to the classical top-down manner of organization, today known as Mode 1) had started losing ground to an alternative organizational scheme (Mode 2). While Mode 1 organization might fairly be said to embody an elitist view of the relationship between scientists and the consumers of scientific products, Mode 2 organization could be reconstrued as accommodating democratic values: responsibility for major decisions is distributed among a much larger group of shareholders. Such a shift in perspective is inherent in much recent STS work, which may even be said to have formed an alliance with current trends in official governmental science policy, particularly in Europe; the underlying agenda is apparently to promote the interests of Big Science within such fields as nano-, bio- and IT technology. On the STS side, the exercise is conducted within a rather nebulous quasi-political framework, involving such themes as the "Politics of Nature" or even the "Parliament of Things". Thus, in Fuller's view, current STS still invites criticism on the normativity issue; these days, however, the charge will not be that STS fails to be normative, but that it has chosen the wrong causes to pursue.

In effecting the transformation of STS from a theoretical to a normative undertaking, Fuller evades many of the obstacles to this approach I have detailed earlier. Fuller does not subscribe to a strongly socio-determinist conception of science, whether referring to interest (the Marxist line) or to socially distributed cognitive patterns (the Durkheimian line). This allows him to escape the quandary in which the Strong Programmers found themselves. His "globalist" conception of science (as an ideal), which has it that science must raise itself beyond the local conditions of its production and serve general interests, means that the problems of relativism which bedevil many other versions of STS are not as pressing for him, either.

I have tried to show, however, that a sore spot in Fuller's project is his continued commitment to constructivism, more particularly to arguments that lead to a more universal constructivism than Fuller himself is willing to embrace. First, it undermines his laudable intention of using results garnered from the sociology and psychology of science to improve upon the performance of scientists. Secondly, it estranges him from certain efforts within current analytic epistemology – also operating under the label of "social epistemology" – which could be useful in his own work.

Chapter 10
An Alternative Road for Science and Technology Studies and the Naturalization of Philosophy of Science

1. Our investigation into the development of Science and Technology Studies has come to an end. I have tried to show how this movement, arguably the most important novelty in the study of science during the last generation, emerged at the intersection of two trends, one long-term and internal, the other short-term and external. The former was the naturalization of philosophy, the latter the search for a better accommodation between natural science and society, inspired by a widespread disaffection in many Western democracies in the post-war period with science and its societal role. The merger between the two trends was fortuitous; there is no necessary communality of aim between them, as I pointed out in Chapter 2. In its early 20th-century logical positivist incarnation, the naturalizing movement was famously allied with (natural) science, precisely that alliance that the recent development has tried to break up. Moreover, concurrently with the developments within the study of science that we have examined, strong and fruitful efforts were under way within the closely related field of *epistemology*, where a naturalistic agenda is pursued in a happy colloboration between scientific disciplines – some of them belonging to natural science – collectively known as *cognitive science*. We examined this programme in Chapter 1 and found that, although some of the contributors are maverick disciplines using novel methods – such as computer simulation of cognitive processes – it harboured no grudge against traditional science and had no secret agenda of discrediting it. As for the science-critical trend, it is not inherently anti-philosophical; on the contrary, it has recently figured as a key element in a philosophical movement, even of a highly orthodox, transcendentalist nature – namely, the critical philosophy of the Frankfurt school, in particular in the version articulated by Jürgen Habermas.

We examined in Chapter 2 how, in several Western countries, worries about the societal role of science led to political and administrative initiatives to improve mutual accommodation between science and society. Science had to show greater accountability to society, in return for political efforts to secure a better understanding of the workings of science in the population at large, generating greater popular willingness to provide for its financial support.

The Science Studies Unit at the University of Edinburgh was one of the fruits of these efforts. It was to be the birthplace of the Strong Programme, which I have chosen as the point of departure for my story. This was the place where contemporary

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misgivings about the role of science were first married with the long-term trend of naturalization. Along the naturalization axis, the Edinburghers took the naturalization efforts of the logical positivists one step further. The logical positivists had worked to extend the range of (natural) science into areas that had previously been the privilege of philosophy, such as analysing the fundamental properties of space and time. Yet, paradoxically, they operated with a notion of science that was itself a priorist, idealized and, hence, non-naturalist. A sheer concern for consistency would dictate that this final remnant of a priorism be eliminated and that science itself be turned into a topic of empirical investigation.

Viewed from the perspective of the short-term trend, which expresses disaffection with science, the Edinburgh school represents a desire to demonstrate that science is not as lofty as it is generally thought to be and its deliverances to society are hardly undisputed blessings. The confluence of the two trends would produce the project of revealing the true nature of science by naturalistic (i.e., empirical) means. In practical terms, this meant using social science to debunk natural science; the idea is that a sober empirical investigation of science will reveal that it does not "escape the contamination of the social" (Aranowitz 1988) and moreover is often a mixed blessing to the society that plays host to it. This is true not only with respect to science's immediate and tangible effects upon human society and the environment, but also such more subtle effects as the spread of scientific ways of managing people and handling social problems.

The Edinburgh School's programme can be described as an effort to undo the politico-cultural agenda of logical positivism, while retaining the latter's basic naturalist epistemology and metaphysics – and borrowing a page or two from its strategy manual. The tools of positivism were to be turned against positivism itself, in order to roll back its cultural influence. The political goal of logical positivism was to transform societal modes of thought in the direction of scientific rationality, promoting a particular kind of scientific-instrumentalist thinking as a means to furthering enlightenment ideals. The strategy adopted for this purpose reflected the assumption that current metaphysical conceptions, with their inherent obscurity, bias and ideological distortion, would melt away when the bright light of scientific rationality was shone on them. Strong Programmers held that the received view of science as something ideal and otherworldly, promoted by philosophers in particular, is similarly metaphysical and ideological. They hoped that it would dissolve when science itself was made an object of scientific understanding.

The logical positivist position embodied a tension that was to some extent mirrored by the Strong Programme. Logical positivism would combine a normative, political agenda at the meta-level with a strongly proclaimed neutralism at the object level. From a strategic point of view, the latter would be at the service of the former: the ideals of scientific neutrality and objectivity would serve as intellectual standards by comparison with which traditional metaphysical thinking would stand forth as inherently flawed. This strategy allowed the logical positivists to escape the predicament created by the radicality of their language-based anti-metaphysical stance, which would condemn even their own political principles as, strictly speaking, meaningless.[67] The matter is handled with great delicacy in the official manifesto of the movement, *The Scientific Conception of the World: The Vienna Circle* (Hahn et al. 1929/1973). Here, a comment is made on the "close affinity" between the movement's scientific world view and its stance to "life questions", including issues of social policy. What is discreetly passed over is that the latter could only issue in meaningless pseudo-statements, were it to be explicitly articulated. Hence, an ethical criticism of the normative content of contemporaneous metaphysical thinking, launched from an alternative ethical position, was not a viable option. As I have argued in this book, the Strong Programmers faced a somewhat similar predicament, in that a strongly voiced normative agenda would have raised suspicions about the neutrality of their own findings about the workings of science. Such suspicions would fatally undermine the integrity of a position that celebrates the power of science as a unique instrument to reveal the true nature of science itself. Wise counsel would dictate that Strong Programmers should exercise considerable discretion in stating their normative agenda.

That a normative agenda with respect to science was present, even in the Strong Programme, is testified to by the fervour with which the project of naturalizing philosophy was pursued. It is hardly an exaggeration to call this stance anti-philosophical, rather that merely a-philosophical, at least as far as Strong Programmers are concerned. Dissatisfaction with philosophy of science in itself could hardly have generated so much heat; STS, however, was after larger prey from the start. It has been my contention that STS joined the naturalization movement mainly to undermine the privileged position of science in society. Philosophy of science came to be seen as an opponent mainly because of its strong alliance with natural science; more precisely, it was viewed this way because it promulgated an idealized view of natural science, of which it was itself deeply convinced, and which it tried to disseminate in society at large.[68]

I have tried to show that this critical stance towards science and the resulting anti-philosophical attitude have been detrimental to STS as an empirical discipline. Paradoxically, the ambition to naturalize philosophy, involving its total replacement by an empirical investigation of science, led STS fatally to compromise its empirical commitments. STS overreached itself in trying to show not merely that science has historically been shaped by societal forces, but even that its susceptibility to such influence is inherent and inescapable. To establish this strong modal conclusion, STS had, ironically, to resort to philosophical arguments – notably, in the case of the Edinburgh School, Wittgenstein's rule-following considerations and in later representatives, various rather eccentric ontologies – to shore up their case and demonstrate that nature's impact upon scientific cognition must invariably pass though a social conduit.

Wittgenstein's late philosophy was apparently highly germane to this naturalistic, yet anti-scientistic, reconstrual of science, since it was itself inherently anti-scientific and anti-rationalistic. As Bloor himself points out, Wittgenstein belongs squarely in the romantic, anti-scientific tradition of European thought (Bloor 1983, p. 160 ff). Bloor believed that he could sequester out these elements, retaining only ideas that would be of use for a reconstrual of science by showing it to be social through and through – without, however, undermining science altogether.

This stance was necessary to avoid charges of pragmatic incoherency, since Bloor's project was itself supposed to be an exercise in scientific method. In theory, such a compromise stand is feasible. However, Wittgenstein's rule-following considerations proved to be an intellectual solvent of such power that, once deployed, its disruptive effects were very hard to contain.

2. Despite Bloor's rhetorical efforts, there was clearly from the outset a whiff of incoherency in the Strong Programme. Science is invoked for the purpose of undermining science. This strategy would be coherent, of course, if the goal were a brute *reductio* of the scientific mode of thought, of this simple form: If science is valid, it is invalid, since this is what the scientific investigation of science itself teaches us. But refutation is not the goal. Social science, in particular the sociology of science, is meant to survive the debacle, not to self-destruct in the process. I have tried, in my discussion of the Edinburgh School, to turn this implicit tension into an explicit contradiction.

The incoherency of the Strong Programme was never conceded by its practitioners. Still, the ambitious strategy of the Programme was abandoned by later representatives of the movement, but for a different reason: the required strong explanations were never forthcoming and, indeed, could not be forthcoming, as I have argued. The explanatory ambitions were given up by later STS'ers, although they made no great fanfare of this, or were reformulated so as to circumvent the problem. With the demise of the explanatory strategy, which after all was the key element of the naturalistic approach, STS went from being tacitly normative to being openly so. The most dramatic example of this transformation was found in the work of Bruno Latour. We saw how he started out, in *Laboratory Life*, very much in the spirit of the Strong Programme, although the macro-sociological tools of the latter were to be replaced by micro-sociological or anthropological ones. Latour cast himself as a neutral observer of science in the tradition of ethnography, moving as an anthropologist among natives. What was supposed to emerge from this neutral, "symmetric" description was the demonstration that science can be explained fully in terms of societal parameters. The immediate conclusion would be that science's own cherished self-description, referring to evidence, argument and rationality would be revealed to be purely epiphenomenal, if not completely mythical. Science would be cut down to its proper size, that is, as one societal institution among others.

However, it soon became clear to Latour that science could not be explained without including reality as a factor in its generation. So, the strategy of showing the social nature of science by explaining it in theoretical terms provided solely by social science would have to be abandoned. The threat now lurked of a relapse to a Mertonian position, where nature and society are both granted a role in the explanation of science. To avoid this embarrassment, an ontological turn was undertaken. While material reality was granted a role in the generation of science, it was simultaneously reconceptualized as being somehow a part of the social world; or, rather, both the material world and the human world were conceptualised as parts of a third, newly discovered realm: the Actant Network. And although this realm is by definition different from the social world, since it is its ontological ancestor, the terms used to describe it are evidently much closer to those traditionally used to decribe the social world. Indeed, Latour notoriously presents the science of this new realm as the successor discipline to sociology. But perhaps it is even closer to political science: Science is now described in quasi-political terms, as a process of negotiating interests, not only among the human participants in the process, but even among the non-humans as well. It is all a matter of forming alliances, which again is a matter of translating the issues such that every party – every actant – sees an interest in joining in.

The villain of the piece is the traditional conception of science, which bestows privilege upon a narrow societal group – the scientists – who have been granted the prerogative of laying down truths to which the rest of society is somehow committed, even though denied a role in their production. According to the new, alternative perspective, the terms in which science is described are explicitly political and the endeavour behind the enterprise is, correspondingly, openly normative: to be rid of this false privilege and to give science an explicitly political, democratic foundation. Science Studies call for at reform of science, so as to empower all social groups that have a stake in it.

3. The critical message inherent in STS – implicit at first, but quickly becoming more explicit – was not lost on the scientific community. This got STS entangled in the Science Wars, which I have left out of the story so far; it is now time to have a brief look at this highly publicized imbroglio.

According to the standard story, hostilities were opened by the publication by Alan Sokal, a physicist at New York University, of a spoof in the American journal *Social Text* (Sokal 1996a). In the article, highly esoteric results from logic, mathematics and physics were ostensibly used to establish points within the cultural and political sphere. Shortly afterwards, Sokal revealed his hoax in the journal *Lingua Franca* (Sokal 1996b), presenting his intervention essentially as an experiment to test the quality of academic standards in the new left Cultural Studies and related disciplines.

In the original article, the targets of criticism were mainly French philosophers and literary theorists such as Jacques Lacan, Luce Irigaray, Louis Althusser and Jacques Derrida. The charge was not primarily that these authors gave a twisted account of science, with a view to diminishing the latter in the public eye, but rather that they used scientific results, in a distorted and garbled form, to prop up conclusions within the political and cultural realm. By so doing, these authors actually paid a lopsided tribute to science, since they tapped into its prestige for their own purposes. Still, prominent representatives of STS such as Bloor, Pickering and Latour appeared in the copious notes. But soon, STS writers were more explicitly targeted. This happened in a book, entitled *Fashionable Nonsense*, that Sokal co-authored with Jean Bricmont, a Belgian physicist (Sokal and Bricmont 1998). Here, both the Strong Programme and Bruno Latour came under fire. But even in this book, the target of criticism is as much the penchant of philosophers and other humanist academics for drawing fallacious inferences from natural science, with a supposed bearing on their own fields, as their occasional attacks on natural science, launched from their native academic turf.

In fact, a critique directed more specifically against central figures in STS had already been voiced some years earlier, in a book by Paul Gross and Norman Levitt (1994). Here, the indictment is precisely that these authors have defamation of science in mind. Still, the real target of criticism is not these (mainly foreign) luminaries but rather someone closer to home, namely, what is somewhat vaguely referred to as the American "academic left". The authors saw the popularity of the criticized work among this faction as a sign of the latter's alleged animosity against science. The Nobel Laureate in physics, Steven Weinberg, also joined the fray with contributions that indicted Latour and Pickering, among others (Weinberg 1992). The charge was, among other things, that STS'ers had contributed to the demise of the Super-Conducting Super Collider project, into which the American scientific community had put so many resources and so much prestige.

The general tenor of the criticism, beyond the charge that the adversaries had misunderstood most of the technical issues under discussion, was a sense of betrayal of science's noble political mission. Both Sokal and Gross and Levitt identified themselves as traditional leftists and as believers in a firm alliance between science and the enlightenment agenda. Science is a friend of the oppressed; the truths it uncovers are liberating. As a result, great shock is experienced when truth is denounced, or even represented as an instrument of oppression in itself.

Faced with these charges, STS'ers generally had difficulty in deciding which leg to stand on. They would alternately play the role of the innocent victim, complaining that the war that was being waged against them was based upon a misunderstanding, since they had never wanted to debunk science, or try to play down the whole thing, insisting that there was never a war in the first place, since this calls for two adversaries while, in this case, there is only one, that is, the natural science community.

This reaction smacks a little of disingenuity, however. It is true that STS'ers are not out to debunk science in the trivial sense of denouncing its results as false, or unsupported, nor would they dismiss its products as generally harmful. Many STS'ers fully appreciate the great benefits that mankind has reaped from the scientific institution. But it is equally true that they radically reconstrue the source of its validity and authority. Their aim is to demystify science; in particular, they denounce the cult of scientific rationality and highlight, and indeed celebrate, science's sheer power to mobilize societal resources on its own behalf; this, to them, is the real secret of its strength.

But here is the point where the STS position becomes ambiguous, bordering on disingenuous: Although science is not uniquely rational, according to STS (but still *no less* rational than other societal practices), they claim that the promulgation of a myth to this effect is crucial to securing its societal efficacy. Scientific results acquire their authority largely by virtue of the public conception that they are produced in a manner elevating them above the ordinary societal hustle and bustle, thus rendering them immune to social bias. Notoriously, STS'ers took great pains to show that this is indeed an illusion and to trace the societal links through which science establishes its dominance; this was the common theme of the many celebrated case-studies. STS'ers have used numerous metaphors to express this feature of science. Collins likens science to the art of building a ship inside a bottle, the fascination of which resides precisely in the shipbuilder's ability to hide the work through which the ship was put there, and leave us with the illusion that it must have been there all along (Collins 1975). This is similar to the sleight of hand through which scientific "facts" are generated while appearing to have somehow always obtained. Latour and Fuller present similar conceptions. Fuller draws a parallel between bourgeois political economy, as depicted by Marx, which is supported by reifications and fictions, and the realist view of science, which projects the outcome of human efforts "out there" as an objectivity effect (Fuller 1988/2002, p. 235 ff). Fuller also traces the power of science back to the circumstance that science controls the way its own history is typically told, that is, as a Whig history of successes leading to the currently accepted science, with all the false starts censored out (Fuller 2007, p. 2, 1997, Chapter 5). STS'ers add that these illusions are socially effective, since they do serve to bestow upon science a status enabling it to deliver the promised goods.

On this background, the reaction of the physics community to the efforts of STS should have come as no surprise. Even if natural scientists were to be persuaded of the truth of the STS story, the general dissemination of this insight would still appear undesirable to them: It would appear as an attempt to call a benign and beneficial bluff. To the majority of scientists, who do not buy the STS story, the challenge would appear even more odious, of course. To them, science is not a confidence trick, but enjoys genuine and well-earned authority; this is an authority it stands to lose, however, if STS'ers succeed in persuading society at large that a trick is being played on them. To the ordinary scientist, it is as if somebody were to claim that all medicines are really somatically inefficacious – their apparent efficacy being a mere placebo effect – and then add that the power to generate this effect precisely constitutes the beauty of medical science. What is even worse, he would expend great effort to spread this message, in a manner likely to annul the placebo effect, while denying any such intention. To the scientific community, this seemed like adding insult to injury. No wonder that the reaction was harsh.

The Science Wars have now run out of steam, which may in part reflect greater circumspection on the part of STS'ers, who have taken to heart that their rhetoric appeared rather more inflammatory than they had intended. More interestingly, the situation also reflects a change in STS strategy, which is now redesigned to accentuate the positive. Instead of continuing to denounce the elitism of the classical Mode 1 science and its philosophical advocates, STS has formed a tacit alliance with technology and the emerging Mode 2 science, singing the praise of the democratic virtues of the new organization of science, in which scientific products are generated in a happy collaboration among scientists, businesses and government institutions. An added strength of this strategy, of course, is that it "goes with the flow" – of money, that is, which is currently being massively channelled away from the kind of projects that STS'ers abhor – such as the Supercollider – towards projects that will immediately benefit all of us, such as using new technologies to produce more and better consumer goods.

In its most recent version, represented by Latour and Pickering, the STS gospel of empowerment and democracy is not limited to human benefactors. The material world and its inherent "agencies" – redescribed in the actant terminology – are not merely the equals of humans when it comes to explaining the generation of science, but are so even in a normative, political sense. We need to take into consideration the rights of nature in our dealings with the environment, even to the extent of instituting a "parliament of things", in which our disagreements are negotiated.

This is a clever manoeuvre by which STS can recruit a very powerful ally, that is, the strong ecological sentiment in current society. The goal is to defend nature against the encroachments of man. What is conveniently sidelined by this move is the contrary worry, the encroachment of the material world upon man, for instance in the form of intrusive material technologies, such as nano-scale devices that will make total surveillance possible, or gene-modified foodstuffs that may pose secret threats to human health. Such worries are made to look misplaced and unsophisticated now that we realize the happy communality among men and material things, from which new technologies emerge. This is the perfect metaphysics and ideology from the point of view of Mode 2 science and the modern conglomerate of IT-, nano- and biotechnology, which constantly have to combat popular scepticism. Latour's metaphysics fits like hand in glove with the endeavour to still such worries.

4. What I have offered in this book is in part an internalistic account of the development of certain methodological controversies within STS, under the impetus of their own inherent logic. But there is also an externalistic, sociological aspect to the story, which is the role of the anti-scientific agenda. Thus, in a very modest way, this study in part itself dabbles in the sociology of science and, indeed, makes use of many of the tools that were developed within STS itself. (This is not contradicted by my adoption of the internal/external distinction, which is used in an innocent way and not in order to suggest the kind of strict division between rational and irrational motives that is abhorred by STS'ers.) The overall structure of the sociological account is Type II-explanation (cf. p. 47), as the *explanandum* is not the emergence of ideas or arguments, but rather their reception. Or, more precisely: The way in which certain ideas that had been around for a very long time – that is, ideas about a naturalization of philosophy – were adopted as instruments for a specific agenda. The account is clearly not meant to be deterministic, but at most probabilistic; minimally, it may be recast as an account in terms of *necessary conditions*, pointing to a feature – the presence of worries about the societal role of science – without which Science Studies would not have followed the path actually taken.

This methodological move has been possible because of the wonderfully self-exemplifying nature of STS. Indeed, it may justly be said that STS's main tenets apply much more accurately to STS itself than to natural science. This is true even for the claim that reality plays little role in the generation of science, a tenet that I have found reason to reject with respect to science in general and to natural science in particular, but which, ironically, happens to apply fairly accurately to STS.

I have argued that STS lost its grip on empirical reality as its explanatory ambitions weakened. According to the Hempelian formula that, as I have argued, largely captures the intent of the original Strong Programme, explanation is isomorphic with prediction, which means that every successful explanation is at the same time a successful testing of the theory invoked in the *explanans*; the difference between the two is merely pragmatic. (The same result follows from any theory of science that construes the validation of scientific theories as a matter of "inference to the best explanation".) However, once explanation is given up, or rather, Hempelian explanation is abandoned in favour of a quite diffuse and unspecified alternative, the external, word-to-world check upon the whole enterprise is now compromised. Cut loose from its empirical moorings, it now drifts freely under the impetus of its tacit political agenda, gradually assuming the character of a political programme, or even an inchoate popular movement. This development is clearly visible in Bruno Latour's work.

The vicissitudes of the concept of explanation in STS illustrate a number of other STS tenets. Latour, Pickering and Fuller all stress the transformation of intellectual content that happens in the production and dissemination of scientific results. To Latour, this follows from the necessity for such content to be continuously "translated" to appeal to the concerns of prospective recruits to the network. To Pickering, it is an aspect of the "mangle of practice", through which all components of the scientific process are constantly being transformed in ways that are in principle contingent and unpredictable. According to Fuller, the change in intellectual content is primarily due to the inevitable information loss suffered when ideas are passed between generations of researchers, or shifted from one intellectual context to another. The main cause is the weakness of the mechanisms whereby intellectual content is transmitted. All these points are clearly illustrated by the history of the concept of explanation, which was officially at the very core of STS doctrine at the outset. The Strong Programme insisted that its goal was causal explanation of the trajectory of natural science, in a strong sense of explanation that is largely captured in Hempel's model. But on this point, an immense cognitive loss was suffered in the span of just two generations of academic work. Theory of explanation had reached high levels of sophistication in the previous decades, with Hempel's model setting the agenda around which an intense critical debate revolved. A conscientious application of the results emerging from this debate to the foundations of the Strong Programme could have saved us from much confusion about the exact nature of the explanatory ambitions of the programme, and would have made possible a sober assessment of its slim chances of success right at the outset.

STS is thus remarkably self-instantiating on this point, too, illustrating to good effect the "logic of opportunism" that it attributes to science and the constant translations of interest between users. Naturalism is fused with Wittgensteinianism, which in turn is fused with anti-scientism, which itself is fused with ideas originating in the ecological movement, and so on. In the process, most of the content is subtly – or not so subtly – transformed. Bloor does not hesitate to straighten up Wittgenstein to suit his purposes, but the transformation is tacitly taken even further in Collins: here, Wittgenstein is fused with Goodman and Hesse to form a hybrid the viability of which I have questioned. Implications that are undesirable, such as those following from Wittgenstein's conservative anti-scientism, are either explicitly denounced, or are simply allowed to drop out of sight, a fate that has also befallen key elements of Hempel's theory of explanation.[69]

The contents of this notion were further obfuscated in later versions of STS when it became clear that it was a liability to the movement, until it was finally transformed into an ontological category of little methodological import in Latour's work. In this process, the insights into explanation that had been won in the previous decades were silently dropped. At an equal pace with this, STS was transformed into a openly normative project.

Steve Fuller's work epitomizes the current standing of STS as an explicitly normative, political project. In Fuller's comprehensive work, no substantial original case-studies of science are presented. He is not overly interested in documenting how science is actually conducted, but rather in how it should be conducted in the future. His concern is explicitly normative and political. And where early STS'ers pinned their hopes for the practical efficacy of their efforts on such diffuse effects as "consciousness raising" in society at large, Fuller does not hesitate to propose concrete institutional structures to safeguard the proper accommodation between science and society. What we need, according to Fuller, is nothing less than a constitution regulating the role of science in society.

The metamorphosis of STS is evident also from its institutional fortunes within academia. If the original scientific ambitions of the Strong Programme had come to fruition, Science Studies would no doubt today stand as central and powerful disciplines within the social sciences; their results would be recognized as signal achievements that the rest of the social sciences must emulate. This status would no doubt be reflected in a strong institutional standing within universities. But this has not happened; on the contrary, many of the original Science Studies units have been disbanded and their staff split up and absorbed into other institutions.

Characteristically, the strongest institutional concentration today of individuals with a science studies background is in business schools, where they typically populate institutes dedicated to the art of Knowledge Management. Here, we see again the slide from a theoretical, descriptive approach to the sciences to a largely practical one – with the added irony that the individuals in question now find themselves operating according to an institutional agenda that is considerably more favourable to the interests of science, technology and private business than the one that originally inspired Science and Technology Studies.

5. So much for the history and current status of STS. Let me finish by briefly sketching out possible future lines of development, followed by my own cautious recommendations.

STS has many avenues of further development. If STS wanted above all to find an outlet for its critical ideological energies, it could transform itself into the *critical sociology of knowledge society*. This would surely be a worthy and natural task for a sociology of knowledge: classical sociology has aptly been called the science of industrial society and the natural counterpart to this in today's world would surely be the science of knowledge society. The critical wing of classical sociology was Marxism and the sociology of knowledge could adopt a similar critical role in relation to knowledge society. It could be to knowledge society what Marxism was to industrial society.

There are many interesting problems to grapple with here, some of which have already been taken up by critical sociologists. For instance: Is knowledge society still a class society and, if so, which are its classes? Certain enthusiastic advocates of the new global world order celebrate knowledge society as a classless society, but that may merely be because the import of the term "class" today is rather different than in industrial society. Other enthusiasts about knowledge society have not had any qualms about pointing out a new upper class, that is, the "creative class", but this could be merely the self-promotion of a particular group of high-profile professionals, and in any case smacks of pop sociology (Florida 2002). Meanwhile, such critics of capitalism as Luc Boltanski and Eve Chiapello consider the new society to be largely the old one, in slight disguise (Boltanski and Chiapello 2006).

Another interesting topic has to do with the "End of History" doctrine launched by Francis Fukuyama half a generation ago (1992). Famously, he predicted the imminent universal victory of the liberal, democratic society incorporating a market economy. This prediction has apparently suffered setbacks, in part because of the proliferation of religiously based, strongly autocratic societies (primarily in the Muslim world), in part because of the remarkable success of the Chinese version of "state capitalism". But an argument can still be made that Fukuyama's prediction will win out in the end. At any rate, this issue has a strong sociology-of-knowledge component to it. The debates largely revolve around the question whether a liberal, democratic society offers the best conditions for the generation of scientific knowledge and for its transformation into new consumer goods, produced according to innovative, knowledge-intensive methods.

Another important issue is international politics. What is the division of labour between the industrialized nations and the rest? Industrialism spawned a celebrated theory about this relationship, that is, the classical Leninist theory of colonialism, which told a story about how the industrialized world acquired overseas colonies as sources of raw materials for its industries and as captive markets for its products. Is that model still valid, or has some parity between the old industrialized world and the "third world" been achieved through the outsourcing of highly skilled job functions in the new "knowledge economy"?

Related to this are questions about the future of nation-states. Now that production processes have come to be much more globally distributed, they are no longer constrained by regional cultural traditions, local trade unions, or by communication in the medium of small national languages. These were all features of the traditional nation-state; thus, globalization brings with it an increased challenge to the future viability of the nation-state as the locus of economic activity. On the other hand, most nation-states in the developed world are currently busily developing national research strategies, with a view to strengthening their national economies. This, too, is a topic that would merit investigation within the framework of sociology of science.

6. In the context of the present book, however, in which we frame STS as an episode in the naturalization of our understanding of science, it is more pertinent to examine the prospects for further work in this direction. Let us examine what lessons have been learnt from the preceding investigation.

First, we must stress that the demand for a naturalized understanding of science persists, even though the particular version we have examined came to grief. Great intellectual interest still attaches to perpetuating the naturalizing agenda chronicled at the beginning of this book, which is simply to say that we still want careful empirical recording and, if possible, also explanation, of every region of empirical reality, including science itself. No part of empirical reality is exempt from such scrutiny; if the outcome occasionally undermines certain cherished, idealized conceptions of the area examined, then so be it. Next, the outcome of such investigation should be put at the service of the classical normative project of the philosophy of science, that is, identifying the characteristics of good science. We remember that STS owed its favourable initial reception to the widespread feeling that traditional, a priorist philosophy of science had come to the end of the line; this was mainly the legacy of Kuhn's work. That situation still persists today, even though STS's attempt to make it a platform for a deconstruction of science misfired. An attempt to fashion an a priorist, normative philosophy of science still faces exactly the same obstacles as before STS appeared on the scene. The prospects of an a priori approach have not been improved in any way by the demise of the critical naturalization project of STS.

The agenda of a reformed naturalistic science of science will be directly contrary to that of STS, however. Where STS wanted to show science to be social through and through, the reformed discipline will highlight those aspects of science that make it answerable to non-social reality. In this way, it will produce materials for a naturalized philosophy of science that shares with its antecedent the agenda to improve upon science.

First, a general observation. We have seen how the ambition of some of the main representatives of STS – especially Latour and Pickering, in their different ways – was to lay the foundations of a basic "social science of science", independent of underlying disciplines. Interdisciplinarity and syncretism were firmly rejected. This ambition reflects an attitude of *reductionism*, although its propagators vehemently deny it. At any rate, there could not be a basic social science, since this would presuppose that society is a closed system. There will always be interference from other systems, however, both from the external environment that envelops society and from its "inner environment", that is, the human beings who operate the social system. (We remember that Latour tried to block investigation into the latter influence by a "moratorium" on the psychology of science.) Sociological theorizing will always reflect the character of the particular societal institutions it examines, which again reflects the diverse natures of the sectors of reality with which those institutions interact. Sociology will thus always be "hyphenated", encompassing a distinctive sociology-of-science, a sociology-of-religion, a sociology-of-art, or a sociology-of-gender, with no hope of finding a universal set of theories and theoretical vocabularies with which to unify all these scientific specialties (cf. Collin 1985).

7. Next, let us turn to some more specific features of the proposed future sociology of science. STS celebrated the entanglement of science and social interests, attempting to make the former more accountable to such interests. We have seen how this ideological concern led STS astray and made it loosen its empirical moorings. By contrast, a reformed STS would precisely try to identify those social mechanisms that make science more responsible to reality. Such mechanisms are located at both micro-, meso- and macro levels. Among the first mentioned are those norms that counteract the tendency of academic debates to grow conceptually amorphous. Among the second are the organizational mechanisms that oppose disciplinary balkanization of the academic world; these are currently swimming against a strong ideological tide that favours the proliferation of ad hoc research centres. Among the last-mentioned are the rearguard forces that fight for the autonomy of the universities against efforts to make them docile tools of industrial policy. Thus, rather than celebrating local societal concerns, STS would highlight the universalistic features of science that make it more resistant to societal influence. What these features are would be discovered empirically, as is indeed the very naturalistic thrust.

The attempt to strengthen the universal aspects of science is akin to the strategy recommended in Fuller's Social Epistemology, although he prefers to talk of a "global" aspect instead. However, there are a number of major differences between the road suggested here and Fuller's (and the rest of STS'ers). In the first place, the discipline I envisage here is narrower than the one proposed by Fuller. It is a sociology of science, geared to providing empirical materials out of which the philosophy of science may build its models of good scientific practice. But the sociology of science has other tasks in addition to that of assisting the philosophy of science, as I suggested in the previous section; hence, it ought to retain its disciplinary autonomy. Fuller's Social Epistemology, on the other hand, is meant to be a successor both to philosophy of science and to classical epistemology. Moreover, the proposed sociology of science is not committed to the assumption that the true subject of human cognition is society rather than the individual, but merely elects to address issues that pertain to the prime example of societal cognition, that is, science.

These are largely matters of division of labour and of institutional organization, however. A more important difference is that the discipline recommended here would be "veritistic", in Goldman's sense (1999). It would aspire to tracing truth, not only truth's surrogates. Now, as Goldman emphasizes, veritism is a very inclusive label. Specifically, it does not imply a commitment to realism, since the kind of anti-realist stance adopted by, for example, van Fraassen (1980), under the name of "constructive empiricism", can be reconstrued veritistically as a methodology that requires theories to have *true observational implications* (Goldman 1999, p. 244 f).

Nevertheless, I would recommend the further step that STS adopt realism, if not necessarily as a basic philosophical commitment then at least as a methodological stance. This is the course most consistent with the universalism I recommend for STS. Instrumentalism tends to dull the inherent impulse of science towards ever greater unification and, hence, universality. This urge is spurred in particular when theories in adjacent but partly overlapping fields are found to involve incompatible assumptions. The paradigm example of this in current natural science is the incompatibility between certain fundamental assumptions in quantum mechanics and in general relativity theory. To the universalistically minded theorist, this is a thorn in the flesh. To an instrumentalist, on the other hand, there is no reason for concern: Since theories are never meant to be representations of reality, but only instruments for its effective handling, the only adverse consequence is that in approaching reality, we have to carry a somewhat larger tool-chest, since there is no single universal implement. This stance means that the push towards universality is weakened; this is a methodological liability, since it prevents us from reaching more universal theories if such happen to exist.

Whether methodological realism is ultimately fruitful is an empirical matter. We must remember that our dismissal of current STS has done nothing to lessen the force of the standard philosophical arguments against realism. Chief among them is the "pessimistic meta-induction", to the effect that all currently accepted theories will someday be rejected, thus dashing our hopes for such long-term cumulativity that could be taken to indicate science's approximation towards ultimate truth (Putnam 1978). We may point out, however, that a veritistic, realistic interpretation of science is tied to the reformed STS in a mutual dependency that might hopefully be of the nature of a *circulus fructuosus*: Findings within the sociology of science might lead to improvements in scientific practice, which might make scientific developments more consistently cumulative over time. In terms of such improvements, a realistic interpretation of scientific theories might become increasingly plausible.

8. A reformed sociology of science would proceed by first carefully recording and documenting the central features of a broad spectrum of different scientific practices. This investigation would cover both detailed features of methodology, criteria of theory selection, instrumentation and social organization. Next, it would compare the results generated by these different methods for their reliability. Finally, it would rank the methods on this dimension.

The approach would embody and further extend a perspective upon science as a multi-layered "inductive machine", already suggested by Mary Hesse a generation ago (1974). At the lowest level, science generates inductive generalizations (inductive hypotheses) on the basis of observational data and selects among them by means of methodological principles, such as simplicity, fruitfulness and ontological neatness (referred to by Hesse as "coherence conditions"). At the next level, which represents a longer time perspective, science makes "second order inductions" on the successes and failures of the methodological principles variously used at the first level, grading them according to whether or not they produced valid ground-level inductions in the long run. A negative assessment may contribute to those fairly rare large-scale shifts in methodological orientation we call "paradigm changes", but minor shifts of this kind, typically tacit, occur more frequently.

What is suggested here is that the sociology of science assist in making this process more explicit and systematic, by strengthening the instruments whereby scientists articulate the methodological principles embodied in their practice and by recording the way that such principles are invoked in decision-making. The main contribution of the sociology of science, however, would be to enrich the model with the social parameters of scientific production, such as the structure of research institutions, organization of the training of Ph.D.'s, publication patterns, distribution of funding and material factors, such as instruments, in order to record their impact on the efficiency of the scientific "learning machine". From this empirical research would eventually flow normative implications with respect to the conduct of science.

The sociology of science would thus form a part of the overall scientific enterprise construed as an inductive machine that reflects on its own practice, in order to improve its performance in a bootstrapping operation. It would consist, first, of an object level, constituted by various empirical sciences, each in its turn divided into at least the two methodological levels mentioned two paragraphs ago. Secondly, there would be a meta-level, consisting of the sociology of science with the first level sciences at its object, and itself displaying a bipartition into a stage in which simple hypotheses are produced, and a stage of selection between them on the basis of various methodological criteria.

In the suggested bootstrapping operation, it is clear that not everything can be undecided, if the process is to get started at all. In the first place, like all empirical sciences, the reformed sociology of science canvassed here assumes the validity of induction, leaving any scruples on this count for general epistemology to worry about. Secondly, it assumes that confrontation with reality in the form of observation and experiment is the ultimate arbiter of theory; in this, it adopts a staunchly empiricist stance. Thirdly, it would abide by a principle of inter-level consistency akin to the Strong Programme's principle of reflexivity. The discreditation and elimination of a methodological principle on a given level must lead to its elimination at all other levels, including the meta-levels at which the sociology of science itself operates. Finally, it would be committed to all generally accepted logical and mathematical principles. All the rest is, in principle, up for grabs – that is, left for empirical investigation to decide. This is also true of the inevitable choices that have to be made with regard to when to accept falsifying observations or when to reject them by invoking disturbing factors, how long to wait before an old theory, or method, is rejected, and so on. All these are questions for which classical philosophy of science has in vain looked for a priori answers.

The above stipulations should dispel such suspicions of circularity as are frequently raised with respect to efforts of naturalization in epistemology or philosophy of science: The proposed scientific project is not supposed to be self-contained or self-validating but is securely grounded in generally accepted epistemic principles of a kind dear to orthodox philosophy; the defence of these principles, if any can be provided, it is happy to leave to the philosophers. But perhaps the project is open to a more practical objection: With all the myriad variables involved, we are faced with a task that would require the entire future history of mankind for its completion. The answer is that the history of mankind and the history of science in particular *are* already, among other things, a laboratory for the testing of our epistemic strategies whether we like it or not; we are willy-nilly the subjects of a cognitive experiment of world-historical duration. What is being suggested here is merely that the sociology of science lend a helping hand in making the process more explicit and systematic.

Nor is it an undertaking that need be completed in order to give a return on our investment; any interim result that might improve scientific practice would count towards justifying our expenditure of time and effort.

We may note that the process might be sped up by computer similation of relevant test parameters, based upon the huge body of data already accumulated in the past – that is, the record of the entire past history of science including details of its instrumentation and social organization. Technical assistance in this enterprise could be got from the fast-growing neighbouring discipline of *learning theory*, a heavily formalized and mathematized field that aims at developing algorithms whereby cognitive systems may generate correct inductive hypotheses about their environment. This discipline has produced numerous non-trivial results that could be employed by the sociology of science; there are indeed strong attempts to extend this discipline towards issues central to the philosophy of science (Kelly et al. 1997). The main difference between learning theory and the approach suggested here is that the former aims chiefly at methods of *discovery*, testing mainly formal strategies of inductive generalization, while the latter aims at a method of *assessment* and considers discovery to be out of reach.[70] Equally significantly, the proposed sociology of science would also model such "material" aspects of science as manner of organization and publication patterns. Still, there are, no doubt, lessons to be learned from the sister discipline.

Another source of inspiration for the proposed discipline is current work in computer modelling of man's hypothetized theory-generating cognitive powers, such as the work of Paul Thagard (1988). In recent publications by Nancy Nersessian, an attempt is made to work data on the socio-cultural conditions of discovery into such models (Nersessian 2008).[71]

Thus what I propose is a role for the sociology of science as contributors to a *normative, naturalistic* approach to science. Efforts falling under the latter rubric indeed exist already, one of the most prominent ones being the programme laid out by Larry Laudan in a series of publications (Laudan 1984, 1987, 1990, 1996). However, Laudan questions the viability of construing science as truth-tracking, i.e. as aimed at the ever closer approximation to truth, taking it to be in the business of puzzle-solving instead. By contrast, the approach suggested here is squarely veritistic, in Goldman's sense.

The policy adopted by STS so far has been precisely the opposite of the one proposed above, although Fuller is a partial exception. Instead of deploying its resources to strengthen science's grip on reality and the development of universal standards of scientific method, STS has eagerly mobilized the familiar roster of philosophical arguments to discredit a realist interpretation of science. In the interest of making science more democratic and sensitive to local concerns, it has celebrated the local, fractured character of science. But a consistently naturalistic sociology of science would surely have to allow the empirical possibility of science using self-reflection – that is, the scientific study of science – to develop a non-relativist methodology that would safeguard a realist interpretation. Behind STS's negative stance clearly lies a fear of the ultimate evil, that is, the institutionalization of science as an elitist enterprise run by experts in the various disciplines, the whole machinery propelled by lofty ideals of scientific method that dictate a strict separation between "internal" scientific values and "external", societal concerns. But however much sympathy one might have with this political agenda, it should not be allowed to blind its adherents to the theoretical possibility of conducting science in a way approximating the strictures of the classical conception. As for Fuller, his efforts to promote a more universal ("global") yet non-transcendentalist orientation in science are weakened by his constructivist commitments, which are philosophically troublesome and alienate him from possible allies.

9. If Science Studies are to contribute to a realist, naturalized philosophy of science, they will have to update themselves on recent developments in general epistemology. Their current position is typically based upon an outdated, foundationalist epistemology that remains an embarrassment, even if its adoption was dictated by strategy rather than a genuine commitment. It served as a premise in an argument proceeding by *modus tollendo tollens* rather than *modus ponens*, leading to the negative conclusion that the results of science could never be warranted in terms of the philosophers' favoured epistemic principles. There are now epistemic stances available, however, that mesh beautifully with the aims of a reformed, veritistic sociology of science. One is reliabilism; another is coherentism. I shall now argue that both would have to be invoked.

The function of reliabilism in the overall picture becomes clear if we draw a comparison between the case of the individual cognizer and social cognition in its most advanced form, that is, science. As we saw in Chapter 1, cognitive science quickly realized the inadequacy of the rationalist picture of individual human cognition embodied in "Good Old Fashioned Artificial Intelligence" (GOFAI). This approach projected the machinery of classical foundationalist epistemology into the human mind, or brain, looking for counterparts to such epistemic functions as generalization (induction) and deductive inference. The structure of foundationalist justification or assessment were to be rediscovered in man's cognitive apparatus.

However, it quickly turned out that this model did not work. This was the result of a fascinating process, through which ancient and venerable doctrines of epistemology were put to an empirical test. The chief instrument of this process was the digital computer. If the human cognizer operates according to the principles of classical foundationalist epistemology, it must be possible to simulate his cognitive powers by means of a digital computer running programmes that capture the sentential framework in which cognition essentially consists. This framework consists of a complex hierarchical structure, ranging from the most abstract to the most concrete levels, tied together by intricate inferential relationships. Focus was upon deductive inferences, which would permit the move from higher-level generalizations to lower-level ones, and, in combination with singular sentences, eventually engender singular sentences detailing concrete facts. There was less focus upon the reverse, inductive movement from concrete data (singular sentences), corresponding to the observation sentences of classical epistemology, to lawlike sentences subsuming them. Obviously, a complete model of human cognition would have to embody a complex cluster of inductive principles with which to extract general information from a stratum of observation sentences.

At any rate, this carefully designed digital counterpart of the Cartesian "ghost in the machine" never managed to deliver what was hoped for it. Certain principled obstacles proved insurmountable, such as the "framework problem", which exposed a weakness in any system organized according to a deductivist, top-down architecture. Such a system may be brilliant at inferring implications from its stock of general premises once it knows which ones are relevant to the problem at hand. Identifying those premises, however, is not in itself a deductive task; instead, various heuristics must be resorted to that are not themselves deductive and the nature of which is in any case moot. Some philosophers, such as Hubert Dreyfus, located the shortcomings of the digital computer as a model of human thought at an even deeper level: Not only are the sentences expressing human knowledge not organized in a deductive hierarchy; most of our knowledge does not even assume a sentential (propositional) form at all, but is instead implicit, intuitive and, above all, embodied in man's very corporeal being.

Instead, neural networks provided a more adequate simulation of the workings of the human cognitive system. According to this model, cognition is not a linear process, but is distributed in a multidimensional network of parallel processing units arranged in multiple layers. The network is made up of nodes, each of which is linked up with neighbouring nodes, in such a way that signals from this cluster are passed on to further nodes in a pattern in part determined by the past history of signals received by each node. In this way, the network processes information fed into the network, in the form of inputs to its topmost layer of nodes, eventually producing an output in the form of a transformed signal.

It turned out that by varying the frequencies with which nodes would retransmit signals, it was possible to generate almost any desired functional correllation between inputs and outputs. In particular, it was possible to "train" a suitably tuned network to produce outputs that would closely match human performance with respect to simple cognitive tasks. The network's output could be made to simulate cognitive processings of the information provided in the input. In this way, it turned out to be possible to produce networks that could handle simple visual discrimination tasks (but eventually including such complex tasks as facial recognition), divide words correctly, and so on.

The neural network model is non-rationalistic: just by inspecting the structure of the information flow, one cannot deduce that it will generate valid knowledge. Assurance in this regard has to be found elsewhere: namely, in an assessment of the outcomes themselves. On this point, the output generated by artificial neural networks are in exactly the same situation as cognitions produced by our real neural system. The principles according to which our neural system operates do not carry their epistemic credentials on their sleeve; in fact, the more we learn about that system, the more we realize that it runs on heuristics that are "quick and dirty" and provide no a priori guarantee of truth. (Notably, the familiar visual illusions are precisely the result of the inevitable imperfections of these heuristics.)

Fortunately, there is now an epistemic theory available that will take care of this problem. *Reliabilism* is designed to overcome the deadlock of traditional justificationist epistemology that all too often made it gravitate towards scepticism. Reliabilism says that we may be said to *know* the truth of a given sentence, even though that sentence is not supported by intrinsic evidence of the kind stipulated by the familiar foundationalist or coherentist epistemologies (Goldman 1986). That is, the sentence in question does not follow from certain other sentences known to the subject by deductive logic, nor according to the logic of "inference to the best explanation", nor indeed from inductive generalization of similar previous instances. Instead, the sentence may be held true, and even count as *known*, if it is produced by a cognitive device that produces truths most of the time. In this manner, we may trust the "evidence of our senses" – that is, the spontaneous beliefs that we form when we use our senses to investigate our surroundings, although we do not have the slightest knowledge about how the relevant sensory organs operate.

10. All these insights derived from reflection on artificial neural networks and on advances within general epistemology may now be transferred to the field of Science Studies. In this area as well, the *network* has been the metaphor of choice in efforts to capture the societal structures that produce scientific results. Actor Network Theory provides the most explicit and elaborate employment of the metaphor; we saw it at work in Bloor, Barnes and Collins as well, and certain formulations in Fuller also bring the network to mind (Fuller 1989/1993, pp. 98–99). For these authors, the ultimate source of the network concept was Quine, from whom it was borrowed and adapted in Mary Hesse's model of scientific theorizing. Quine's network model of (scientific) knowledge is indeed closely conformal with the neural network model. It is a model of knowledge, doubling as a model of learning. It contains an input layer of nodes, the activation of which roughly corresponds to sensory inputs (observation sentences); these are tied, through numerous layers, to deeper levels of cognitive structure (theoretical beliefs and sentences). An input to the system creates a disturbance that is gradually diffused and dampened by the interlinkage of sentences, but not without leaving a permanent trace. This trace is manifested in a changed output pattern in the cognizer, that is, a changed propensity to utter certain observation sentences when in the vicinity of certain objects. In STS, the linkage between sentences, which to Quine are due to reinforcement by operant conditioning, are reinterpreted as social links, expressing the various interests of the parties tied together in the network and their resulting willingness or reluctance to give up a certain epistemic commitment.

We noted above that a reliabilist epistemology dispells the worries we might have concerning the well-foundedness of beliefs generated by the neural networking mechanism. This feature, too, transfers from the individual case to scientific knowledge; we need not worry about the lack of inherent rationality of the societal microprocesses of which science consists. It may be true that scientists are mainly motivated by the prospect of academic credit or fame and that the interaction between scientists is to a large extent a matter of bargaining or politicking. The vicissitudes of these processes were brought out well in Latour's work, and in related laboratory studies carried out by Karin Knorr-Cetina and others (Knorr-Cetina 1981). Yet none of this would necessarily rob scientific products of their validity, if the overall social structure of research activities were designed in a manner so as to enhance the reality-sensitivity of the overall enterprise. To ascertain that validity, however, we have to go to the products themselves. It does not reveal itself to a scrutiny of the detailed interactions between researchers, nor by reflection on the overall structure of the institutions of science. We have to measure the validity of scientific theories against observational data concerning the output of these institutions.

There is a crucial difference between individual and communal cognition (i.e., science) in this respect, however. On a strong interpretation of the neural network model of individual cognition, nothing proposition-like is to be found inside the network; hence, nothing is truth-evaluable there. Matters are different in the case of science, however. Here, we have a mixture of elements that are propositional and truth-evaluable and elements without this distinction. Among the former are abstract theoretical formulations, often heavily mathematized; among the latter are instruments, laboratory procedures, modes of organization and financing, conventions of scientific publication, and so on. Traditional philosophy of science highlighted the truth-evaluable elements, to the virtual neglect of the rest. STS has rectified this flaw, but at the expense of the theoretical elements, even denying that truth evaluation is relevant to an understanding of the role of scientific theories at all. We need to be able to interpret what goes on inside the scientific network in such a way that its theoretical components are assigned a truth value.

This is where coherentism enters the picture. The key idea is simply to put an epistemic gloss upon those models of scientific networks with which STS'ers have provided us with, and have illustrated with so many intriguing case studies. To STS'ers, scientific networks are formed by agents (or "actants"), who try to further their individual, diverse interests by adopting each others' results. The way to effect the cognitive transformation is by suggesting that whenever scientist A incorporates results generated by scientist B in his or her work in such a way as to strengthen A's credibility, this reflects back upon B's results and provides prima facie evidence for them. In this way, social networks are turned into epistemic networks of the kind examined by coherentism.

This move is based on the premise, of course, that the entire interlocked structure is sensitive to the influence of non-social factors at some point; in short, that it is sensitive to the impact of reality. Otherwise, the entire construction becomes vulnerable to the classical objection to (an overly strong) epistemic coherentism: that any large, internally coherent structure of statements will eo ipso be epistemically validated and count as true. This leads to a rampant proliferation of truths and, hence, to a plurality of parallel realities. We must ensure that the entire structure is securely moored to procedures whereby it is confronted with reality and that observational data are not invariably overruled by theoretical considerations. This reality check is secured by the reliabilist element of the overall epistemology.[72]

11. However, there are clearly challenges to the application of coherentism both in the doctrines and in the empirical findings of STS. One is that the items that make up networks are not merely propositions and other theoretical elements, but even material items such as instruments and social features such as organisational structures; these heterogenous elements cannot cohere in the epistemologist's preferred sense. This problem must be handled by tying these non-propositional elements to the theoretical ones by virtue of the former's function as items needed for the testing and dissemination of the latter. To achieve this, these functions must themselves be put into propositional form, that is, they must be carefully articulated in propositional accounts. As part of this process, laboratory apparatus must no longer be allowed to feature as mere "black boxes", but must be "opened up" so that the theoretical assumptions on which their construction is based can be made explicit. Once rendered explicit, these assumptions form part of the over-all propositional network that they serve to extend and strengthen and from which they in turn receive evidential support when the epistemic output of the network is found to be reliable.

There is another, more specific worry. In the ongoing debates about coherentism, one of the crucial issues is the nature of the relation that makes a set of items cohere. This is normally construed as explanation, or logical inference. The findings of STS indicate, however, that the actual relations tying networks together are different from, and rather weaker than, what is assumed by coherentism, even when we restrict ourselves to purely theoretical elements. A common theme to all STS writers, as we have seen, is the looseness of semantical and inferential links involved in scientific theorizing. Bloor and Barnes stressed the flexibility of terms that follows from Wittgenstein's rule following considerations and that implies the unrestricted revisability of observational corollaries of theories. Latour emphasized how intellectual content is transformed and translated every time a new actant joins the network. Pickering demonstrated how intellectual elements, too, are "mangled" throughout scientific practice. Fuller pointed to the large information loss suffered in the transfer of scientific results. We have not accepted all these claims, or indeed any one of them in the strong forms in which their authors presented them; still, they hold a kernel of truth that considerably weakens the inferential support that the network delivers to each of its component nodes.

The challenge to coherentism is at the same time a challenge to the realist interpretation of scientific theories. The latter is obviously weakened if a case can be made that science is not a rigid propositional structure of clearly defined theoretical nodes, held in place by firm inferential links, but is a shaky structure composed of loose, forever-transmuting conceptual elements, held together by an alliance of parties who see an advantage in such collaboration. The latter picture supports the conclusion that the trajectory of science is largely contingent and that other alliances, between other players, would have generated different theories with an equal claim to truth. In brief, they point towards an instrumentalist interpretation of scientific theories.

The good news, however, is that the empirical discovery of these weaknesses constitutes the first, minimal step towards mending them. We can now go on to address the problems, in the same way that other discoveries of weak elements in the scientific process has led to measures to amend them. This is the bootstrapping policy we have been recommending, which STS tried to block by invoking a priori, philosophical arguments to prove that science could never raise itself above the sphere of bargaining and politics. The old debate between realists and instrumentalists goes on, even after the play made by STS to end it once and for all in favour of instrumentalism. What was overlooked by STS was that any detailed empirical documentation of those features of science that support an instrumentalist interpretation of the entire enterprise – the frequent looseness of its joints, the intrusions of power and politics – may also be read as a recipe for their emendation.

12. As should have become clear, the naturalization of philosophy of science suggested here is not a total one – that is, the radical elimination of non-empirical elements in the understanding of science. The suggestion is rather that philosophy of science enrich itself with empirical elements, culled from a reformed sociology of science geared to that very project. We saw that progress beyond the stance adopted by STS so far can only be made because novel philosophical theories about the nature of evidence have become available. The lesson here is that a naturalized philosophy of science must constantly keep track of and incorporate such new developments from general epistemology.

There are other points at which philosophy will have to be invoked: for instance, in the assessment of the coherency of a given body of knowledge. Here, philosophical techniques of conceptual analysis will be employed to record shifts in conceptual content, together with tools from the philosophy of language to check the constancy of the meaning of terms across generational gaps and disciplinary shifts. Philosophical techniques are also needed to elucidate the logical structure of scientific theories, the logical positivists' special area of interest of old.

Finally, there is a need for philosophy to police adherence to the general principles of cognitive coherency. There is a constant danger that a critical science of science will apply tools that somehow undermine its own credentials. The Strong Programme tried to forestall this problem by setting up its Reflexivity Requirement, to the effect that the patterns of explanation (and other methodological devices) adopted by Science Studies would have to be applicable to sociology of science itself, without generating incoherency. In theory, this will indeed take care of the problem, but there were massive problems of compliance, since the Edinburghers would in practice dismiss any charge brought against them to the effect that they failed to live up to their own principle; such charges mainly revolved around the problem of relativity. Similarly, we have seen how Harry Collins would (in vain) develop his curious methodology of "meta-alternation" to tackle the problem; had it not been for a lack of space, even more striking illustrations of the phenomenon could have been found in the work of other, minor STS figures. Where Collins tried to tackle the problem of relativity by the sequential adoption of different epistemic platforms, alternatively located at the object level and a meta-level, Steve Woolgar and Malcolm Ashmore recommended the *simultaneous* adoption of plural theoretical perspectives, and the occupation of multiple writer positions, in scientific publications (Woolgar and Ashmore 1988). Thus the problem of relativity was to be solved by letting the representatives of every position have a say. In practice, this meant that the write-up of STS research results would comprise a plurality of authorial voices, each representing a different epistemic perspective upon the object of investigation. Later, Michael Mulkay went on to experiment with unorthodox literary forms in scientific writing, such as parody and irony, to eliminate any remaining trace of authorial authority (Mulkay 1991). In the rest of the STS community, there was general agreement that this road could only lead to the eventual self-destruction of the entire enterprise.[73]

However, Woolgar and Ashmore's recommendation had the virtue of squarely facing the problem of reflexivity, while most mainstream STS'ers chose to sweep it under the carpet. The conclusion seems clear, from this episode as well as from the general history of science, that empirical scientists are not well equipped – nor indeed very willing – to do the work of conceptual clarification and reflection on epistemic fundamentals that is needed to stay clear of methodological or metaphysical minefields. Philosophy is needed for this task.

13. Let us finally turn to the issue of explanation that has loomed so large in this book. What stance should a reformed sociology of science adopt on this point? Let it be said at the outset that there is no reason why only one single mode of explanation should be adopted. Once the dream of a unified, basic "Science of Science" is given up, so should the inclination to legislate that only one particular kind of explanation is allowed; STS'ers have typically be much too dogmatic on this point. Consequently, I shall abstain from any narrow recommendations, but instead just make a few general points.

First, we should accept the old Edinburgh slogan that both good and bad science calls for explanation. The issue as to whether they call for the "same" explanation, on the other hand, is a red herring, as I have argued. Given the protean nature of "sameness", that question lacks any substantial content. There are other dimensions, however, in which the sociology of science would do well to display modesty. Thus, I have argued that it must restrict itself to explaining the *reception* of theories, not their *inception* (Chapter 3, Section 8).

Next, I would urge that the sociology of science drop the STS ban on explanations tracing the antecedents of scientific beliefs all the way back to the physical realm. Let us call accounts which do so abstain *proximal* accounts, while *distal* accounts move beyond the social sphere. The inputs to a proximal account are observations, and the account traces the way such inputs are processed in the societal practice that constitutes science. Distal accounts, on the other hand, trace the explanatory trail all the way back to the physical events that are the ultimate source of the observational data. Thereby, an asymmetry is inevitably established between accounts that demonstrate a harmony between physical reality and the theories devised to explain it (i.e., *true* theories), and accounts that reveal the resulting theory to be *false* (cf. page 39). Hence, according to STS, we should restrict ourselves to proximal explanation, in which symmetry reigns.

In the course of our investigation, we have encountered a number of arguments why the sociology of science should eschew distal explanation. I shall briefly review these arguments, along with the rebuttals of them that I have offered along the way, and add a few more arguments.

Let me preface the account with the remark that, when we discuss the distinction between distal and proximal explanation, we are moving within a framework of *causal* explanation. These I would construe roughly along Hempelian lines, at least with respect to the requirement that they presuppose lawlike generalizations of a certain strength. No doubt, in the social sciences only imperfect generalizations will be found, which will adversely affect the explanatory tightness of the explanations; this is different from their validity, however. Generalizations within the social sphere can probably never be made perfectly tight since they are *heteronomic*, to use Davidson's term (1970). That is, the attempt to make them tighter will eventually force us to shift to a more fine-grained, reductive level of description that will transport us out of the social sciences altogether.

Let us now turn to the arguments. The most radical argument was provided by Latour, who derived his stance directly from the constructivist nature of his approach. This was accommodated in his Third Rule of Method:

> Since the settlement of a controversy is the *cause* of Nature's representation, not the consequence, we can never use the outcome – Nature – to explain how and why a controversy has been settled.
>
> (Latour 1987, p. 99)

I shall not comment here upon the sloppiness of this formulation, which allows numerous interpretations of the rule. In Chapters 6 and 7, I spent considerable time trying to establish the nature of the constructivist stance behind it. Here, I shall restrict myself to the observation that this constructivist attitude has constantly got STS'ers into trouble; my recommendation has been simply to drop it. I shall not argue the correctness of the basic position here, beyond the observation that the Dummett-style anti-realism towards which Latour's constructivism gravitates in his later work was never transferred successfully (by Dummett) from the realm of mathematics where it was originally developed and into the realm of empirical knowledge, including science. The crux of the matter resides in the much less clear conditions constituting "conclusive verification" of a scientific claim, as compared to their mathematical counterparts. Besides, there are specific problems with anti-realism with respect to the past, which even Dummett would later concede (2004).[74]

Let us next turn to an argument against distal explanation which I presented in Chapter 8 as a possible defence of Pickering's position on this score. According to instrumentalism, explanations are tools we use to gain an intellectual grasp of phenomena. The ultimate goal of such understanding is "coping", that is, the power to predict events and to change their course if we wish. No proper purpose is served, however, if instrumentalist posits are invoked in the context of explaining the *genesis of scientific beliefs* involving those very posits, which is precisely the context in which the sociology of science operates. Indeed, such explanations could only serve to obfuscate the point that, but for social contingencies, an altogether different theory would have been adopted, accompanied by an equally persuasive story of its genesis. This is precisely the obfuscation that Whiggish historiograhy is designed to achieve, and that STS tries to dispel.

This argument collapses, however, once we adopt a realist ontology. The purpose of science is henceforth not (only) to help us cope with reality, but to get an idea of its inherent constitution and how we manage to form an adequate conception of it. It must surely be a part of any complete account of the world to enlighten us, not only about how nature is constituted, but also how we come to know this fact.

Another argument is found in Barnes and Bloor. This is the observation that material reality is not a useful explanatory resource in the sociology of science, since reality is (supposedly) one, while scientific interpretations of it are legion. We cannot explain multiplicity by unity or identity, but need to appeal to causes that are equally diverse, with social variation offering itself as the obvious candidate. The sociology of science is precisely oriented towards recording this variation and the way it engenders different views on reality (Barnes and Bloor 1982, p. 34).

The obvious rejoinder to this argument is that, at least as far as historical variation is concerned – which is primarily what is documented in STS's celebrated case stories – the difference between our current science and that of the Greeks, or the medievals, or of any other epoch prior to our own, is primarily due to the dramatic difference in the amount of data at the disposal of scientists then and now. Science is data-driven and the divergence between the scientific outlook of different epochs can be traced back to differences in this parameter. This is not to deny, of course, that data require theories and instruments for their identification and collection; indeed, these elements are irresolvably fused in science. It is only to say that as long as the role of data is granted at all, along with the continuous growth of their number, a realist will never be at a loss to explain the (historical) divergence and multiplicity of science.

Finally, let us consider a purely methodological argument that is no doubt an important concern among STS'ers, but only rarely stated explicitly. It is the simple observation that, in providing distal explanations, sociologists of science would stray beyond the limits of their competence. Such explanations essentially bring with them a commitment to a particular view of the physical universe. As sociologists, the investigators of science have no expertise in this area, hence, they should studiously avoid any such commitments. If Science Studies are to maintain their scientific legitimacy, they cannot undertake commitments beyond what their methods will sustain. The latter are those of the social sciences; hence Science Studies must remain studiously neutral with respect to the validity of the natural science under examination. This can only be secured if they refrain from tracing the causal antecedents of scientific beliefs all the way back to the physical realm. In other words, they must restrict themselves to proximal explanation.

This argument is sometimes given an extra twist. The view of nature adopted by the sociology of science when dealing in distal explanations would, by default, be that of current science. Thus sociology of science would surrender its autonomy to the experts of the disciplines it endeavours to deconstruct. This goes against the deepest ideological instincts of Science Studies, and is evidently a scenario they want to avoid at all cost.[75] Ideology aside, the proximalist policy is surely much too strict and abstemious. Any scientific discipline incurs commitments that it cannot make good in terms of its own native methods. There will be numerous auxiliary hypotheses involved, for instance, in the measuring apparatus and elsewhere, which the discipline in question cannot itself vouchsafe. To ban all such commitments would cripple science. In STS jargon, this is known as the dialectics between *topics* and *resources* in the conduct of science. Sociology cannot turn every aspect of non-social reality into a topic, but must treat certain things as unquestioned resources.

14. It is true, of course, that STS would often be well-advised to avoid distal explanation in areas where scientific controversy is intense – which happens to be precisely the areas arousing the interest of STS'ers. But, even here, STS would occasionally have something to offer by proffering explanations that could help us adjudicate the methodological soundness of rival scientific positions and thereby aid in identifying valid knowledge. If STS would forget temporarily its political correctness, embodied in its celebrated principle of "symmetry", it could help to decide matters in certain cases. Let me illustrate the point with a scientific issue the resolution of which would have profound consequences for our outlook upon mankind's position in the universe – and which, for the same reason, has generated both intense controversy among scientists and considerable interest in the general public. This is the problem of the existence of extra-terrestrial intelligent life. There seems to be very hard evidence indeed for a positive answer to this question, in the form of numerous, largely similar and mutually supportive witness reports about encounters with extra-terrestrials and UFO's, even including detailed stories about abductions. Here, the sociology of science might get up its courage and comment upon this state of affairs. Can these reports be explained away, as is normally assumed, by reference (among other things) to insights into the sociology of science, or more broadly the sociology of knowledge? Findings concerning the motivations and dynamics of knowledge claims, both within smaller groups and at the societal level, would appear to be highly relevant to this issue. To understand what is going on, the sociological angle would no doubt have to be supplemented with elements from witness psychology and other disciplines, but it appears clear that sociology of science could play an important part. At any rate, if the alleged first-hand testimony is allowed to stand, it must weigh heavily in favour of a positive answer to the question about extra-terrestrials. Such an answer would have a dramatic impact, among other things, on current discussions in physics, astronomy and biology concerning the frequency with which conditions for the emergence of life obtain in the universe, and on pessimistic speculations in social science to the effect that technologically advanced civilizations will self-destruct before they reach a level of sophistication, scientific and cultural, where they can establish contact with other civilizations. (It would also raise serious political and military concerns about how to tackle what might be interpreted as preparations for an invasion of our planet.) The sociology of science would do well to throw its weight in the balance in the adjudication of such issues.[76]

In addressing this kind of questions, the reformed sociology of knowledge I advocate could make a clear and concrete contribution to our understanding of the world in which we live, even outside of the social realm. Its main service to us, however, would still be that of helping to put the scientific enterprise on a more secure methodological footing in its everyday work. In my view, the pursuit of this goal is no less noble that STS's current political efforts to make science more accountable to society and more permeable to democratic control. Unfortunately, orthodox STS holds these two aims to be incompatible: According to this view, refining the methodology of science and thus strengthening science's credentials would inevitably involve reinforcing the elitist ideology that sustains the scientific enterprise, and hence further entrench the societal privilege of scientists as a professional class. Yet this view is only another unfortunate consequence of the constructivist bias of orthodox STS, according to which the "special cognitive authority" of science is indeed *nothing but* the social privilege traditionally bestowed upon its practitioners; it is all a matter of "social attribution". This conflation, however, falls away with the adoption of the realist, veritistic and reliabilist view of science I have advocated in this book.

Notes

1. This dimension is missed in Michael Friedman's otherwise highly perceptive analysis of the aims of STS, cf. Friedman (1998).
2. There is a direct line leading from these Durkheimian notions to key conceptions in current Science Studies, cf. Chapter 3.
3. Not all analytic philosophy is naturalistic, even in the broader sense of "naturalism". Frege (1918) and Popper (1972) indulge a more inclusive ontological scheme reminiscent of Plato, but it is fair to say that in this, they stand very isolated in 20th-century analytical thought. In this book, we look only at the dominant, naturalistic trend.
4. For a nuanced account of the relationship between Carnap and Neurath's views on these issues, see Uebel (2009).
5. For an excellent introduction to the history of cognitive science, see Bechtel et al. (1998).
6. Notable pioneers of externalism are Dretske (1981), Goldman (1986) and Nozick (1981).
7. This theme would later be elaborated by Norwood Russell Hanson, cf. Hanson (1958).
8. Cf. *Logik der Forschung*, (1934). Popper's criticism became available to the anglophone world only when this work was translated into English in 1959, under the title *The Logic of Scientific Discovery*.
9. In the following, I am much indebted to Enebakk 2005 for factual detail. Enebakk takes exception to many key points in the construal I put upon them, however (personal communication).
10. Some of the pertinent lines of disagreement were articulated in the so called *Positivismusstreit*, cf. Adorno (1969).
11. For a particularly eloquent homage to Kuhn in this respect, see Barnes (1982).
12. Merton had established himself as an important figure in the field already with his doctoral dissertation, *Science, Technology and Society in Seventeenth Century England*, published as Merton (1938). He followed suit with a series of influential articles, many of which are collected in Merton (1973).
13. In adopting this reverential stance, Merton was heir to a tradition in sociology of science with Marxist roots, known as *Wissenssoziologie*. In the *German Ideology* (Marx 1845), Marx had stated that human existence shapes human consciousness; but he would restrict this effect to the "Superstructure", the cultural sphere, and exempt mathematics and the natural sciences from such influence. The Marxian tradition, carried on by such authors as Karl Mannheim (1968), remained faithful to this stance.
14. Cf. Douglas (1966, 1970); Durkheim (1915); Durkheim and Mauss (1963).
15. Among the most important of the critical contributions were Laudan (1977, 1981), Newton-Smith (1977) and Flew (1982). Once the first shock at the Strong Programme's assault on philosophical orthodoxy had subsided, philosophers would start developing positions that would accommodate STS's insights into the social aspects of science, looking for a middle course between rationalistic myth-mongering and sociological reduction. For examples, see Kitcher (1993) and Haack (2003).

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16. In the final chapter, I shall critically address the argument that Science Studies should not deal in explanations that trace the ancestry of scientific beliefs all the way back to their physical causes.
17. We shall see later that a more radical position ensues if we take seriously the Strong Programmers' "finitism" that eventually robs experience of any say in the matter.
18. We may have general doubts about functionalist explanation, such as to the strength of the feed-back mechanisms that are needed between the functional requirements and the items that satisfy them; but I shall bypass such worries here; Cf. Hempel (1959); Nagel (1961).
19. Among them are the Friedman–Kitcher "unification" model of explanation (Friedman 1974; Kitcher 1981), and van Fraassen's "pragmatic" model of explanation (van Fraassen 1980). Pragmatic models of explanation, which currently dominate the discussion (cf. Faye 1999, 2007), should probably be construed as meta-models which may be held conjointly with (for example) causal or intentional models. They embody the position that there is no such thing as a complete explanation, since all requests for explanation are made from a specific point of view and reflect a particular cognitive (or practical) interest. By the same token, the pragmatic models renounce the kind of explanatory exclusiveness that Strong Programmers need.
20. Barnes discusses the relationship between these two kinds of interests in Barnes (1982), p. 114 f, but seems oblivious to their differential impact upon his theory.
21. For recent contributions to the philosophy of science that explicitly invoke the conception of science as truth-tracking, see Psillos (1999) and Roush (2005).
22. This point is missed in Barnes (1982, p. 103), where the author argues that no conflict exists between science being determined by interests, and its seeking truth.
23. For an alternative way to handle the objection to the Strong Programme, see Fuller (1988), p. 239 ff. There are traces of a similar argument in Barnes (1974, p. 148).
24. The only place where the distinction is observed is in Barnes (1982, p. 101); but it immediately gets conflated again on the following page.
25. For an attempt to further develop this idea, cf. Pettit (1993, p. 175 ff).
26. For a good illustration of the way that a particular interpretation of a concept may conversely be put to an ideological *use*, see George Lakoff's analysis of the concept of a *family*, in Lakoff (2002).
27. In Barnes (1982, p. 36), we see how Barnes slurs over this difference by a particularly loose use of the term "familiarity".
28. It is instructive to contrast and compare this with the conventionalist aspect of Popper's philosophy of science. Popper adopts a conventionalist attitude to observation reports in science, but *not* to theories (Popper 1959, p. 109). He prevents this conventional status from spreading to the theoretical level by a methodological rule to the effect that conventionalist moves must never serve to save theories from falsification (ibid., p. 82). That is, we must not conventionally adopt a description of an experimental outcome that is designed to save the theory from falsification. It is striking that, in *Conjectures and Refutations*, Popper presents an argument concerning the similarity of objects of observation that anticipates aspects of both Bloor's Wittgensteinian argument and Collins's reflections on the replication of experiments (Popper 1963b, p. 44 f). However, being a semantic realist, Popper does not take experimental reports to *define* the theoretical terms in question, thus avoiding the problem of semantic indeterminacy we examined above. In view of these differences, Bloor's complaint that Popper's philosophy is just as deserving of the strictures that have been directed at the Strong Programme is clearly misguided (Bloor 1976/1991, p. 159).
29. Cf. Barnes et al. (1996, p. 78). As it happens, Bloor offers an outline of a sociological theory that will explain, in general terms, why one policy rather than another is adopted, cf. Bloor (1983, p. 141 ff). This suggestion is inspired by Mary Douglas's grid-group theory presented in Douglas (1966). We cannot go into this issue here, however.
30. Surprisingly, there are very few comparative treatments of Quine-style and Edinburgh-style naturalistic approaches to science in the literature. Roth 1987 provides an incisive analysis, but misses the fact that the radical indeterminacy implications of the Strong Programme

render scientific controversies vacuous. Kusch (2002) is a philosophically sophisticated attempt to develop the kind of "communitarian" epistemology required to support the Edinburgh approach; but even Kusch fails to address the indeterminacy issue.

31. The question is moot as to whether Quine is entitled to the kind of semantic theory that accommodates this distinction. There are tendencies in Quine towards a fully holistic semantics, according to which the only possessor of semantic significance is the overall scientific network, with the implications, first, that two networks with the same global consequences have the same semantic import and, secondly, that there is no way to assign differential semantic import to isolated elements of the two networks.
32. In the famous formulation from *Word and Object*: "If we are limning the true and ultimate structure of reality, the canonical scheme for us is the austere scheme that knows no quotation but direct quotation and no propositional attitudes but only the physical constitution and behavior of organisms" (op. cit., p. 221).
33. He appears to reject such reconstruals of ordinary scientific disagreements in a comment upon Collins in Barnes et al. (1996, p. 74).
34. This point is relevant in connection with Bloor's rebuttal of what has been put forth as the "ultimate refutation" of sociological interest explanation, fortified by Wittgensteinian semantic indeterminacy, that is, that objects of interests themselves have to be "interpreted" by their subjects, that is, subsumed under determinate descriptions, which leads to an infinite regress if such subsumption can only be undertaken against a background of further interests, and so on ad infinitum (Brown 1989, p. 54 f). Bloor objects that interests may determine belief causally, without our reflecting upon them or interpreting them (Bloor 1976/1991, p. 173). Now it is certainly true that interests may incline us to adopt a certain belief without previous cogitation about the likely practical consequences of its truth. But interests cannot influence us *qua* interests, rather than *qua* physiological conditions of our brains, say, unless we can describe, in meaningful terms, the situations that stimulate those interests. For instance, the cognitive mechanisms described by cognitive dissonance theory (Festinger 1957) may cause an inveterate smoker to ignore the health hazard posed by smoking; this censorship mechanism is obviously deeply subconscious and does not involve explicit deliberation. (Indeed, it would cease to operate once the subject became aware of it.) But even this mechanism presupposes that the subject be capable of entertaining meaningful thoughts concerning the relationship between smoking and poor health. If no such thoughts could be attributed, we could not construe what is going on as involving interests being triggered by the prospect of achieving their objects at all. Thus the regress of description is re-established once more.
35. In Barnes (1982, p. 103), Barnes makes a remark pertaining to this issue: "Goals and interests bear upon the judgement involved in any act of concept application. But such a judgement can only be made if other concepts are assumed to have a routine usage which others will continue to follow, and which can accordingly be taken for granted as a stable feature when the judgement is made." Wittgenstein would wholeheartedly agree. The problem, however, is that, given Bloor and Barnes's views concerning the role of interests in concept application, the notion of "routine usage", untainted by interests, must be rejected as mythical.
36. These *sociological* discontinuities do not allow a distinction between "strictly epistemic" ("rational") concerns and "merely social" ones to re-enter through the back door, a distinction which Collins explicitly repudiates (see Collins 2002). According to Collins's concentric model, the differences between the rings are definable in standard sociological terms; that is, in terms of the functions or institutional affiliations of members of the rings. This is brought out by the historical case studies. The concerns active within the core set largely have to do with issues of scientific methodology. Different issues come into play when we move into the wider circle. Here, concerns are over funding and career planning.
37. The situation is well illustrated by Collins's interchange with Latour in the "Chicken" debate (Collins and Yearley 1992a, b; Callon and Latour 1992). Latour argues that physical reality must be included in the explanations, although only in the form of "actants". Collins rejoinders that only "agents" in the normal sense are required. Agency only accrues to things if it is granted to them by human agents, whereby agency returns to humans, in the final analysis. As

it happens, Latour is just as slippery as Collins is vague, with respect to the nature of explanation issue. The controversy cannot be resolved until a more precise notion of "explanation" is agreed upon.

38. Collins could also have invoked the familiar argument structure known as "Fries's trilemma", which points out that a justificationist conception of rationality leads either to dogmatism, to infinite regress, or to a vicious circle.
39. In Collins and Evans (2007), the authors make a brief mention of *reliability* of judgement as a mark of expertise (op. cit., p. 68). This too might be read as an opening towards a realist reconstrual of knowledge. However, as is made clear by the father of reliabilist epistemology, Alvin Goldman, reliabilism may be given a non-realist as well as a realist interpretation (Goldman 1999, p. 244 f); in the name of consistency, the former is the one we must attribute to Collins here.
40. In view of the haste with which the quasi-economical, macro-social underpinnings are abandoned, one may suspect that these served mainly as a bid for legitimacy in the heavily Marxist-dominated French academia of the early 1970s. The approach shows particular affinity with the "structural" Marxism of Louis Althusser (Althusser and Balibar 1965). The quasi-Marxist apparatus served as what Latour would later refer to as an "obligatory passage point", that is, a dogma to which one must pay ritual respect to get ahead in academia.
41. A similar point is made in Tilley (1981), provoking a response from the authors in the Postscript to the 2nd edition of *Laboratory Life* (op. cit., p. 281) which, however, rather dodges the issue. As it happens, in *Science in Action* (Latour 1987), Latour provides a much superior account of laboratory discourse by filling out the highly enthymemic arguments found in that setting with tacit premises. The procedure is governed by an implicit acknowledgement of the falsificationist logic of the situation (op. cit., p. 45 f).
42. This shift in Latour's theoretical orientation was inspired by the work of Michel Callon, cf. "Some Elements of a Sociology of Translation: Domestication of the Scallops and the Fishermen of St. Brieuc Bay", in Law, J. (ed.), *Power, Action and Belief: A New Sociology of Knowledge* (Callon 1986).
43. For a comment upon Collins's apparent essentialism with respect to the distinction between humans and non-humans, see Bohlin (2000).
44. The idea hails from Popper's notion of "verisimilitude", cf. Popper (1963c). Popper's proposal was challenged by Kuhn, Feyerabend and others who argued that the notion was rendered inapplicable by the incommensurability of scientific facts which made the counting of such facts problematic, cf. Kuhn (1970); Feyerabend (1970, 1975); also Popper (1970).
45. For a dispute between Bloor and Latour over the precise role of nature in the generation of scientific knowledge, see Bloor (1999a, b); Latour (1999).
46. On this point, Latour is influenced by Whitehead and his "process philosophy" (Whitehead 1929).
47. On this point, Latour's thinking is influenced in particular by Michel Serres's philosophy; cf. Serres (1983).
48. On a plausible reading, the ontology of the *Tractatus* also shares the Latourian principle that all existents are particulars.
49. Dummett (1964, 1969). References to Dummett's work are absent in Latour, however.
50. Dummett (2006), especially Lecture 4. Note that this work is a printed version of the Gifford Lectures originally given in 1999, which means that Dummett's radical stance in this book actually antedates the more moderate position in Dummett (2004).
51. A celebrated example of STS work emphasising the local character of construal is Garfinkel et al. (1981). This – highly controversial – study would provide a good illustration of what Latour's local constructivism would amount to in practice.
52. In Chapter 8 of *Pandora's Hope*, entitled "A Politics Freed of Science", Latour adopts an explicit division between what he calls "Science no. 1" and "Science no. 2" that closely mirrors that between Mode 1 and Mode 2. The latter is praised, whereas the former is made an object of scorn.

53. The phraseology may cover rather different things in Pickering's usage, however. Thus, in the article cited, he glosses the term "interest" as "a particular constructive cognitive orientation towards the field of discourse" (op. cit., p. 109).
54. As it happens, Pickering could find support for this mode of explanation in the extant literature, for example Kaplan (1964, p. 327 ff).
55. Notice the chummy tone in Pickering's exchange with some representatives of STS in *Studies in History and Philosophy of Science*, 1999, vol. 30, and compare it to the bitter tone of Bloor's "Anti-Latour". The critics do not comment directly upon the potentially disruptive effects of Pickering's criticism of orthodox STS, but choose to focus on the minimal ambitions of his own alternative model (cf. Pinch 1999; Turner 1999). In his response (Pickering 1999), Pickering largely grants his critics' diagnosis of the situation, but adds that any hopes of a stronger stance on the part of Science Studies is an illusion.
56. There is a growing literature in ethics concerning the coherence of construing nature or the things in it as objects of ethical obligation. Some of this literature revolves around the question whether possession of consciousness or sensitivity is a precondition for such a status. These issues are completely bypassed by Pickering, who seems to hold that his normative attitude to nature follows directly and unquestionably from his post-humanist metaphysics.
57. For an indication that the Cartesian tradition is still very much alive in philosophy, see Barry Stroud's works. A recent contribution is Stroud (2000).
58. For philosophical reactions to this issue, see Cohen (1986) and Goldman (1999, p. 230) ff.
59. Cf. Fuller (1989/1993, p. 213 f); also the comments upon Machiavellianism in Fuller (1993, p. 270 ff).
60. See also the detailed proposals concerning the reconstruction of scientific organization in Fuller (2000a, p. 131 ff).
61. Fuller served as a witness for the defence in *Kitzmiller vs. Dover Area School District* (2005), in which the latter tried to uphold its right to include Intelligent Design texts into its science curriculum.
62. For a treatment of Fuller's work with an emphasis on normative issues pertaining to science policy, see Remedios (2003).
63. In Fuller (2002), the author applies this treatment to 20th-century analytic philosophy in toto, in order to point out the roots of the remaining anti-naturalistic elements of this movement. Fuller's approach shows considerable affinity to that adopted by Randall Collins in *A Sociology of Philosophies* (1998), to which Fuller often refers approvingly.
64. Fuller's reservations concerning the role of empirical data is demonstrated in his recommendation that expensive tests within Big Science be replaced with computer simulations; cf. Fuller (2000a), p. 145.
65. The effects of lighting upon worker efficiency was the topic of the first round of the celebrated Hawthorne studies, but the unexpected outcome of this study – that is, that worker productivity went up even when lighting was reduced – shifted the focus to social conditions, which were explored in subsequent phases. It is significant that the Hawthorne findings remain an object of debate and rival interpretations even today, almost three generations after the first data were collected. This interest is not purely theoretical, since potential implications for management practice are rife. For a discussion, see Jaffe (2001, p. 65 ff).
66. As a matter of fact, Fuller recognizes the possibility of an activist response to incommensurability – cf. Fuller (2000b, pp. 29, 350) – but fails to follow up on the idea.
67. For an example of such condemnation, see Carnap (1959, p. 77).
68. In "Philosophy and the Sociology of Knowledge" (1999), Martin Kusch offers an alternative interpretation of the historical context of the current confrontation between philosophers and sociologists of science; contrasting the latter, as I have done, with the much different attitude of philosophers to the naturalizing efforts within cognitive science. Taking his point of departure in the philosophers' side of the controversy, Kusch misses the fact that the initiative to the skirmish came largely from the sociologists, and that the bone of contention was not primarily the status of philosophy, but that of natural science instead.

69. Cynics will suspect that the critical potential of Hempel's work is precisely the reason why it is so signally absent in STS writings. An important theme in Hempel's work is that, for principled reasons, history and the social sciences will have difficulty in living up to the standards of explanation that are achieved in the natural sciences (Hempel 1942). This message would evidently be unwelcome to STS'ers and one should not be surprised to find them turning a blind eye towards it.
70. For a discussion of the viability of a learning theoretical approach to discovery, cf. Hendricks and Pedersen (1998).
71. What is proposed here is the use of computers as *aids* in the analysis and assessment of research strategies, not the full computerization of the project. It is not assumed that the computers can do the work on their own, without constant interaction with their human users. Hence the STS critique of the idea that science could be conducted by computers running strictly algorithmic programmes has no bearing on the proposal made here (Collins 1985/1992, 1990).
72. Among the prominent case-studies produced by STS, an promising candidate for such epistemic reinterpretation would be Pickering (1984), but Latour (1988a) might also be of interest.
73. Cf. the spoof by Trevor Pinch in Pinch and Pinch (1988); see also Latour (1988b); Collins and Yearley (1992a).
74. For a powerful criticism of anti-realism with respect to the past, directed against Dummett 's position but *a forteriori* effective against Latour's, see Peacocke (2005).
75. Cf. Latour and Woolgar (1979/1986, p. 29), also the "Chicken" debate between Collins and Latour, where one of the most potent weapons wielded is the allegation that the methodology adopted by the other side will subjugate Science Studies to the authority of natural science (Collins and Yearley 1992a, b; Callon and Latour 1992).
76. Contributions by Harry Collins to a closely related issue document how orthodox STS'ers – belonging to what Collins would call the Second Wave – would much prefer to remain studiously non-committed with respect to such questions; cf. Collins and Cox (1976, 1977). Collins's Third Wave represents a first step towards abandoning this stance. However, as long as an anti-realist attitude is maintained with respect to the objects of natural science, this reorientation cannot fully succeed.

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