Richard Schantz, Markus Seidel

The Problem of Relativism in the Sociology of (Scientific) Knowledge

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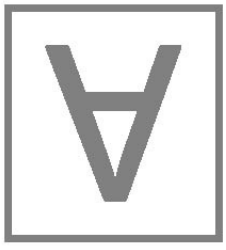

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Introduction

RICHARD SCHANTZ AND MARKUS SEIDEL

Background and Divergent Sources of Relativism in the Sociology of (Scientific) Knowledge

It is no exaggeration to claim that it was above all twentieth century work in the philosophy of science and the sociology of (scientific) knowledge that set the scene for a flourishing of relativist (and constructivist) claims. To be sure, cognitive relativism is by no means a new doctrine. Rather, it is one of the oldest topics in philosophy. Since Plato presented his arguments against Protagoras' claim that man is the measure of all things in the *Theaetetus*, the discussion of relativist doctrines gave rise to fruitful and heated argument. However, by and large in the history of philosophy cognitive relativists are hard to find.

This situation changed dramatically in the last century. Of course, just as in the case of the early sophists there are sociological and historical explanations for this rise of relativist thought in certain quarters – so there are good sociological and historical explanations for the refusal of relativist thought in other quarters.[1] But there is also a philosophical explanation for this change – an explanation by the changes of thought in epistemology and the philosophy of science *itself*.

In the middle of the last century by now classical figures in philosophy

1 This should be – and is – also accepted by those very critical of relativist sociology of knowledge. Thus, e.g., Richard Fumerton, who argues for a return to a traditional form of foundationalism, rhetorically asks: "I am a confirmed foundationalist and I studied at Brown – just a coincidence?" (Fumerton 2010, p. 103).

with quite disparate background proposed ideas that might seem to promote relativist theses: Willard van Orman Quine, Thomas S. Kuhn and Ludwig Wittgenstein.

Thus, Quine in his masterpiece *Two Dogmas of Empiricism* attacked the radical empiricist doctrine of reductionism, according to which scientific discourse can be defined in terms of observation and logico-mathematical auxiliaries by claiming that "our statements about the external world face the tribunal of sense experience not individually but only as a corporate body"[2]. This confirmational holism gives rise to his famous thesis of the underdetermination of theory by the data that has been used hitherto prominently in relativist argumentation. If, as Quine maintains, "any statement can be held true come what may"[3], then – so many relativists argue – which one in fact is held true depends crucially on social factors.[4]

At the time Quine's paper was published, in 1951, he was a senior fellow at the Society of Fellows at Harvard University. There, one of the junior fellows was confronted also with Quine's ideas in *Two Dogmas of Empiricism* that – as he remarked – "had a considerable impact"[5] on him: The historian of science Thomas S. Kuhn. About ten years after the publication of Quine's paper, Kuhn published his *The Structure of Scientific Revolutions* that – beyond doubt – proved eminently influential for the development of the philosophy of science in the twentieth century.[6] Kuhn attacked the classical scientific realist's idea that science progresses by ever closer approaching the truth: the history of science, he argues, shows that scientific progress rather has to be understood also in terms of radical, revolutionary changes of scientific paradigms. The argument in *Structure* surely was a watershed in philosophy of science and has been used extensively by relativists to argue for their theses. Besides Kuhn's

2 Quine 1980, p. 41.

3 Quine 1980, p. 43.

4 Cf. e.g. Bloor 1991, p. 16: "[…] theories and theoretical knowledge are not things which are given in our experience. […] This does not mean that theory is unresponsive to experience, It is, but it is not given along with the experience it explains, nor is it uniquely supported by it. Another agency apart from the physical world is required to guide and support this component of knowledge. The theoretical component of knowledge is a social component, and it is a necessary part of truth, not a sign of mere error."

5 Kuhn 2000, p. 279.

6 Kuhn 1970.

thesis of the theory-ladenness of observation and the incommensurability-theses that are taken on behalf of anti-realism, his conclusions arguably open the gate also to sociological forms of relativism: after all, Kuhn claims that "as in political revolutions, so in paradigm choice – there is no standard higher than the assent of the relevant community. To discover how scientific revolutions are effected, we shall therefore have to examine not only the impact of nature and of logic, but also the techniques of persuasive argumentation effective within the quite special groups that constitute the community of scientists."[7]

Remarks like these echo ideas of the last of the authors important for the flourishing of relativist thought in the last century: Ludwig Wittgenstein. Thus, in his *On Certainty*, a collection of late notes especially concerning epistemological questions, he imagines people consulting an oracle.[8] Concerning the questions "Is it wrong for them to consult an oracle and be guided by it? – If we call this 'wrong' aren't we using our language-game as a base from which to *combat* theirs?"[9], he comments: "I said I would 'combat' the other man, – but wouldn't I offer him *reasons*? Certainly, but how far would they go? At the end of reasons comes *persuasion*. (Think what happens when missionaries convert natives.)"[10] If, in the end, we are forced to use convertive and persuasive strategies, the investigation of these strategies surely is a classical task for the sociology of knowledge. Thus, arguably, Wittgenstein's later philosophical views in his *Philosophical Investigations* and *On Certainty* can be seen as inspiring relativist thought[11] and, in fact, have been very influential in the arguments of philosophers that can be associated with relativism – as for example Richard Rorty.[12] Some authors take Wittgenstein's considerations also to be exercises in the sociology of knowledge.[13]

It goes without saying that it is highly controversial whether Quine's,

7 Kuhn 1970, p. 94. It is surely remarks like these that led Quine to bemoan "the tendency of […] Kuhn […] to belittle the role of evidence and to accentuate cultural relativism." (Quine 1969, p. 87).

8 This is not quite as imagery as Wittgenstein suggests since he was familiar with Evans-Pritchard's study of the Azande (cf. Evans-Pritchard 1937).

9 Wittgenstein 1975, § 609.

10 Wittgenstein 1975, § 612.

11 Cf. e.g. Kusch 2010. For a different opinion cf. Williams 2007.

12 Cf. e.g. Rorty 1989.

13 Cf. esp. Bloor 1983, Bloor 1997.

Kuhn's and Wittgenstein's views can be taken to sustain relativism.[14] What is undeniable, however, is that these trains of thought have been used to corroborate relativist thought in general and also especially in the sociology of (scientific) knowledge since the middle of the last century. This, at least, is the story to be told from recent history of *philosophy*.

It is a remarkable feature of the debate that the development of relativist and also constructivist thought in the sociology of (scientific) knowledge can be reconstructed quite differently once we throw a glance at the history of *sociology* itself. For ease of exposition it is convenient to distinguish roughly between *classical* and *new* sociology of knowledge. The inauguration of the sociology of knowledge, i.e. of *classical* sociology of knowledge, as an own branch of sociological research at the beginning of the last century was heavily affected by discussions about relativist implications in the so-called *Streit um die Wissenssoziologie*.[15] Classical sociology of knowledge – especially the more radical form of Karl Mannheim – was confronted with severe attacks on its 'sociologism' that was taken to imply a devastatingly self-refuting relativism. At about the same time as Mannheim's German edition of *Ideology and Utopia* was published Alfred Schütz aimed to lay down a phenomenological foundation of sociology in his *Der sinnhafte Aufbau der sozialen Welt*.

Drawing on the work of Schütz his pupils Peter L. Berger and Thomas Luckmann proposed a *new* sociology of knowledge in their *The Social Construction of Reality*. Classical sociology of knowledge, they argue, has been unduly focused on ideologies and theoretical knowledge, thus only capturing a small part of what passes as knowledge in a society. Berger and Luckmann propose instead that the sociology of knowledge should more strongly take into account 'what everybody knows'.[16] In any case, with Berger's and Luckmann's book the talk of *social construction* starts to become popular in many branches of the social and cultural sciences – and also intensely attacked from outside these areas of research – but, what-

14 Furthermore, it is controversial whether some of these authors *are* relativists. Kuhn, for example, denied to be a relativist in the sense understood by his critics (cf. Kuhn 1970, pp. 205 f.). Cf. with respect to Wittgenstein: Williams 2007.

15 Cf. Meja/Stehr 1982.

16 Cf. Berger/Luckmann 1966, p. 65.

ever the subsequent development of the notion of 'social construction',[17] it should not be forgotten that, as the subtitle of their book makes clear, it originates as a key notion in *A Treatise in the Sociology of Knowledge*.

It is the conviction of the editors of this volume – although both see themselves not as cognitive relativists or constructivists – that it is an enormously fruitful enterprise to bring together these different trains of thought in the debate. We think that after the heated debates between the disciplines in what – unfortunately – has been called 'The Science Wars' cooled down it is time for a new look at the problems of relativism in the sociology of (scientific) knowledge. The reason is simple: Though not that heated anymore the differences of opinion are still there and they are far from minor ones! Actually the relativism debate still goes on.[18]

The present volume grew out of talks given at the international conference 'The Problem of Relativism in the Sociology of (Scientific) Knowledge' held at the University of Siegen on March 22nd and 23rd 2011. It was co-organised by the University of Siegen and the Center for Philosophy of Science (ZfW) at the University of Münster. The aim of the conference was to bring together philosophers and sociologists working in the field and to discuss the problems of relativism from a systematic *as well as* from a historical perspective. This aim is reflected in the present volume which contains both papers attacking and defending relativist approaches and papers focused on particular authors who played an important role in the history of the debate. Before we will give an overview of the papers we would like to thank all the helpers from Siegen and Münster who made the conference such an enjoyable event and rendered the publication of this volume possible. We want to express our special gratitude to Rafael Hüntelmann from ontos-publisher for the helpful and uncomplicated cooperation in preparing this publication and Mario Franz for the typesetting. Last but not least we would like to thank all the contributers to the conference and to this volume: After all, the success of such a project stands and falls with the contributions, and we are convinced that it has been a success.

17 Cf. Hacking 1999 for exposition.

18 Cf. e.g.: Bloor 2007, Bloor 2008, Boghossian 2006, Hales 2006, Hales 2011, Mosteller 2008, Pritchard 2009, Sankey 2010.

The Chapters

Barry Barnes defends relativist research programmes as scientifically and naturalistically inspired. By way of reporting the history, background and development of 'Edinburgh relativism' – especially pointing to the importance of Thomas Kuhn's work – he summarises the sociological perspective that motivated such programmes in four key points. He argues that especially the fourth key point, the finitist claim that knowledge does not inherently imply how knowledge is to be used such that the links between knowledge and action are the foci of empirical curiosity, bespeaks the scientific attitude of 'Edinburgh relativists'. Barnes aims to sustain this claim by pointing to the development of fruitful empirical studies carried out in naturalistic and notably relativistic spirit.

Harvey Siegel discusses the question of whether epistemological relativism is an incoherent position. After rehearsing Plato's case for incoherence he examines the position of the proponents of the Strong Programme in the sociology of scientific knowledge. Siegel distinguishes between innocuous and more contentious claims to be found in their writings and examines their arguments for the latter. He focuses on the 'no transcendence, therefore relativism'-argument; arguing that from acceptance of the impossibility to achieve a 'perspectiveless perspective' relativism does not follow. Despite such an impossibility, so Siegel claims, there is a sense in which we *can* transcend our own, actual perspective. Furthermore, drawing on the possibility of sociological accounts of the causes of the credibility of belief that conflict with the account favoured by Strong Programmers, he concludes that the Programme's relativism is at odds with its avowed scientific status and finally falls prey to the charge of incoherence.

In the first two parts of his paper, Richard Schantz argues that anti-realism seems to be a necessary condition of any serious form of relativism. He deals with the debate between realism and anti-realism and defends a version of metaphysical and epistemological realism with respect to the world of physical objects in space and time. The third part examines the currently popular proposal that relativism should be characterized in terms of the idea of faultless disagreement, disagreements in which both parties can be right. Schantz criticizes this idea and argues that faultless disagreement is an illusion. Accordingly, there are only or-

dinary disagreements, disagreements in which at most one party can be right. In the fourth part he asks what concept of truth adherents of truth relativism are working with. Schantz looks at the alternatives plausibly available to relativists but comes to the sobering conclusion that no really convincing answer has been given to this fundamental question up to now and that the prospects for giving one are pretty dim.

Magdalena Eckes, Simon Erll and André Wenclawiak are concerned with two questions: Is perception theory-laden and could this lead to epistemic relativism? They argue that the answer to the first question is dependent on how perception is conceived – conceptual or non-conceptual. Therefore, their focus is not only on classical proponents of theory-laden observation like Norwood Hanson and Thomas Kuhn, but also on McDowell as a contemporary philosopher of perception who takes the content of experience to consist of concepts. The authors try to show that non-conceptual content of perception will not lead to any serious kind of theory-ladenness and hence no relativism. If the content of experience is, on the other hand, taken to be conceptual, it is much more difficult to escape epistemic relativism.

Maria Baghramian attempts to clarify the extent and the nature of the link between the contentious doctrines of social constructivism and relativism, where the former is often identified with the latter. She distinguishes between three levels of construction, beginning with the uncontroversial claim that human institutions are socially constructed and moving via the social construction of theories, to the contentious claims that even facts are socially constructed. She argues that in each of these cases the connections between constructivist approaches and relativism are either non-existent or not quite as straightforward as critics have suggested. The aim of her paper is not to defend either relativism or constructivism, but to argue against a tendency to underestimate the strength of the arguments in favour of relativism by associating it with patently absurd doctrines.

Hubert Knoblauch relates the question of relativism to the new sociology of knowledge that – starting in the 1960s – has been at the origins of social constructivism. He argues that, whereas the notion of constructivism was diffused in various branches of the social sciences into what Hacking somewhat disparagingly called "the social construction of everything", it was often overlooked that social constructivism *sensu strictu*

resulted from an extended phenomenological debate on the foundation of knowledge in the basic meaning of experiences. Accounting for the criticism and deficiencies of Husserl's attempt for an ultimate foundation of knowledge, Knoblauch maintains, the reference point for knowledge of any kind was to be the mundane life-world as analyzed by Alfred Schütz. As important as the role of the life-world as a fixed (anthropological) reference point to social constructivism, the questions as to how to identify its "mathesis universalis" turned out difficult, and, according to Knoblauch, in the face of the failure (and the surrender) of the search for linguistic and cultural universals, unsuccessful. He aims to show that the critique of linguistic reductionism and the extension to communicative action leads to a transformation into communicative constructivism. After sketching the major aspects of communicative action and communicative forms the paper finally looks at the consequences of this new approach in the sociology of knowledge on the problem of relativism. On Knoblauch's account, while communicative action allows for a certain reflexivity, rationality is a form of belief inscribed in and presupposed by communicative action.

Martin Endress points to the idea of arguing with historicism against historicism, and thus tries to renew an argument Karl Mannheim in his most prominent Weimarian period established by configurating a type of reflexive analysis in his sociology of knowledge-approach. With Mannheim's concept of reflective relationism, Endreß maintains, a three step analysis including the selectivity, perspectivity, and constructivity of any knowledge is established that constitutes its very historicity. He believes, that the current importance of Mannheim's solution is due to the double-edged sword it established: arguing against the claims for validity, on the one hand, and stating a certain type of validity, on the other hand. Finally, Endreß argues that Mannheim's historical solution of the problem in question still holds for systematic reasons.

Markus Seidel focuses on one central aspect of Karl Mannheim's sociology of knowledge: his exemption of the contents of mathematics and the natural sciences from sociological investigations. After emphasizing the importance of Mannheim's contribution and his exemption-thesis to the history and development of the field and the problem of relativism, he surveys several interpretations of the thesis – especially those put forward by proponents of the so-called 'Strong Programme'. Seidel argues

that these interpretations do not get the philosophical background and impetus of Mannheim's contribution right. By distinguishing between naturalistic and anti-naturalistic strands in Mannheim's work he proposes a new reading on which Mannheim did not exempt the contents of the areas in question principally or because of a lack of nerve and will. It is argued that Mannheim's exemption-thesis rather is a consequence of his own sketchy *sociological* investigations of 'the paradigm of the natural sciences'.

Eva-Maria Jung addresses the question of how Michael Polanyi's theory of knowledge faces the problem of relativism. In the middle of the twentieth century, Polanyi introduced the concept of "tacit knowledge" that is widely used in recent approaches to cognition.

After summarizing Polanyi's main ideas, Jung discusses his theory in relation to the problem of relativism. She argues that although Polanyi explicitly rejects relativism his discussion of scientific controversies yields certain relativist conclusions. Moreover, his theory threatens to become inconsistent due to a tension between these relativist tendencies and his epistemic and scientific realism. At the end of her contribution, Jung highlights some major similarities and differences between Polanyi's account and recent approaches to tacit and explicit knowledge.

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Relativism as a Completion of the Scientific Project

BARRY BARNES

Relativism is widely perceived incorrectly as an anti-scientific position. It is important to remember that there are many forms of relativism and that some of them involve a positive view of the natural sciences and may even be presented as expressions of a scientific orientation. A high regard for science may not merely be consistent with relativism but the inspiration for it. Consider moral and ethical relativism: the fact that no scientific basis exists for setting one set of moral or ethical doctrines above others is often reckoned a powerful argument in its favour. Ontological relativism may be advocated on the same grounds: what is real may neither be observed directly nor reliably deduced from what is apparent, and so it can be argued that no scientific basis exists for any particular ontology. It is intriguing to note as well that some of the stronger forms of rational choice theory arguably imply relativism. If the beliefs of human beings are all rationally held, then the manifest variability of those beliefs, and the evident conflict between many of them, – even in the context of the natural sciences, and among scientists confronted with the same evidence, – might be taken to be an argument for epistemological relativism. And of course strong empiricism points us in the same direction. Bodies of belief about empirical phenomena are sets of generalisations about groups or clusters of those phenomena. But the phenomena themselves do not tell us how we must group them. Many different groupings are possible, as the basis of different conceptual schemes or classification

systems. And this supports a relativistic account of conceptual schemes and hence of the different systems of verbal knowledge of empirical phenomena that can be established on the basis of them.

'Edinburgh relativism', as it is now called, nicely exemplifies how a significant form of relativism may conflict with the dominant stereotype. Initially developed as a relativistic analysis of the natural sciences and scientific knowledge, it was nonetheless inspired by an unambiguously positive orientation to them. With this in mind, and aware also of the special focus of this volume on the history of relativism in relation to science, I want to begin by recalling the development and reception of this form of relativism. Be warned, however, that what I have to say should not be relied upon as history. Given my own involvement in a story that began over forty years ago I can scarcely claim to be a reliable and dispassionate reporter of events many of which are now too distant for me even to recall in any detail.

In the late 1960s, I joined a small group of scientifically trained staff at the newly founded Science Studies Unit at Edinburgh University. The remit of the Unit was to develop courses designed to broaden the education of natural science students and increase their awareness of the wider impact and significance of science and scientific research. But alongside this a programme of research soon emerged, with the aim of understanding the knowledge and activity of the sciences themselves, that is, their content and substance and why that content and substance counted as knowledge. As a sociologist, I myself looked to the literature of the social sciences for material relevant to this aim, just as colleagues in the Unit looked to literatures in history and philosophy. And it slowly became clear that whilst existing social-scientific studies of knowledge included little systematic work of the requisite kind on the natural sciences, they did provide a tacitly accepted perspective that could be tried out as a basis for such work. Let me summarise the key points of this perspective.

1. Knowledge was understood as the possession of a collective, not an individual. Durkheim had famously contrasted collective representations with individual perceptions and beliefs, and identified the conversion of the latter into the former as a process functionally indispensable to the life of human beings. Other writers had focused more on know-how than know-what and on knowl-

edge as methods or competences, but again it was only by virtue of being shared and having standing in a collective that these could be counted as knowledge.

2. Understood in this way, knowledge had been identified as part of the shared cultural tradition of a collective. Looking across collectives, what was given epistemic standing was clearly liable to vary and even to conflict. But in any given collective knowledge was in the first instance received from the ancestors and accorded epistemic standing on their authority; and whilst it did of course change in the course of use, such change was a matter of the adaptation of a shared epistemic inheritance not of the piecemeal elimination and/or replacement of those separate components of it that proved untrue or invalid.
3. The distribution and use of knowledge in collectives had been recognised as socially structured. In particular, in societies with extensive division of labour and institutional differentiation, different forms of knowledge and the specialists who carried them had standing in different domains, and the results of their application therein were evaluated in different ways.
4. Accounts of the use and application of knowledge wherein its 'implications' were assumed to inhere in the knowledge itself had been recognised as untenable, in large parts of micro-sociology [including ethnomethodology] and anthropology. In these fields the use of knowledge was being studied as contingent social action. The links people made between their knowledge and the actions 'implied' by it were increasingly being transformed into foci of empirical curiosity.

One way of understanding 'Edinburgh relativism' is as something which emerged along with our growing awareness that these general points could be extended to the knowledge of the natural sciences without any modification or supplementation. The general account applied. And nothing extra needed to be added to it in order to account for the special epistemic standing of science, its putative truth, its alleged use of validation procedures specific to it alone, its character as 'genuine' knowledge rather than something merely taken to be such: all of this could be encompassed by the general account as it stood and had its paral-

lels in discourse surrounding other forms of knowledge, including the no-longer-valid knowledge that littered the history of science itself. Of course, this awareness did not emerge in an instant; it developed through an extended period of research and reflection involving examination of case study accounts of scientific knowledge from what we took to be a naturalistic, scientific perspective. And neither did it take on a final form and stop developing: it became an identifiable 'it' only when it was eventually rationalised by David Bloor as a continuing *research programme* that aimed to understand all knowledge, including scientific knowledge, in the same way, and to account for its acceptance and credibility naturalistically and causally, without regard for the truth or falsity of the knowledge in any context-independent sense.[1] This relativist programme clearly articulated the naturalistic, scientific orientation which was its incompletely rationalised starting point and identified that starting point as itself a potential target of its own investigations. The programme was thus explicitly reflexive as relativism requires. Indeed, having extended the scientific project to science, it welcomed its further extension to itself as a part of the endless task of bringing that project to completion.[2]

It was probably just as well, as awareness of this programme grew in the 1970s, that we were working in an autonomous academic unit, sustained by our teaching activities and the interest and enthusiasm of large numbers of natural science students. For we quickly became aware that we were not supposed to be doing what we had set out to do, that relativism was an anathema, and that science had to be accounted for by reference to reason and experience not ancestry and authority.[3] It can be

1 Cf. Bloor 1976.

2 It is perhaps worth bearing in mind the context. It did not seem especially subversive, working in a science faculty, teaching science students, to omit explicit expressions of allegiance to science or accounts of what made it so special: there was no demand, no felt loss of authority, awareness that in practice its knowledge and expertise were valued even after the ups and down of the 1960s. Science wasn't an institution in decline like some of the churches; it was growing and thriving both generally and in academic contexts wherein the bureaucratic wrecking squads had yet to intrude. Unlike some fields, the sciences had no strong need to legitimate themselves and find ways of bolstering their authority.

3 Contrary to what at one point was widely claimed by commentators and critics indifferent to what we had set down in print, 'Edinburgh relativism' nowhere denies the existence of an external world, nor even its irrelevance to what humans claim to know of it. It actually acknowledges the existence and indeed the relevance of that world and merely denies that the world is a sufficient basis for understanding any verbal account of it or what

hard to grasp today just how strongly science was sacralised at that time, and correspondingly difficult to understand why so many academics reacted to our work in knee-jerk fashion, as if it was an attack on science, and thought nothing of hurling abuse at fantastical misrepresentations of what we were saying – apparently on the assumption that something so self evidently objectionable needed no serious examination and all that mattered was that audiences were warned against it somehow or other. Things are different today of course, but these initial misrepresentations need to be mentioned, if only because they are still occasionally cited by commentators, out of indolence now perhaps, as much as hostility.[4]

In any event, considerable labour had to be expended at the time not just on defending our views but on clarifying them, and we found ourselves both referring back to our original sources of inspiration and ransacking other literatures for helpful materials. I vaguely recall appeals to the physiology of the eye and its ability to enrich our understanding of blurred images as readily as focused ones, and to the innumerable different maps that may all alike be accounted maps of the same terrain, in efforts to convey elements of our position. And I recall as well the shared pleasure of finding salient supportive arguments in empiricist and inductivist philosophy, and no less salient case studies in the historical literature and in ethnomethodology. But probably the most important of all the work we drew upon initially was that of Thomas Kuhn, which had been invaluable in the development of our ideas almost from the start.

Even today many people look back on Kuhn as a relativist of a strongly idealist sort, whose primary philosophical concern was with the problem of theory choice in science, and the revolutions wherein one scientific theory or paradigm is replaced with another. We read him differently. The paradigms famously referred to in 'The Structure of Scientific Revolutions'[5] are not theories: they are exemplary achievements – particular solved problems which a community of scientists has accepted as the au-

is known of it. Of course, when an understanding of conflicts between knowledge claims is sought, references to the world [of which there is one in which we all live] tend to be less illuminating than references to social contexts [of which there are many].

4 Of course there was also a number of thoughtful and well-informed critics: Martin Hollis, Steven Lukes and Donald Campbell come immediately to mind, but there were several others and, albeit via disagreement, we learned much from them.

5 Kuhn 1962.

thoritative basis for the solution of further problems in their field. In his later work Kuhn refers to them as exemplars. For Kuhn exemplars are the basic units in which scientific knowledge is transmitted and acquired, and the extension of that knowledge involves treating unresolved problems as analogous to existing solved problems and thereby solving them in turn. A process of case-to-case modelling is involved, not the process of secure inference from general to particular of the sort that accounts of scientific theories and their logical implications had long encouraged us to imagine.[6]

Once Kuhn's notion of paradigm is understood as denoting an exemplary achievement his account of scientific knowledge is made visible as entirely consistent with a sociological understanding of knowledge in general, something that quickly becomes apparent if it is compared with the four key points of such an understanding I set out earlier. In particular, Kuhn's account can then be seen to be consistent with the final point, which has proved to be at once the most difficult and fecund of the four. The analogies through which the knowledge incarnate in exemplars is extended and generalised are not logically compelling. Even the simplest of such analogies, involving direct intuitions of empirical similarity, do not permit indefeasible inferences to further instances; empirical similarity is not a transitive relation. Accounts of exemplars being extended in processes of modelling and analogy are accounts of contingent actions, of people treating things as the same where they could instead have treated them as different. And accounts of people acting in agreement as they extend knowledge from exemplars are accounts of contingent social actions, coordinated by collective [epistemic] authority.[7]

6 This view led some philosophers to revise their understanding of what theories were, in order to save the notion, as it were. Formalisms were constructed wherein sets of exemplars had to be regarded as essential constituents of theories, part of the very notion of what a theory was. And whilst this work may be thought unduly conservative, it was very impressive notwithstanding. But although it did make a mark in philosophy, in Germany especially, it seems never to have been given the recognition it deserved. (Sneed, 1971, Belzer, Moulines and Sneed, 1987).

7 Kuhn provided many fine examples of these processes at work in science. But he also offered a useful argument for an understanding of science as contingent action that engages effectively with those who persist in seeing it as involving the validation of theory by observation or measurement. Measurements, he points out, are never in perfect accord with theoretical predictions (Kuhn 1961). There is invariably a difference between the one and the other, so that if evidence is to be taken as supporting a theory it must we adjudged 'close

In 'Second Thoughts on Paradigms', Kuhn spoke of the "truism that anything is similar to, and also different from, anything else"[8] but one of his prime concerns throughout both his philosophical and historical work was to encourage us to attend to this supposed truism. Evidently, it was a much neglected truism at the time, and we remain less curious than Kuhn's remark suggests about why we count one thing the same as another. Certainly, in sociology awareness of this question in various guises has long existed and proved productive, but for the most part within the confines of sociological theory and micro-sociology, despite its clear relevance to the whole of the field. And in philosophy, for all that major figures like Wittgenstein and Goodman had highlighted the problem and it was recognised that empirical sameness was an intransitive relation, researchers have often preferred to take no account in their practice of what they have been willing to accede to in the abstract. Rationalist philosophers in particular have preferred an extensional account of knowledge, one where knowledge clearly illuminates an endless path forward from one correct application to another for its users to follow, to a finitist account wherein users themselves had to hack out the path as they went along.

It is worth going into a little more detail on the problem at issue here, even though the key points are not to my knowledge controversial. How, given Kuhn's 'truism', is one thing to be identified as the same as *rather than* different from another, as we must if knowledge is to be applied? Given the indefinite complexity of the material world, any two objects are going to be non-identical even if we label them as of the same kind. But if we extend the label from one object to another, and from that one to yet another, and so on, every time two non-identical objects strike us as 'very like' or 'near enough the same' or whatever – we shall be liable to end up referring to anything and everything as of the same kind. Many references based on 'small' differences will cumulatively bridge the

enough' or 'in reasonable agreement' with it. But there is no independent means of ascertaining what counts as 'enough' or as 'reasonable' and the scientist has to look to communally accepted conventions of what so counts, conventions that very enormously between different contexts. This may be seen as a specific version of the general finitist argument mentioned in what follows.

8 Kuhn 1977, p. 307.

gap between 'vastly different' objects; they will take our references every which way, and render our label useless for all practical purposes. For our convenience we may of course further identify our non-identical objects as 'the same' in some respect or property, but then the transitivity problem recurs. Using a same-colour relation, for example, we may start with an exemplary red object and end applying 'red' to a yellow one. Indeed a chain of sameness relations between objects *empirically indistinguishable* in colour could begin with a red one and end with a yellow one. Moreover, the relation of [empirical] self-sameness of an object over time is no different from the relation between two objects separated spatially. Empirical sameness has to be treated as an intransitive relation even where we are currently unable to discern empirical difference, and even when we extend what we reckon [empirically] to know of an object back upon itself. All this suggests that where and as a science makes use of a classification scheme permitting reference to just so many distinct and separate kinds of object more than mere empirical scrutiny of the objects must be involved. And indeed it is widely accepted that rules and conventions of classification are also necessary to account for how that activity proceeds. But even to accept this is not enough. If we invoke rules or conventions to help to account for judgements of sameness, the transitivity problem recurs yet again. How can an existing instance of the correct application of a rule or convention be reliably be extended to the next instance? Is there any way in which we can reliably figure out what a rule [really] implies, so that we can identify what the rule is 'telling' the rule follower to do, ahead of its application in the next instance? The finitist answer was that no such way existed: the actions rationalised by judgements of sameness had to be treated as contingent actions. But I shall not seek to justify this answer further here.

This purpose of this paper is not to advance technical arguments on behalf of 'Edinburgh relativism', and those above are not intended to do that. They are intended merely to expose the difficulties that surrounded and still surround an issue that has concerned it for many years.[9] Unlike the other three components in the sociological conception of knowledge

9 It is interesting to note that the philosophers who have systematically explored the problem here – including Goodman, Quine, ,Hesse, and of course, turning to a slightly different context, Wittgenstein – have varied in their views on their salience for epistemology and relativism. The clearest and most systematic discussion I know of is in Hesse 1974.

set out earlier, which can now seem banal even as applied to natural science, and elicit little debate, the fourth point has been a continuing focus both of argument and further research. Indeed, I now understand my own original words on the subject, written more than thirty years ago, better than I did then, although fortunately I find that I still broadly agree with them. Thus, work focused on this fourth point illustrates particularly well how 'Edinburgh relativism' developed and grew as a research programme. Participants did not receive relativism as revelation and thereupon leap happily out of their office windows, as some commentators appear to believe they should have done. What they did, somewhat after the fashion of the sciences in which they were trained, was to try to learn a bit more about, and get a more detailed understanding of, what they were talking about.

Recall now what the purpose of these recollections actually is. They are intended to illustrate how, contrary to widespread philosophical stereotypes, relativism could be inspired by a high regard for science rather than a wish to undermine it. I hope to have said enough now not just to show that 'Edinburgh relativism' was so inspired but to convey a little of how it was so inspired, – as well as to indicate how widespread and strongly held the stereotypes with which it conflicted once were, and how easily they could mislead and give rise to harm. But having gone so far I may as well fast track forward and link the story to the present, given that neither 'Edinburgh relativism' nor the negative stereotypes of relativism I have described are yet extinct.

In fact the more recent years of the story are the harder to recall and summarise, since they cover exciting times when there was a great deal happening and too much to do. In a few words, what initially was the search for resources to buttress our views became at the same time a search for topics of broader interest on which productive research could be carried out. In philosophy, David Bloor's work on Wittgenstein had this character,[10] as did much else produced in other disciplinary contexts.

10 Cf. Bloor 1983, 1997. I have cited Bloor's work throughout because of its continuing direct engagement with the problem of relativism that is the topic of this book. But to get a proper sense of how things were unfolding in the '80s and '90s, and why nobody was much inclined to leap out of the office window, it is necessary to look as well at much else, not least the superbly original, accomplished and influential work of Steven Shapin and Donald Mackenzie.

And the historical and sociological studies produced in Edinburgh were often inspired as much by the opportunity to study fascinating topics as by the desire to exemplify a relativist standpoint. At the same time too the entire context of research was changing, as more and more people from many different backgrounds directed their attention to science and scientific knowledge and brought new attitudes and perspectives with them. The unscientific reverence for a sacralised natural science ubiquitous when we began our work was rapidly being transformed into the taste of just some groups among many, in a setting wherein a strongly critical approach to science and scientific knowledge was also establishing itself, and there were even those who were critical of relativist claims because they were critical of claims in general and who wrote at length on the virtues of silence. This, of course, was a slightly double-edged development for us, and I recall finding it a little disconcerting when I finally noticed late in the 1980s that in my field at least we were no longer being attacked as subversives but as orthodoxy.

I want to mention an important book published at the end of this period as a sign and symbol of how things had moved on. It is one of a number that would serve, and I choose it partly because of my own continuing interest in genetics and its history. *Lords of the Fly*, 1994, Robert Kohler's account of Drosophila genetics, can be seen as a part of a long revisionist trend away from the heroic histories of classical, 'Mendelian' genetics that I had encountered and learned to question in my youth. It is tempting to relate this trend to an epistemic downgrade of Mendelism ongoing among geneticists themselves as their field became increasingly 'molecularised', but that could easily be wrong. Certainly, a major shift of epistemic authority from classical to molecular genetics was occurring as Kohler's book was being written, and may conceivably have made its writing that much easier, but there is no positive sign of its effects in the work itself. Rather, it offered a history that had set aside any concern with the epistemic standing of the science. From my narrow perspective it exemplified a kind of history, no longer thin on the ground, which was making many of our old arguments gratuitous, as what they sought was built in to historical method. More important than all this, however, were the forward looking aspects of the book. It was firmly centred on newly prominent foci of research and began with an extended justification for a shift of historians' attention toward them. It was a history of scientists

at work; of their experimental practice; the artefacts involved therein including living artefacts; the 'material culture' of science; and its moral economy. Again, some of this shift might have been encouraged by associated secular trends; like the relentless transformation of science from a pursuit ordered along pseudo-aristocratic lines to a specialised form of paid employment, and the larger no less relentless move to a less deferential society, wherein work is work, as it were, hand is valorised as much or more than head, and expertise can no longer expect to be put on a pedestal. Even so, work of this kind was greatly needed, the more so for its earlier neglect. And in subsequent years it has fed back into and enriched the historical study of science generally, not to mention work in other disciplines.[11]

From roughly this point in time to the present, with several of the groups and sub-fields now studying scientific knowledge doing so in a tolerably naturalistic way, many of the old arguments not so much settled as in abeyance, and the individuals who had carried it forward relocating and finding new challenges in new locations, it becomes more difficult to trace the path of the programme, or even to parry the question of whether a programme following a path is still there to be traced. In my own work, problems thrown up as it has gone along have tended to be the backdrop I refer to, and I tend not to worry about how secure is the chain of sameness relations that connects me to the initial relativist position. Things have stayed the same; things have changed: have it how you will. But I have largely worked in the social sciences, and in philosophy it is easier for anyone looking to trace the path of a programme via explicit citations of its formal tenets and commitments, and to judge whether it is succeeding or flagging in the face of potent criticisms and arguments.

11 Kohler explicitly mentions his lack of attention to philosophy and sociology and particularly to the social constructivist sociology which had been important in pioneering the study of experimental practices. But he nonetheless makes an intriguing contribution to social constructivism himself. Read in the light of his new priorities, the true hero of Kohler's book is not Thomas Hunt Morgan, the intellectual inspiration and overlord of fly room research, but Calvin Bridges, the indispensable technical supremo, ruthless killer of non-Mendelian flies and skilled curator and tracker of those permitted to live. Bridges constructed a set of laboratory flies of which Mendelian genetics was true [or, if you prefer, to which it could be productively applied]. Given the whole earth, he might be imagined reconstructing the entire D. Melanogasta species so that Mendelism was true of it. Here is an interesting way in which the truth of a theory may be established: if your theory doesn't correspond to reality, then reconstruct reality, as Bridges did.

In the context of philosophy 'Edinburgh relativism' actually sits at the site of a potentially endless debate: the war between science and reason is not due to end any time soon and 'Edinburgh relativism' has made a distinctive contribution to it that merits repetition whenever hostilities resume, and 'relativism' is attacked, as periodically it continues to be. A very recent episode of this kind is the eruption of militant, rationalist atheism among some scientists currently attracting attention in England, with Richard Dawkins its most prominent figure. An earlier, larger and more prolonged series of encounters was the so-called science-wars, wherein the physicist Alan Sokal led an alliance of scientists and philosophers into battle against relativism, post-modernism and various other tendencies offensive to good rationalists. Both Dawkins and Sokal deplore relativism. But the relativism they both deplore is basically that of the venerable philosophical stereotype that characterises it as irrationalist and anti scientific, – although the stereotype is a model of subtlety compared to the accounts that have been propagated by these two. Both figure among those mentioned earlier, who are prone to wonder why relativists don't leap out of their office windows.[12]

In the context of the science wars a continuing interest in 'Edinburgh relativism' can be documented as its tenets were cited and debated and ancient criticisms were rehashed, but of course what we have here are polemical confrontations between public intellectuals, and if the aim is to check how far its basic tenets and commitments have continued to be interesting at a more technical level they are not salient. Here, the need is to review their standing in the appropriate settings, where some of them at least have continued to prompt reflection and debate. As far as its ba-

12 It is hard to be fair to eminent thinkers such as these, since it is hard to work out when they are being serious. How serious was Dawkins 1995 when he challenged relativism with the claim that 'Airplanes built according to scientific principles work […]. Airplanes built to tribal or mythological specifications don't'? It is hard to say, but the second half of the quote is surely no more than empty polemic. I know nothing of the flightless airplanes built by the New Guinea aircraft industry, but Dawkins evidently knows no more, nor anything else relevant to what is claimed. The first part may seem a plausible empirical claim, but where is the evidence? David Bloor (Bloor forthcoming, see also Bloor 2008 for the basis of the discussion in this note) has studied of aircraft design in England and Germany early in the last century. The English sought to design wings in accord with scientific principles, whereas the Germans knowingly adopted designs that were inconsistent with them. The Germans produced the better aircraft, with wings that achieved superior lift. Dawkins appears to have had an antediluvian view of the science-technology relationship in 1995.

sic commitment to naturalism is concerned, that is, to proceed in analogy with the practice of the sciences in order to understand it and the knowledge engendered and sustained by it, the major focus of ongoing academic controversy is the programme's readiness to propose causal accounts of intentional human actions. Important constituencies in both philosophy and the social sciences object strongly to use of the institution of causal connection in the understanding of voluntary human behaviour and insist that the appropriate resource here is the institution of responsible action; but this of course creates a dualist scheme of just the kind that the programme opposes – unless, that is, one rejects causal accounting altogether and treats all of the material world as alike possessed of the same Divine spark that, as some believe, animates humans. As far as its model of knowledge generally is concerned, the finitist claim that I have already discussed continues to be the component that inspires the most research and attracts the most critical interest.

Some of this more recent work has gone relatively unremarked in sociology and philosophy of science because it is no longer entirely focused on the distinctive concerns of those fields. Science had long been the sacred form of knowledge to which relativistic theories did not apply, could not apply, were not permitted to apply. But when a relativistic perspective was finally brought to bear upon it, scientific knowledge became for a time the test-bed of its ideas, the place to try out newly legitimated methods and techniques, a site of intense debate. Among those at work in this context, whether disposed toward relativism or opposed to it, understanding of some of the issues came up from behind and then moved on beyond what existed elsewhere. The flow of knowledge reversed itself to some extent in consequence, as researchers who had previously imported insights and exemplars in order to study science looked beyond it for sites to apply what they had learned thereby. The rationalisation for this, of course, already existed: if all forms of human knowledge were to be understood in the same way, then knowledge of scientific knowledge should routinely apply to knowledge generally; it merely involved transferring techniques and exemplars across conventionally drawn boundaries in the opposite direction from before. Indeed, at some point the practice of this relativist epistemology/sociology of scientific knowledge was almost sure to become, what in the abstract it was already, an epistemology/sociology of knowledge simpliciter.

The work that most easily exemplifies this change is that surrounding the finitist claim. My earlier discussion used the example of a kind term being applied to objects: the finitist claim is that there is no inherently correct way of applying the term and every successive application must be treated accordingly as a contingent action. But the discussion applies analogously to laws, rules and norms, principles and postulates, and so forth, which also lack inherent empirical implications. And all of these entities are encountered in contexts beyond science as well as within it, and their study in those contexts is no less interesting and important. Thus, it is important in sociology to study the application of kind terms, and human kind terms in particular, in everyday contexts.[13] As far as laws are concerned, the concept of scientific law actually derives from juridical contexts, with the initial modification en route to science being the replacement of a human with a Divine legislator[14] and a redirection of interest from science back to law may add to an understanding of how laws are applied in both contexts.[15] The problem of the implications of rules and norms arises in every kind of social context: with those who study bureaucratic hierarchies and organisations addressing it as part of the enduring question of how rules are to be enforced,[16] and those interested in joint and collective action asking how agreement on norms and

13 Research is moved by practical concerns here, as in the obvious cases of 'race' 'ethnicity' and 'gender'. But there is also increasing theoretical interest in the use of terms that designate human statuses rather than describing their empirical state. Status designations have fascinating self-referential features which are at last beginning to attract the attention they deserve.

14 Cf. Zilsel 2000.

15 Legal professionals hold more diverse and conflicting views on law than scientists appear to do. In the US, for example, we have 'strict constructionists' angered by 'judge-made law', and 'realists' who believe there is no other sort. Moreover, different forms and systems of law enrich the legal imagination. It is tempting to conjecture that case and precedent law eases the understanding toward finitism more than statute law does.

16 The conclusion to be drawn from a finitist account here is that any action at the foot of a hierarchy may be rationalised as in conformity with the relevant rules and instructions sent down from above. This is not to say of course that the rationalisation will be accepted by those above, who may make a Hobbesian response to the rationalisation, and indeed ought to do just that sometimes if their organisation is too big to fail. But it helps to account for the recent recession, and to explain why hierarchical organisations make extensive use of expensive lawyers.

what they imply in particular cases is sustained in groups.[17] And this last problem, whether with regard to norms or to values and principles has also long been faced by moral and ethical theorists.

By way of conclusion I want to emphasise the value of addressing this basic problem in different domains by referring to a philosopher who has habitually done just this. Jürgen Habermas moves back and forth between the sciences and other contexts, using his understanding of each in an effort to improve his understanding of the other, – something of which I wholly approve and which I have tried to do myself. But Habermas is a rationalist philosopher, and since he and I have started from opposed positions it is perhaps unsurprising that we have ended with opposed positions. Consider how he has linked science and moral philosophy. Habermas assumes that the knowledge of science has a propositional form, that it consists in statements with determinate universal implications that may or may not be true, the actual truth of which may be addressed through rational argument. And he goes on to assert that moral norms have an analogous propositional form and that their determinate meanings permit rational argument about their acceptability. My own belief, in contrast, is that scientific knowledge does not have a propositional form, that Habermas is led astray by his conviction that it does, and that in morals as in science contingent actions are rationalised as following from whatever formulations are taken to imply them. I have no desire to resolve the difficult issue of which of these positions is the more plausible here; but to be able to evaluate them in terms of exemplars drawn from two such different contexts can surely only be helpful to those who look for some sort of resolution in due course.

As it happens, however, Habermas has himself identified difficulties in his own account of moral norms, akin to those that would be exposed by a finitist critique. "No norm contains within itself the rules for its application" he tells us,

> Yet moral justifications are pointless unless the decontextualisation of the general norms used in justification is compensated for in the process of application. […] discourse ethics cannot evade the difficult problem of whether the application

17 Cf. Barnes 1995.

> of rules to particular cases necessitates a […] distinct faculty of […] judgement […]. The neo-Aristotelian way out of this dilemma is to argue that practical reason should forswear its universalistic intent […][18]

From a finitist perspective, of course, 'the application of rules to particular cases' is contingent action, and does indeed, with a vengeance, constitute a 'difficult problem' for a universalising rationalism.[19] It is interesting that the problem identified in this passage was accompanied by no solution; instead Habermas in effect set it on one side and continued to develop his theory of discourse ethics regardless.

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18 Habermas 1990, p. 206.

19 Cf. Barnes 2000, pp. 133 ff.

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Relativism, Incoherence, and the Strong Programme

HARVEY SIEGEL

Plato's Case for the Incoherence of Relativism

As is typical in philosophy, the articulation or characterization of a controversial doctrine is crucial to the assessment of arguments for and against it. Such characterizations are themselves controversial, but unavoidable if philosophical scrutiny of such doctrines is to take place. In full recognition of this ineliminable controversiality, in this paper I will take epistemological relativism to be the view that knowledge (and/or truth or justification) is relative – to time, to place, to society, to culture, to historical epoch, to conceptual scheme or framework, or to personal training or conviction – in that what counts as knowledge (or as true or justified) depends upon the value of one or more of these variables. According to the relativist, knowledge is relative in this way because different cultures, societies, epochs, etc. accept different sets of background principles, criteria, and/or standards of evaluation for knowledge-claims, and there is no neutral way of choosing between these alternative sets of standards. So the relativist's basic thesis is that a claim's status as knowledge (and/or the truth or rational justifiability of such knowledge-claims) is relative to the standards used in evaluating such claims; and (further) that such alternative standards cannot themselves be neutrally evaluated in terms of some fair, encompassing meta-standard.[1] A somewhat more technical characterization of epistemological relativism is as follows:

1 The character of such 'neutrality' is addressed in Siegel 2004.

ER: For any knowledge-claim p, p can be evaluated (assessed, established, etc.) only according to (with reference to) one or another set of background principles and standards of evaluation s_1, …, s_n; and, given a different set (or sets) of background principles and standards s'_1, …, s'_n, there is no neutral (that is, neutral with respect to the two (or more) alternative sets of principles and standards) way of choosing between the two (or more) alternative sets in evaluating p with respect to truth or rational justification. p's truth and rational justifiability are relative to the standards used in evaluating p.[2]

The doctrine of relativism, so understood, is usually traced to Protagoras, who is portrayed in Plato's *Theaetetus* as holding that "man is the measure of all things" ('homo mensura'), and that any given thing "is to me such as it appears to me, and is to you such as it appears to you".[3] Plato's Socrates characterizes Protagorean relativism as consisting in the view that "what seems true to anyone is true for him to whom it seems so".[4] This view is a form of relativism in the sense just explained, since for the Protagorean there is no standard higher than the individual – with her own specific location in time, place, culture, framework, etc. – with reference to which claims to truth (and so knowledge) can be adjudicated. But relativism is best understood as a more general doctrine than the Protagorean version of it, which places the source of relativism at the level of standards rather than (as for the Protagorean) at the level of personal opinion or perception, and as such aptly characterizes more recent, influential versions of relativism, including the one to be discussed in what follows. What can be said for and against it?

The Main Argument Against

Opponents of relativism have made many criticisms of the doctrine; by far the most fundamental is the charge that relativism is self-referentially

2 Siegel 1987, p. 6.
3 Plato 1961, 152a.
4 Plato 1961, 170a.

incoherent or self-refuting, in that defending the doctrine requires one to give it up. There are several versions of the incoherence charge. The most powerful[5] is that relativism precludes the possibility of determining the truth, justificatory status, or, more generally, the epistemic merit of contentious claims and theses – including itself – since according to relativism no claim or thesis can fail any test of epistemic adequacy or be judged unjustified or false.

Take Protagorean relativism as an example. If "what *seems* true [or justified] to anyone *is* true [or justified] for him to whom it seems so" (emphases added), then no sincere claim can fail to be true or be justifiably judged to be false. But if there is no possibility that a (sincerely held) claim or doctrine can be false, the very distinction between truth and falsity is given up; a 'false' belief is reduced simply to one which is not believed. While Protagorean relativism is in the first instance a doctrine about the relativity of truth, it is readily extended to matters of epistemic appraisal generally (as the bracketed insertions in the just-quoted expression of Protagorean relativism are meant to illustrate) and understood as asserting the relativity of standards of justification as well as those of truth. If read in this way, it follows from this form of relativism that there is no possibility that a belief sincerely judged by a person to be true or justified can be false or unjustified. The end result is that the very notions of truth and justifiedness are undermined. But if this is so, the doctrine of relativism cannot itself be true or justified. This can be seen by reflexively applying the doctrine to itself:

> *ER'*: *ER* can be evaluated (assessed, established, etc.) only according to (with reference to) one or another set of background principles and standards of evaluation s_1, …, s_n; and, given a different set (or sets) of background principles and standards s'_1, …, s'_n, there is no neutral (that is, neutral with respect to the two (or more) alternative sets of principles and standards) way of choosing between the two (or more) alternative sets in evaluating *ER* with respect to truth or rational justification. *ER*'s truth and rational justifiability are relative to the standards used in evaluating *ER*.

5 For others cf. Siegel 1987.

Relativism is thus (according to this argument) incoherent in that, if it is true (or justified), the very notion of truth (or justifiedness) is undermined, in which case relativism cannot itself be true (or justified). This undermining results because the relativism of standards alleged by the relativist renders it impossible to distinguish truth (and justifiedness) from its (their) contrary (-ies). The assertion and defense of relativism requires one to presuppose neutral standards in accordance with which contentious claims and doctrines can be assessed; but relativism denies the possibility of evaluation in accordance with such neutral standards. Thus the doctrine of relativism cannot be coherently defended – it can be defended only by being given up. Relativism is thus *impotent* – incapable of defending itself – and falls to this fundamental reflexive difficulty. Defending relativism non-relativistically is logically impossible, in that any such defense must appeal to that to which the relativist cannot appeal except by giving up relativism; while 'defending' relativism relativistically is not *defending* it, i.e., providing any non-question-begging reason for thinking it to be in any way epistemically superior to non-relativism.[6]

To put this fundamental difficulty facing the relativist in a somewhat different way: insofar as she is taking issue with her non-relativist philosophical opponent, the relativist wants both (a) to offer a general, non-relative view of knowledge (and/or truth or justification), and assert that that general view – i.e., that knowledge is relative – is epistemically superior and preferable to its rivals; and also (b) to deny that such a general, non-relative view is possible or defensible. But the relativist cannot defend the view of knowledge offered in (a), according to which relativism is epistemically superior to non-relativism, in a way consistent with her own commitment to relativism. On the other hand, 'defending' relativism in a way that does not assert its epistemic superiority is not to defend it at all; neither is it to engage seriously the cluster of issues that divide the relativist from her non-relativist philosophical opponent. Embracing (b) – i.e., denying that a general, non-relative view of knowledge (including the relativist view) is possible or defensible – similarly precludes the relativist from seriously engaging the issues to which her relativism is a response. Moreover, defending (b) requires a commitment to (a), which commitment the commitment to (b) itself precludes.

6 Siegel 1987, ch. 1, cf. Morris 2008.

In short: the relativist needs to embrace both (a), in order to see her position both as a rival to, and, further, as epistemically superior to, the position of her non-relativist opponent; and (b), in order to honor the fundamental requirements of relativism. But the mutual embrace of (a) and (b) is logically incoherent. For the embrace of (a) forces the rejection of (b): if relativism is the epistemically superior view of knowledge (i.e., (a)), then one general view of knowledge is both possible and defensible as epistemically superior to its rivals (contrary to (b)). Similarly, the embrace of (b) forces the rejection of (a): if no general, non-relative view of knowledge is possible or defensible (i.e., (b)), then it cannot be that relativism is itself epistemically superior to its rivals (contrary to (a)). Here again the argument strongly suggests that the assertion and defense of relativism is incoherent.

Can the Strong Programme overcome these difficulties? Let us see.

The Sociology of Science and the Strong Programme

The sociology of scientific knowledge concerns itself with the sociological processes through which such knowledge is generated or produced, the processes through which it is 'legitimated' and accepted within a particular community, and other sociological processes and phenomena which play a role in the collective human effort to know. Traditionally, this sort of sociological investigation into the production, acceptance, legitimation and dissemination of knowledge has been taken by sociologists and epistemologists alike to be distinct from genuine epistemological inquiry, for the most that can be expected from the former sort of inquiry is a descriptive, causal account of how some particular community *C* produced and came to accept some knowledge-claim *p* or theory *T*, while the truth and/or justificatory status of *p* and *T* cannot be settled by such causal accounts: *C*'s *regarding p as* true or justified, however caused, is one thing; *p*'s *being* true or justified quite another. In this way a sharp division between sociological and epistemological inquiry concerning science and its claims to knowledge has traditionally been drawn, a division which cedes to sociology the task of describing and explaining scientific beliefs and attitudes at the sociological level, and to epistemology the task of evaluating such beliefs and, more generally, dealing with the norma-

tive assessment of candidate knowledge-claims.[7] Indeed, it has seemed to many that the 'sociology of knowledge' is a misnomer, in that inquiry conducted under that banner happily ignores any distinction between genuine knowledge and its counterfeits, and is better called the sociology, not of knowledge, but of *belief*.

Leaving the question of what such inquiry should be called to one side, advocates of the Strong Programme explicitly reject, for the purposes of their inquiries, any such distinction between genuine knowledge and spurious impostors to that title, and explicitly accept that, for them, knowledge is nothing more than belief. As David Bloor puts it, "Knowledge for the sociologist is whatever men take to be knowledge. It consists of those beliefs which men confidently hold to and live by".[8] As Barry Barnes and Bloor, in their widely cited defense of relativism, write: "We refer to any collectively accepted system of belief as 'knowledge'".[9] Their preference for this "terminological convention"[10] concerning 'knowledge' – in contrast to the more usual 'convention,' which takes for granted that, since one of the central tasks of epistemology is to say what knowledge is, for purposes of epistemological theorizing it is of central importance to distinguish between genuine knowledge and spurious contenders for that title, however widely believed – has the unfortunate consequence that much of the debate between proponents and opponents of the relativism of the 'strong programmers' seems to be ineffectual, due to these very different understandings of 'knowledge.' Nevertheless, in view of the wide-ranging influence of the Strong Programme in the broad area of science studies, the centrality of relativism in the overall perspective of

7 It is worth noting that one of the protagonists of this section, Barry Barnes, once explicitly endorsed this distinction, and agreed that the sociologist's project is distinct from the epistemologist's: "The sociologist is concerned with the naturalistic understanding of what people take to be knowledge, and not with the evaluative assessment of what deserves to be so taken; his orientation is normally distinct from that of the philosopher or epistemologist." (Barnes 1977, p. 1). This acknowledgement does not sit well with his paper with Bloor (Barnes/ Bloor 1982) discussed below, since that paper emphatically rejects this distinction, and advocates epistemological relativism on the basis of the sociologist's concern with 'what people take to be knowledge' – indeed, far from these being two distinct projects, the epistemological point, according to that paper, follows directly from the sociological ones.

8 Bloor 1976, p. 2.

9 Barnes/Bloor 1982, p. 22 Fn. 5.

10 Ibid.

that programme, and the fundamental status of Barnes and Bloor's argument for relativism in that perspective, it behooves us to consider that argument here.

Central to their case for relativism is their claim that relativism is required for science: "Far from being a threat to the scientific understanding of forms of knowledge, relativism is required by it. Our claim is that relativism is essential to all those disciplines such as anthropology, sociology, the history of institutions and ideas, and even cognitive psychology, which account for the diversity of systems of knowledge, their distribution and the manner of their change".[11] There are three things to notice about this proclamation. First, it must be remembered that by 'knowledge' Barnes and Bloor mean belief; their claim is that social scientists studying alternative systems of belief and the dynamics of belief change at the social level, if that study is to be scientific, must study both systems thought by the sociologist to be normatively praiseworthy, and systems thought to be less praiseworthy. No epistemologist who rejects relativism, and who believes that the non-relative normative evaluation of belief is possible, need disagree with this. Second, since 'epistemology' is not included in the list of subjects for which relativism is thought to be essential, the passage, as far as goes, seems to be compatible with the rejection of epistemological relativism: holding that social scientists studying belief systems and the dynamics of belief change must conduct their studies in ways that don't invoke the social scientists' normative evaluations of the beliefs under study seems straightforwardly compatible with the legitimacy, in other contexts, of just such normative evaluations. But third, the proclamation is unclear as to the sense of 'relativism' alleged here to be 'essential' for social scientific inquiry: are Barnes and Bloor making the innocuous point that social scientists studying belief distribution and the dynamics of belief change must study belief systems of both epistemically meritorious and epistemically less meritorious normative status; or the philosophically more contentious claim that any such distinctions concerning epistemic merit are illusory? Only the latter would qualify their view as a version of relativism of the sort we are concerned with here.

11 Barnes/Bloor 1982, pp. 21–2. See also Barnes' contribution to this volume, which aims to defend this claim.

The answer to this question is, unfortunately, less than clear. On the one hand, they endorse what I just called the 'innocuous point':

> Our equivalence postulate is that all beliefs are on a par with one another with respect to the causes of their credibility […]. The position we shall defend is that the incidence of all beliefs without exception calls for empirical investigation and must be accounted for by finding the specific, local causes of this credibility. This means that regardless of whether the sociologist evaluates a belief as true or rational, or as false and irrational, he must search for the causes of its credibility. In all cases he will ask, for instance, if a belief is part of the routine cognitive and technical competences handed down from generation to generation. Is it enjoined by the authorities of the society? Is it transmitted by established institutions of socialization or supported by accepted agencies of social control? Is it bound up with patterns of vested interest? […] All of these questions can, and should, be answered without regard to the status of the belief as it is judged and evaluated by the sociologist's own standards.[12]

In this central passage Barnes and Bloor are clear that (a) epistemic evaluation is possible (although, as we will see in a moment, only relative to local contexts), even though the sociologist is to ignore such evaluation in her inquiries and investigate the causes of the credibility (or lack thereof) of all beliefs independently of their normative status, and (b) by 'causes of credibility' they mean those factors which cause believers to believe as they do, i.e., to regard some beliefs as credible and others not. The causes of a belief's credibility thus are not, for Barnes and Bloor (contrary to some causal theories of justification), those factors which cause beliefs to *be* justified or worthy of belief; they are rather the factors which cause beliefs to be *regarded* by believers *as* credible (although again, as we'll see in a moment, Barnes and Bloor reject this distinction). The epistemic status of all beliefs is thus left open: once the sociologist identifies the causes of community *C*'s regarding belief system *BS* as credible, her work is done. It is no concern of the sociologist to determine whether or not beliefs so regarded really are credible. So far, then, Barnes and Bloor are not committed to any philosophically controversial sort of relativism.

But they also endorse what I called above the 'philosophically more contentious claim', committing themselves to epistemological relativism of the sort with which we are here concerned. Discussing two tribes and their local epistemic predilections, Barnes and Bloor write:

12 Barnes/Bloor 1982, p. 23.

> The crucial point is that a relativist accepts that his preferences and evaluations are as context-bound as those of the tribes T1 and T2. Similarly he accepts that none of the justifications of his preferences can be formulated in absolute or context-independent terms. In the last analysis, he acknowledges that his justifications will stop at some principle or alleged matter of fact that only has local credibility […]. For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such. Because he thinks that there are no context-free or super-cultural norms of rationality he does not see rationally and irrationally held beliefs as making up two distinct and qualitatively different classes of thing […]. Hence the relativist conclusion that they are to be explained in the same way.[13]

Unlike the passage cited earlier, in this passage Barnes and Bloor clearly seem to endorse an epistemologically contentious form of relativism according to which 'non-local' epistemic evaluation, and a distinction between genuinely rational beliefs and those which are erroneously regarded as such, are philosophical fantasies. Let us examine their case.

As this passage makes clear, Barnes and Bloor reject the distinction drawn above between beliefs that are regarded, perhaps erroneously, as justified, and beliefs that actually are justified: "For the relativist there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such." This is parallel to their rejection of any distinction between genuine knowledge and a counterfeit taken by some to be genuine. Genuine knowledge, and 'really rational' beliefs, just are what people regard as such; to be *regarded as* genuine is to *be* genuine.

There are three points to make here. The first is that this 'locality claim' (let us call it) is not a consequence of the equivalence postulate concerning the causes of credibility of beliefs with which Barnes and Bloor define their brand of relativism; it is an independent dimension of their view which requires its own justification (to be considered below). The second is that their equation of genuine knowledge (and 'really rational' belief) and that which is taken to be knowledge (and rational belief) flows naturally from their initial decision to adopt the 'convention' according to which 'knowledge' is defined as belief. Insofar, their rejection of the 'is regarded as/is' distinction is of no epistemological moment, since epistemologists are concerned with a quite different conception of knowledge,

13 Barnes/Bloor 1982, pp. 27 f.

and are centrally concerned to distinguish the genuine article from imposters, however sincerely they might be embraced as genuine by some believers.

But third, Barnes and Bloor do offer a reason for rejecting any such distinction, namely that all such judgments of genuineness will themselves be only 'local':

> […] a relativist accepts that his preferences and evaluations are […] context-bound […]. Similarly he accepts that none of the justifications of his preferences can be formulated in absolute or context-independent terms. In the last analysis, he acknowledges that his justifications will stop at some principle or alleged matter of fact that only has local credibility.

That is, it is not possible for any cognizer, including the sociologist, to escape her local context and judge from some 'context-free', 'super-cultural' or context-independent perspective.

This claim is, in fact, nothing more than (a version of) the conclusion of the 'no transcendence' argument for relativism to be addressed next. Barnes and Bloor's argument is in the end one of very simple form: all judgment is local – no judgments have any positive epistemic status beyond that granted them by epistemic agents in some locale, and there is no getting beyond such locales to reach a context-independent platform from which to judge – therefore relativism. Let us consider this argument, before returning to further examination of the Strong Programme.

Is It Possible to 'Transcend' One's Perspective?

It is widely acknowledged that one can never completely escape one's perspective, framework, or conceptual scheme and achieve a 'God's eye view' or a 'view from nowhere'[14]; that all cognitive activity is inevitably conducted from some ongoing perspective or point of view. A typical expression of this thesis is Quine's:

> The philosopher's task differs from the others', then, in detail; but in no such drastic way as those suppose who imagine for the philosopher a vantage point outside the conceptual scheme that he takes in charge. There is no such cosmic exile. He cannot study and revise the fundamental conceptual scheme of science and common sense without having some conceptual scheme, whether the same or another no less in need of philosophical scrutiny, in which to work.[15]

14 Cf. Nagel 1986.
15 Quine 1960, p. 275–6.

Philosophers generally grant Quine's point: there is no 'cosmic exile' from all conceptual schemes; one cannot cognize except from within the confines of some scheme or other. But from the relatively uncontroversial claim that we cannot escape all perspectives and achieve a 'view from nowhere' or 'perspectiveless perspective,' it seems a short step to the relativistic conclusion that what we can know, or what can be true or justified, is itself relative to the schemes, frameworks or perspectives that inevitably limit our judgment; that, since there is no 'perspectiveless' judgment, there is no possibility of achieving a perspective that would allow us to non-question-beggingly compare and evaluate either judgments issued from different perspectives, or alternative perspectives themselves. That is, the uncontroversial claim that all judgments inevitably occur in the context of some perspective or other might be thought to entail that all judgments are therefore bound or determined by such perspectives, which are in effect inescapable – and so that what a given epistemic agent is able to know, or regard as true or justified, is problematically limited by her perspective or framework in such a way, or to such an extent, that relativism inevitably results. Is relativism correctly derived in this way?

It is not – or so I will argue. The alleged entailment just mentioned fails; even though we cannot attain a 'perspectiveless perspective,' in the relevant sense we can nevertheless 'transcend' our frameworks and perspectives. We must distinguish between transcending or escaping *any given* perspective from transcending *all* perspectives. Once this distinction is drawn, the 'no transcendence, therefore relativism' argument fails.

Let me first lay out the argument explicitly:

No Transcendence:

(1) Non-relative judgments require the possibility of getting outside of, freeing oneself from the influence of, or transcending one's perspective, framework, or conceptual scheme.
(2) It is not possible to escape or transcend one's conceptual scheme. There is no 'perspectiveless perspective' from which one can judge.
(3) Therefore, relativism.

Our question is: Is this argument any good? Does it establish epistemological relativism? I argue next that it does not, because of an ambiguity concerning 'transcendence'.

Are we limited by our perspectives, such that we cannot achieve any critical perspective on them? Are we really 'trapped' within our perspectives in this way? Common sense and every day experience suggest the contrary. Perhaps the most obvious range of counter-examples involves the cognitive activities of children. Three and four year old children, for example, can count and have a reasonable grasp of whole numbers, but have no understanding of fractions or decimals, i.e., parts of whole numbers that are themselves numbers. If asked 'is there a number between 1 and 2?,' they will answer in the negative, and will be unable to comprehend any suggestion to the contrary. But, given normal psychological development, within a few years such children will answer affirmatively; they will have no problem recognizing that, for example, 1.5 is a number between 1 and 2, and more generally, that there are non-whole numbers.[16] This seems a perfectly straightforward case of the modification of a perspective or framework, or of the abandonment of one framework for another, which belies the claim that we are trapped in, bound by, or limited to our frameworks.

Examples from the natural sciences and mathematics can equally easily be given. Consider, for example, the hard won recognition of the existence of things too small to see with the naked eye, achieved in large part by the invention and development of the microscope; or the interanimation of space and time and of the large scale non-Euclidean geometry of the universe, achieved over a period of decades around the turn of the twentieth century and culminating in Einstein's general theory of relativity. Both of these are cases plausibly described as fundamental changes in conceptual scheme or framework: the first as a fundamental change in our understanding of the range of existing things; the second as a fundamental change in our understanding of space and time themselves. Similar remarks apply to Cantor's discovery (and proof) of the differing sizes of infinite sets, and of other fundamental mathematical discoveries: they

16 Grasp of fractions and decimals usually involves a process which extends over several years and is in part a function of what is taught, when. The classic work in this area is Gelman and Gallistel 1978; it (including their account of what counts as a 'reasonable grasp' of numbers) is summarized briefly and lucidly in Moshman, Glover and Bruning 1987, pp. 420–3.

are plausibly described as fundamental changes in conceptual scheme. All of them involve the transcendence of prior schemes in favor of new, superior ones.

Very different sorts of examples can also be given. Consider, for example, the 'male sexist pig' who has no awareness or understanding of women other than as (sex) objects, but who in the course of his experience comes to realize (if only dimly) that he does treat women as objects, that many women want not to be so treated, and that there might well be something objectionable about treating women in that way. Suppose that this benighted male comes eventually to a full(er) awareness of the injustice of his earlier treatment of women; he comes to believe that it is wrong to treat women as objects and, over a considerable period of time and with the help of many women (and perhaps some courses in the Women's Studies Department), he develops a radically different and more respectful view of women and (hallelujah!) treats them accordingly. (Surely many men have had their consciousnesses raised to some extent in this way in recent decades.) Here again it seems that our subject has had his perspective altered and, indeed, improved; that is, he has 'transcended' his old sexist perspective for another.

In these examples not only have perspectives altered; the cognizers considered all regard their later perspectives as improvements; i.e., as better than, superior to, their earlier ones. If asked, these cognizers will be able to offer reasons which purport to justify those judgments of superiority. Those reasons, and the judgment that they are good ones that offer justification for the superiority of those later perspectives, are of course made from the perspective of those later perspectives or frameworks; they are not outside of all frameworks or issued from a perspectiveless perspective. Thus is acknowledged the uncontroversial second premise of the argument under consideration. But the conclusion is undermined by the several counter-examples offered: epistemic agents always judge from some perspective or other, but there is no reason to think that they are trapped in or bound by their perspectives such that they cannot subject them to critical scrutiny. In this sense, we *can* 'transcend' our perspectives; and this sense is sufficient to defeat the argument for relativism we have been considering. As Popper puts the point:

> I do admit that at any moment we are prisoners caught in the framework of our theories; our expectations; our past experiences; our language. But we are prisoners

> in a Pickwickian sense: if we try, we can break out of our frameworks at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one; and we can at any moment break out of it again.
>
> The central point is that a critical discussion and a comparison of the various frameworks is always possible.[17]

Here Popper clearly draws the crucial distinction that undermines this path to relativism. While the Quinean point that we inevitably judge from some framework or other, that we cannot judge from a perspectiveless perspective, must be granted, it does not follow that our judgments are necessarily tainted by the fact that they are made from some framework or other. On the contrary, we can and regularly do 'transcend' our frameworks from the perspective of other, 'roomier' ones, in which can fit both our earlier one and relevant rivals to it – and in this way fair, nonrelative evaluations of both our judgments and the frameworks/perspectives from which they are made are possible.

The 'framework relativist' may reject these alleged examples of transcendence, and in this way seek to preserve the argument we have been considering. This raises in a pointed way the question: what are 'frameworks', 'contexts', 'conceptual schemes', or 'perspectives', such that our judgments and our ability to know is bound by them in a way which precludes transcendence? I have thus far understood these locutions in an intuitive and rather uncritical way, since it seems clear that the examples given – do/do not recognize non-whole numbers, do/do not recognize the existence of objects too small to see with the naked eye, do/do not recognize the interanimation of space and time, do/do not recognize women other than as (sex) objects, etc. – are sufficiently general that such differences constitute differences in conceptual framework or scheme if anything does. Equally plausible examples of alternative schemes are the range of 'scientific revolutions' made so much of by Kuhn, for example the shifts from the Ptolemaic to the Copernican, Newtonian, and Einsteinian conceptions of the heavens. Understood so generously that all these examples are indeed examples of alternative frameworks or schemes, the argument for relativism based upon that generous understanding of these terms seems clearly deficient. Attempts to resuscitate the argument minimally require a more careful explication of these terms than I have given

17 Popper 1970, p. 56.

them here – and, it must be said, than defenders of 'framework' relativism have typically given them. Further, they require attention to Davidson's famous argument against the possibility of such alternative schemes, and hence of a version of relativism based upon them.[18] Absent such efforts, the 'no transcendence, therefore relativism' argument seems clearly to fail.

It remains to establish that the 'no transcendence' argument is actually the basis of the Strong Programme's relativism. Happily, this is easily done. That argument, as we have seen, depends upon the idea that we cannot transcend our schemes to achieve a 'perspectiveless perspective' or 'view from nowhere'. Barnes and Bloor's defense of relativism, cited and discussed above, rests on their contentions that evaluations of beliefs are inevitably "context-bound"; that justifications cannot "be formulated in absolute or context-independent terms"; that "justifications will stop at some principle or alleged matter of fact that only has local credibility"; that "there are no context-free or super-cultural norms of rationality" to which appeal is possible in justificatory efforts. Let us grant that all this is correct. As we have seen, none of it entails that non-relative epistemic evaluation is impossible. That the relativism of the Strong Programme rests on the 'no transcendence' argument is clear. That the argument fails is equally so.

Let us consider next some further difficulties with the relativism of the Strong Programme.

Further Difficulties with the Strong Programme's Relativism

Barnes and Bloor's 'equivalence postulate' insists that all beliefs, however appraised from whatever perspective, be dealt with in the same way by the sociologist: that is, their 'credibility' is to be explained causally. The sociologist's task is to identify the 'causes of credibility' of beliefs, i.e., the social forces that explain their development, acceptance, and change. This causal thesis is not something that the opponent of relativism need reject, since that opponent can simply distinguish between the causes of belief, on the one hand, and the epistemic status of belief, however caused, on the other. Barnes and Bloor would reject this distinction, since 'epistemic status' for them just means 'locally perceived epistemic status', and

18 Cf. Davidson 1984. For discussion, cf. Siegel 1987, ch. 2.

the causal question in which they are interested is precisely: what social forces cause belief system *BS* to be perceived, in a given locale, as having the status it is perceived to have? But the non-relativist can happily acknowledge the scientific legitimacy of the question. The important point here is that the legitimacy of the question, and the 'equivalence postulate' more generally, offers no support to relativism; the symmetry of explanation is perfectly compatible with the non-relativity of epistemic evaluation: The social forces (e.g., feudalism, religion, poverty, etc.) that brought about the acceptance of the Aristotelian belief system in the Middle Ages is one thing, the epistemic status of that system another. Of course Barnes and Bloor reject any non-relativist reading of the latter, but their reason for doing so – the 'no transcendence' argument – fails.

As we have seen, for Barnes and Bloor there is nothing more to 'knowledge' than community approval.[19] The task of the sociologist of science is not to give an epistemic account of why community *C rightly* regards some theory *T* or claim *p* as knowledge (or justified), but rather to give a causal account of community *C*'s coming to so regard them.

Consider the character of such a causal account. Presumably it will have the general form: '(Particular) social forces cause the credibility of belief systems within a given community,' or, schematically, '*SF* cause the credibility of *BS* in *C*.' So suppose the sociologist proposes such a causal account of belief credibility – say, that the belief that relativism is self-referentially incoherent is caused to be credible in the community of analytic epistemologists in the second half of the twentieth century by social forces involving the elite status of private research institutions, the reward system within such institutions, etc. How do Barnes and Bloor regard such accounts? As relativists, they seem to have no choice but to regard them relativistically: within community of sociologists C_1 – say, the one located in Edinburgh and environs in the last quarter of the twentieth century – social forces cause the belief in question to be highly credible; whereas within community C_2 – say, the one located around Merton in the United States in the third quarter of that century – that belief is caused by social forces to be less credible. In both communities credibility is just

19 This brings to mind Kuhn's famous remark that, with respect to paradigm choice, "there is no standard higher than the assent of the relevant community." (Kuhn 1962, p. 94) Here is one clear instance of Kuhn's influence on the Strong Programme in particular, and on post-Kuhnian sociology of science more generally.

'credibility-as-perceived-in-that-community'; to be *regarded as* credible is to *be* credible. Barnes and Bloor are clear that they accept this consequence of their views: the sociologist enjoys no special exemption from the 'equivalence postulate'; the credibility of her beliefs, like all scientific and other beliefs, is to be explained causally.

So far none of this poses any difficulty for Barnes and Bloor. But consider now the case in which two different communities of sociologists account for the credibility of a belief system in a third community, i.e., in which C_1 and C_2 offer alternative accounts of the social forces which cause a belief (system) to be credible in a third community C. Let C_1 and C_2 be the communities of sociologists just identified; let C be the community of analytic epistemologists in the United States and Western Europe in the third quarter of the twentieth century;[20] let *BS* be that system of beliefs concerning knowledge, truth, justification, etc., that includes the belief that relativism is self-referentially incoherent; let SF_1 be the social forces cited by C_1 as those which cause the credibility of *BS* in C (for example, social and economic forces involving the power structure, reward system, and student selection procedures of prestigious universities during the time period in question); let SF_2 be the quite different social forces cited by C_2 as those that cause the credibility of *BS* in C (for example, social forces that encourage respect for conservative values such as '(perceived) common sense', which is manifested in the members' of C's appreciation of the standard Platonic arguments for incoherence); finally, let CC_1 be the account of the causes of credibility of *BS* in C offered by C_1, and let CC_2 be the account of the causes of credibility of *BS* in C offered by C_2. The question is: how are we to think about these alternative accounts CC_1 and CC_2? Barnes and Bloor regard the evaluation of these alternatives as a scientific matter: the sociologist of knowledge is, after all, a scientist. But they also regard all such judgments as relative: the scientific worth of these accounts will be judged variously – or rather, will be caused to be credible to varying degrees – by scientists in differing communities. But this raises the question: why do Barnes and Bloor place so much impor-

20 For the record, Barnes and Bloor do talk about "the received culture of epistemologists" (Barnes/Bloor 1982, p. 39); there is nothing unfair in characterizing their view in such a way that specific academic groups – e.g., epistemologists, sociologists, and even particular 'schools' within these groups – constitute their own local communities which can be investigated sociologically in order to determine the causes of the credibility of their belief systems.

tance on the scientific character of sociological accounts of the causes of credibility of belief systems, if all such accounts will themselves have only local credibility?

To sharpen this problem: suppose Barnes and Bloor favor some particular CC_1, and their sociological opponents (the 'weak programmers') favor an incompatible CC_2, of the credibility of some *BS* in some *C*. As relativists, Barnes and Bloor seem forced to acknowledge that their preferred account CC_1 itself has only local credibility – i.e., it is caused to be credible in the community of strong programmers – while the account they reject, CC_2, is equally locally credible in the rival community of weak programmers. Is this sensibly regarded as a scientific account of scientific knowledge? Since judgments of the causes of credibility, and of the scientific merits of competing accounts of those causes, are themselves relative to locale, it seems that the Strong Programme's relativism is at odds with its scientific status.

This last point brings us, finally, back to the problem of incoherence. Barnes and Bloor appear not to have overcome this problem. First, as just noted, their yearning for a scientific sociology of science does not sit well with their endorsement of relativism, since the former requires a non-relativistic notion of causality, and a non-relativistic account of the specific causes of credibility of any particular belief system, that the latter precludes.

Second, their argument for relativism itself requires the rejection of that conclusion. Barnes and Bloor claim to show, in their discussion, that "the balance of argument favours a relativist theory of knowledge".[21] By this it is clear that they do not mean that their argument supports relativism only from the perspective of their own community of sociologists, but rather that it supports it generally, and should be found persuasive even by those outside that community (e.g., philosophers who endorse 'rationalism'). Insofar as they see themselves as providing a justification of relativism which has epistemic force beyond their local community of sociologists, and as providing a case for thinking that 'rationalism' is mistaken – as they clearly do see themselves as doing – their relativ-

21 Barnes/Bloor 1982, p. 21. Note that Barnes and Bloor are here explicitly calling their argument for relativism a contribution to the theory of *knowledge*, and not a contribution to sociology in particular or the social sciences more generally.

ism contravenes these claims. For if their arguments are successful, and their claims true (or justified), the epistemic status of these arguments and claims extend beyond the bounds of their local community, thus undermining their relativism. If, on the other hand, their relativism remains, then their claim to have arguments for it whose force extends beyond their community is undermined, since their relativism, according to which epistemic judgments are necessarily local and context-bound, explicitly rejects any such possibility. Either way, their relativism is incompatible with their claim to be able to justify it in terms of 'the balance of argument'. This combination remains incoherent: the latter depends upon a non-relative sense of 'argument' or 'evidence' which the former precludes.

Of course Barnes and Bloor could bite the bullet here and retreat to the view that the balance of argument does not favor relativism *tout court*, but does so only for those already on the inside of their community – that that balance favors relativism only locally, i.e., relative to their community. In this case, their argument would be presented as having no tendency or ability to establish the error of 'rationalist' ways to those in rationalist communities, let alone to fair-minded students of the issue generally. But if their case is indeed taken by them to be limited in this way, why bother making that case to the rest of us in the first place? Here we see again relativism's impotence.

Given the quite familiar way in which Barnes and Bloor face the incoherence problem, their attempt to deflect it requires brief comment. They eschew two alternative 'equivalence postulates' – that all "general conceptions of the natural order" are either equally false, or equally true – because they both "run into technical difficulties" involving incoherence.[22] In favoring their chosen 'equivalence postulate' concerning the 'causes of credibility' of beliefs, Barnes and Bloor believe themselves to have avoided these 'technical difficulties.' (Space precludes speculation concerning the causes of the credibility (for them) of *that* belief.) I have just argued that, on the contrary, those technical difficulties have not been overcome, mainly because, independently of their chosen equivalence postulate, they hold that all judgments of truth, justification, etc., are equally local and admit of no higher-order assessment. That is, they

22 Cf. Barnes/Bloor 1982, p. 22.

endorse the problematic 'no transcendence' argument for relativism – and this is sufficient to give life to the 'technical difficulties' involving incoherence.

To summarize: 1. Given their refusal to distinguish between knowledge and belief, Barnes and Bloor's arguments concerning the 'equivalence postulate' establish at most the relativity of belief. This sort of relativism is uncontroversial, indeed trivial. 2. The equivalence postulate concerning the causes of credibility does not entail relativism; only 'locality' – a quite independent thesis – entails this conclusion. 3. The argument for this thesis relies on the unsuccessful 'no transcendence' argument for relativism, and so fails. 4. A non-relative notion of causality appears to be required for the scientific study of belief that the Strong Programme recommends. 5. Finally, despite their heroic attempts to deflect it, the self-refutation/incoherence problem remains as much a problem for Barnes and Bloor as for other advocates of relativism.

None of this is to deny that science is a social activity, that scientists have interests other than the 'purely cognitive,' or that the sociological study of science is an eminently worthwhile undertaking – it is; they do; and it is. The question concerns not the viability or worth of the sociological investigation of science, but only the tendency of such investigation to support epistemological relativism. If my arguments succeed, it does not.

A More Recent Defense of the Strong Programme

One might think that my focus on Barnes and Bloor's famous paper of 1982, however influential it might have been, is unfortunately out of date. I must admit the legitimacy of the charge. Consequently, I next consider briefly a more recent paper of Bloor's, which I will argue does little to blunt the criticisms made thus far.

Bloor's 'Sociology of Scientific Knowledge' (2004) is a long, systematic, and thorough discussion of the Strong Programme's central tenets and the critical reaction to it over the years. It is a commanding paper, but, unfortunately, it does not resolve the issues at the center of the present effort. In fact, the problems with Barnes and Bloor's treatment of relativism in their classic 1982 paper remain problematic in Bloor's more recent discussion.

In this paper, Bloor endorses once again the 'innocuous reading' of relativism noted above: "For the purpose of the sociology of knowledge relativism is the thesis that the credibility of all beliefs calls for explanation in terms of local, contingent causes".[23] As explained above, no one interested in denying epistemological relativism need disagree. In this sense we're all 'relativists'.

However, Bloor denies the legitimacy of any 'evidential/social' distinction, insisting that the two are inextricably bound:

> Whatever measure is used [to determine warrant or evidential support] will have the character of a collective choice, and it will have to be sustained as a convention. The social, in other words, is right in there, in the midst of the rational process of warranting. Warranting is not acceptance minus the social, it is itself a process whose structure and content cannot be properly analysed without identifying its conventional and social dimension.[24]

This is an important passage, which I think is helpful in identifying a key point on which the advocates and critics of relativism talk past one another. Critics aren't talking about any such process. Warrant, as the critics (and epistemologists more generally) understand it, is an evidential relation obtaining (or not) between a claim and its purportedly supporting evidence. The epistemic relation of support is one thing; the process we utilize in our effort to measure it another.

Bloor concludes his lengthy discussion with the lovely closing remark:

> [The] critic of epistemological relativism […] must lay claim to absolute standards.
>
> There are bound to be those who believe they can evade this responsibility. They will think that they can reject relativism without, at some point, embracing absolutism. There will, no doubt, be talk of a "third-way", and of going "beyond" the choice between relativism and absolutism. But those who claim they are both non-relativists and non-absolutists are deluding themselves. Critics of the relativism of the sociology of knowledge should not prevaricate. They should have the courage of their convictions, and the clarity of mind, to declare their absolutism and to show the world the absolute values they have been vouchsafed. Having done this, they can then explain to the ever curious sociologist just how they accomplished this epistemological miracle.[25]

The relativism Bloor here defends is not that articulated by the 'in-

23 Bloor 2004, p. 936.
24 Bloor 2004, p. 950.
25 Bloor 2004, p. 953.

nocuous reading'. Rather, the 'epistemological miracle' that Bloor here challenges the non-relativist to embrace is that contemplated in the 'contentious reading': Namely, that of transcending all perspectives and specifying "some standards or beliefs [that] are really rational as distinct from merely locally accepted as such." But this, as we have seen, is just to invoke the flawed 'no transcendence' argument for relativism. Bloor is right that we can't perform that epistemological miracle, of judging from outside all perspectives. But it is not necessary to do that in order to reject the 'contentious reading'.

Here, as earlier, Bloor successfully defends the 'innocuous reading', but fails to distinguish it from the 'contentious reading'. Bloor emphasizes the importance of the participants in this dispute specifying the form of relativism embraced/rejected:

> Those who reject relativism sometimes fasten upon one special form of the doctrine, refute this to their satisfaction, and then allow themselves to proceed as if they had refuted relativism as such […]. The only real basis for identifying a position as relativist lies in its rejection of a corresponding form of absolutism.[26]

But Bloor himself sometimes defends the innocuous reading of relativism;[27] sometimes the contentious reading;[28] he sometimes criticizes the 'absolutism' that rejects the symmetry of explanation, sometimes that which embraces the 'view from nowhere' and 'epistemological miracles'. That is, he has not followed his own wise counsel to be clear about the 'absolutism' that his own brand of relativism rejects. In so far as he rejects an asymmetry of explanation, and embraces the 'innocuous reading' of relativism, the anti-relativist epistemologist can happily agree with him. In so far as he rejects the 'epistemological miracle' of the achievement of a 'view from nowhere' or 'perspectiveless perspective', the just-mentioned epistemologist can again agree. However, in so far as Bloor thinks that rejecting all of these types of 'absolutism' requires one to embrace the 'contentious reading', such that one's epistemic judgments and evaluations are necessarily 'local', and thus that alternative epistemic evaluations cannot themselves be fairly or non-question-beggingly assessed, he has fallen victim to the 'no transcendence' argument criticized

26 Bloor 2004, p. 935.
27 Cf. e.g. Bloor 2004, p. 936.
28 Cf. e.g. Bloor 2004, p. 953.

above. Yes, we always judge from our own, particular, perspective. From this the problematic form of relativism criticized at the outset simply does not follow. Despite Bloor's protestations to the contrary, the 'contentious reading' remains incoherent.[29]

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29 This paper draws on Siegel 1987, 2004, and 2011; I thank the publishers for permission. It was written for the conference on 'The Problem of Relativism in the Sociology of (Scientific) Knowledge' held at the Universität Siegen in March 2011. I am grateful to Markus Seidel and Richard Schantz for the invitation to participate in the conference, and to Seidel, Schantz, and the other participants for their helpful comments.

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Realism and Relativism

RICHARD SCHANTZ

I

Realism and relativism are among the oldest positions in philosophy, and I think that debates between them are among its oldest and, arguably, most fruitful debates. Since Protogoras, who famously claimed that man is the measure of all things, many philosophers have been suspicious of the central claim of truth absolutism, the claim that all statements, if true, are absolutely true. Only few philosophers, however, followed Protagoras' radical dictum that all statements are only relatively true which, if applied to itself, seems somehow to lead to dialectical incoherence. So many philosophers, though they typically reject global relativism, adopt local versions of truth relativism, relativism about specific domains of discourse or subject matters. Consequently, the form of truth relativism, or alethic relativism, they endorse is that many, or at least some statements are only relatively true. For example, one might maintain that the statements of mathematics are absolutely true, while typical statements of the empirical sciences are only relatively true. It is evident that relativistic theses will be more plausible about some subject matters than about others. We all agree that Einstein made the most important of all relativistic discoveries, the discovery namely, that statements about the motion of objects and about their mass as well as statements about the simultaneity of events are only relatively true, true in relation to a variable frame of reference.

II

To gain an understanding of the dialectical relationship between realism and relativism, it is helpful to begin with a characterization of the traditional philosophical debate between realism and anti-realism. It is helpful to begin this way because, as we will see, anti-realism seems to be a necessary presupposition of any serious form of relativism. In this context, I will also introduce another important philosophical figure, the epistemological skeptic.

The basic idea of realism about a particular domain can be roughly expressed as a conjunction of two theses, an existence thesis and an independence thesis: firstly the kinds of thing distinctive of that domain exist, and secondly their existence and nature are objective and independent of us, of our perceptions, thoughts and language. The things within the domain must be out there to be discovered rather than constructed or constituted by our minds or our conceptual schemes. Anti-realism rejects this conjunction of theses. Some forms of anti-realism attack the existence thesis by flatly denying that entities of the relevant kind really exist, while according to other forms entities of the relevant kind do exist but are not objective or have no independent status in reality.

Like relativism, realism and anti-realism are domain-specific positions. There are distinct categories of entities one can be realist or anti-realist about: physical objects, universals, mental states, space, time, moral values, God, numbers, meaning, and so on. Most philosophers are neither global realists nor global anti-realists. Rather, most of them are pickers and choosers, local realists about some kind of entities, and local anti-realists about others.

Let us take a look at the traditional debate between realism and anti-realism about the external, physical world, that is, the debate about whether physical objects and events exist independently of the mental. Realists maintain that there really are mountains, rivers and stars, and that their existence and nature, what they are like, are constitutively independent of what anyone happens to believe or say about the matter. Usually anti-realists do not dispute that there are such things. Quite the contrary, they maintain that, of course, everyday macroscopic objects do indeed exist but, so they typically add, not objectively, not independently of us. Rather, they contend, their existence is somehow relative to some conceptual scheme or framework or paradigm.

Realism and anti-realism, as so far characterized, are ontological or metaphysical theses. It is often suggested, however, that this characterization should be supplemented to include an epistemological thesis. Two proposals, not incompatible but highlighting different aspects of our epistemic situation, can be distinguished. Some realists, the epistemological optimists, claim that we are mostly capable of acquiring knowledge about the objective world; they are persuaded that, difficult as it may be, the world is principally epistemically accessible to us.[1] Other realists, however, the epistemological pessimists, are a bit more cautious in this regard.[2] They tend to stress the ever-present possibility of error and ignorance. There is no guarantee that our beliefs about the world, even if they are maximally supported by evidence, are true. Truth about the world, so they characteristically contend, is always potentially evidence-transcendent or verification-transcendent.

The crucial point is that, according to realism, there is a logical gap between our beliefs about the world, or our sensory experiences of it, and the way the world is in itself. The totality of our beliefs about the world is one thing, the objective world quite another; obviously, our believing something to be so-and-so, does not make it so. This basic realist conviction, apparently a platitude acknowledged by both naive and reflective common sense, is the main reason why many philosophers think that realism is threatened by a deep internal tension: if the world is constitutively independent of our experiences and beliefs, then how can we be confident of gaining any knowledge about it?

While the camp of the so-called epistemological realists seeks to combine its ontological thesis of the mind-independence of the world with the epistemological thesis that it is nevertheless humanly possible to gain knowledge about it, epistemological skeptics resolve the apparent tension in realism in a quite different way: they assert that it is impossible to know anything about the external, objective world at all; we can attain knowledge, at most, only of our minds and its ideas or representations. This is the standpoint of skepticism about the external world. We can know neither that there is an external world nor, should there be one, what it is like.

In this dialectical situation anti-realists and relativists of various stripes –

1 Cf. Davidson 1986, Devitt 1984, McDowell 1994.
2 Cf. Nagel 1986, Stroud 1984, Williams 1978.

idealists, verificationists, phenomenalists, Kantians, social constructivists etc. – enter the stage. Deeply convinced that metaphysical realism opens the door to skepticism, anti-realists argue that the realist conception of an independent world behind the appearances, beyond the world as we perceive it and believe it to be, is wrong or even incoherent. They often urge that there are significant connections between understanding and verification, which the skeptic simply ignores. The only world there is, the only world we can find out about, is the world our senses present to us. A hypothesis with no connection to experience is regarded as spurious; after all, it could never be verified or falsified. So anti-realists tend to assert that we cannot even understand the skeptic's speculations about the wildly different ways the world might really be, even though all our experiences remain unaltered. According to anti-realism, there is indeed an ordinary perceptible world, a world of trees, tables, and stars, but this world is, in a philosophically significant sense, dependent on, or constituted by, or relative to, our epistemic activities. So what the anti-realists are willing to abandon is the distinctive realist conception of the world as what is there anyway, the alleged objectivity and autonomy of the world. To block skepticism, they offer us a revisionary ontology, which marks a considerable departure from the deeply entrenched metaphysics of common sense.

The skeptic, on the other hand, need not deny the existence of an independent world. She is an agnostic, who merely claims that we do not know whether or not such an objective world, a world as the realist conceives it, really exists. To achieve her aim, it is enough to raise reasonable doubts about our beliefs concerning the external world, thereby seeking to show that we are not justified in holding these beliefs, and so do not know that they are true. Anti-realism is a drastic reaction to skepticism; it is a radical form of anti-skepticism. Our knowledge of the world is unproblematic and secure because it seems to be not very difficult to know what is going on in our own minds – in our subjective or internal world.

The fundamental realist conviction that the facts of the world are not bound to be what we take them to be is often expressed as a thesis about the relation between truth and justification: our beliefs about the objective world, even if they were maximally supported by evidence, might still be false. Truth, for the realist, is a radically non-epistemic notion,[3] and

3 Cf. Putnam 1978, p. 125.

so the truth of our beliefs about the world is always potentially evidence-transcendent. As Rudolf Carnap pointed out some time ago, truth is one thing, justification another.[4]

Some realists, and I am among them, are persuaded that the best way to explicate their conception of truth is to embrace a version of the correspondence theory of truth whose central thesis is that a proposition is true just in case there is a fact to which it corresponds, and false just in case it does not correspond to a fact. According to them, truth is, basically, a dyadic relationship between a truth bearer – a statement or belief – and a truth maker, a reality that has an objective status with respect to the truth bearer. Truth makers are familiar entities: objects having certain properties and standing in certain relations to each other at various spatiotemporal locations. Further, they think that truth can be explained by objective referential relationships between language and the thought expressed on the one hand and the external world on the other.[5] Such a referential explication, then, allows us to develop a plausible form of the correspondence theory of truth that can take the intuitive idea seriously that a statement is true if and only if the fact or state of affairs actually obtains whose obtaining is asserted by the statement. Correspondence does not require something as pretentious as a relation of structural isomorphism between statements and facts, as early Ludwig Wittgenstein und Bertrand Russell once thought.[6] Ordinary reference is enough.

The fact that tomatoes are red is both a necessary and a sufficient condition for the truth of the statement that tomatoes are red. It is true that p if and only if p. Facts, as I see them, are not completely artificial, "sentence-shaped" objects, mere shadows cast by our linguistic practice of making statements, as Peter Strawson and Donald Davidson suggest.[7] And facts are not true thoughts either, as Gottlob Frege contended.[8] Rather, facts are components of the objective, mind-independent spatiotemporal world. Neither the existence nor the nature of facts depends, in any philosophically significant sense, on what we believe, think or perceive. Moreover, an essential element of the realist view is that truth transcends verifiability, that a statement can be true even though we, beings with

4 Cf. Carnap 1935.
5 Cf. Schantz 1996, 2001.
6 Cf. Wittgenstein 1922, Russell 1912.
7 Cf. Strawson 1950, Davidson 1990.
8 Cf. Frege 1919.

our sensory and cognitive nature, are not at all able to verify it. Truth and objective reality cannot be reduced to what we are able to ascertain. Truth, according to alethic realism, is evidence-transcendent; it exceeds rational acceptability.[9]

But, actually, realists need not espouse a detailed form of the correspondence theory of truth. It is also open to them to adopt a version of the so-called deflationary or minimalist conception of truth, and many contemporary realists do so. The central claim of deflationism is that the various attempts of traditional philosophers to analyze the inner nature of truth were misguided, and thus must be deflated.[10] Truth, on the deflationary account, has no underlying nature. Hence the concept of truth is not a philosophically contentious concept, not a concept that stands for a substantial or robust property or relation. On the contrary, on their view truth is a purely formal or logical concept, a concept whose correct explanation requires far less extravagant conceptual resources than adherents of substantive theories believe. This is so, they claim, because propositions expressed by sentences of the form "The proposition that p is true" are logically equivalent to the proposition expressed by p itself. Consequently, the whole content of the truth-predicate is given by the totality of appropriate instances of the conceptually fundamental equivalence schema "The proposition that p is true if and only if p". The parallelism with Alfred Tarski's famous equivalences of "form T": "X (the sentence 'p') is true if and only if p" is obvious.

The crucial point, for the realist, is that the deflationary perspective shares the idea that the truth of a belief about the world depends on the way the world is, and thus is, in at least a minimal sense, a matter of correspondence or fitting the facts. Truth is, both for the authentic correspondence theorist and for the deflationist, a radically non-epistemic concept, a concept without any conceptual connections to verification, justification or other epistemic notions. Anti-realists and relativists tend to overlook the option of connecting realism with a deflationary account of truth. Commonly, the target of their assaults is the traditional combination of realism with a robust correspondence theory of truth.

The central realist idea, however, that truth is an epistemically unconstrained concept, has quite often been regarded as an entering wedge for

9 Cf. Schantz 1996.

10 Cf. Horwich 1990, Quine 1990, Soames 1999.

a variant of the skeptical challenge. The objection is often raised that if the truth of a belief does not in any way depend on an internal trait of it, such as its epistemic status, but only on a relation to something external, a transcendent fact in the objective world, then the tie between justification and truth is severed. On the realist view, so it is often argued, the criterion of truth and the nature of truth seem to be torn apart, with the disastrous consequence that it becomes impossible to determine whether our beliefs are true. This is supposed to be so because in order to determine whether a belief is true we would have to determine whether it corresponds to a fact. But we cannot compare a belief with reality because there is no direct, conceptually unmediated grasp of facts or objects which we are simply given to our consciousness. All our epistemic states are conceptually structured and have a propositional content. Thus, in attempting to apprehend the external side of the correspondence relation, we find ourselves with just another belief and so end up comparing a belief with a different belief ¾ even if it is a perceptual belief. We can never get outside the circle of our beliefs to inspect the independent facts that are supposed to make our beliefs true.

I have scrutinized and rejected this argument elsewhere and defended the intuitive standpoint that we are indeed capable of comparing our beliefs with the facts.[11] Why is such a comparison supposed to be impossible? I have found no compelling reason for this supposition. Indeed I think that no mysterious feat is required for comparing our beliefs with the world. Often it seems to be supposed that realism requires something like a pure, unmediated presentation of facts to our awareness in order to be viable. But realism requires no such thing. All that is required are commonly accepted methods for acquiring knowledge. It doesn't matter how we come to know the facts. What is essential is only that we do come to know them. I believe we have, under standard conditions, through perception a direct cognitive access to external facts, an access that enables us to ascertain whether or not these facts render our beliefs true. But the directness or immediacy is not really essential. To determine whether the statement that p corresponds to a fact we just have to determine whether p – no matter how, whether directly or by more or less indirect routes, such as induction and inference to the best explanation. Thus realism,

11 Cf. Schantz 1999.

even in combination with the correspondence theory, by no means leads to the absurd conclusion that beliefs and judgments are the only things to which we have a cognitive access.

III

So far I have portrayed realism with respect to the physical world in space and time and defended this position against an extremely influential epistemologically motivated anti-realist and relativistic objection. Let us now turn to areas where the prospects for anti-realist and relativistic convictions seem to be much brighter. These are areas where the realist conception of a realm of absolute, mind-independent facts that might exceed the limits of humanly attainable knowledge and the closely related conception of truth as epistemically unconstrained seem much less plausible or even quite bizarre. Such areas seem to abound.

Many philosophers have defended relativistic views about subject matters which have a certain normative character, such as ethics, aesthetics and epistemic justification, and even more philosophers are convinced that a form of relativism about judgements of taste, about what Crispin Wright in a judicious article nicely calls "disputes of inclination",[12] simply must be right. Take the statement "Chilli is delicious". One might come to think that this statement is true relative to some person's standard of taste, or perspective, and false relative to another person's culinary standard. We seem to be faced with a genuine disagreement, with incompatible attitudes in that one person affirms something that is inconsistent with what another person affirms. But neither of them seems to be wrong or at fault with regard to the basic norms to evaluate acts of assertion of these kinds. It is not the case that their assertions are equally justified, on the basis of the evidence available to them, although at most one of them is true. Relativism is not an epistemological stance, but a metaphysical stance. The reasons for the irresolvability of the disagreement are not epistemic but metaphysical reasons. They do not lie in our limitations to know the relevant facts. Rather, the relativist proclaims, there just are no relevant facts waiting to be discovered.

Many philosophers are so fascinated by the idea of faultless disagree-

12 Wright 2006, p. 38.

ment that they have attempted to extend it to other allegedly non-objective areas such as morality, aesthetics, justification and, much more controversial, even to theoretical science and ontology. So we have good reason to throw a careful glance at this apparently forceful thought. Faultless disagreement can be characterized as a conjunction of two theses: Firstly, there can be inconsistent statements about the same subject matter which, secondly, are nevertheless equally correct, so that nobody need be in error. That sounds somehow puzzling. Indeed, if appearances are taken at face value, the idea seems even to be incoherent. The propositions the disputing parties assert cannot both be true, for that would be an outright violation of the law of non-contradiction. The point is pretty simple. Suppose that A asserts p, and B asserts not-p. Now assume further, as requested, that both assertions are correct. But if p is true, then B's assertion is false, and if not-p is true, then A's assertion is false. Hence both assertions cannot be correct. It seems that disagreements have to be conceived as ordinary disagreements in which at most one participant can be right. Obviously, however, ordinary disagreements are of no help to the relativist, since everybody is prepared to recognize them.

Of course, the relativist does not want to be committed to allowing exceptions to the law of non-contradiction. So in order to neutralize this serious objection, some philosophers have proposed a conception of relative truth. The truth or falsity of beliefs, or of the propositions that are their contents, are not absolute but, rather, must be relativized to certain parameters.

To get a grip on the idea, let us consider the sentence type "It is raining". According to standard semantics, this sentence does not express a complete proposition or truth-conditional content. It is particular utterances of this sentence type that do express complete and determinate propositions because, when uttered in specific contexts, they express propositions of the form "It is raining at location l at time t", where the values of l and t are determined by the context of the utterance. We can say that utterances of the first sentence are elliptical for utterances of the expanded sentence. So, on the standard semantic account, the truth of an utterance of "It is raining" is relative to the context of utterance because the proposition expressed, what is said, by such an utterance is relative to the context in which it is made. Hence one utterance, when made in London at t_1, would express the proposition that it is raining in London at t_1, while

another utterance, made in Paris at t_2, would express the proposition that it is raining in Paris at t_2. And, of course, one of these utterances might be true, and the other false.

In an influential article, John Perry claims that the content of "It is raining" is not a complete proposition but a propositional function, true of some places and false of others.[13] The place is not determined by the literal semantic content of an utterance of this sentence type but, rather, by the situation in which the utterance comes off. His leading idea is that the proposition expressed by an utterance may contain "unarticulated constituents", constituents which are unrepresented in the overt syntax of the sentence uttered. The sentence as a whole is about the unarticulated constituent, even though this element does not correspond to any part of the sentence. Perry maintains that unless the proposition semantically expressed by an utterance of this sentence contained a place, the proposition would be incomplete, and hence, would not be truth evaluable. Moreover, he goes on to argue that unarticulated constituents are not even represented at the level of the deep syntax; the sentences contain no hidden indexical expression that refers to the location. But this point is more controversial. Fortunately, we do not have to enter into this thorny debate here.

What is important is that by now it is widely recognized that no satisfactory semantic theory can ignore the role of contexts in which expressions are used and interpreted. In recent years, a lot of powerful machinery has been developed by philosophers of language, philosophical logicians and theoretical linguists for the scientific study of contexts and the discourses that take place in them. The times when context-dependence was regarded as a defect of natural languages are over. Rather, many authors presently talk about the "dynamic turn" in the semantics of natural languages. Consequently, the interface between semantics and pragmatics has become a significant field of linguistic research. So Jon Barwise and John Perry have developed a relation theory of meaning, "situation semantics", which represents the meaning of a simple declarative sentence as a relation between situations, between an utterance on the one hand, and a described situation on the other.[14] The interpretation of a statement

13 Cf. Perry 1986.
14 Cf. Barwise/Perry 1983.

made with such a sentence on a certain occasion is the described situation. And Hans Kamp developed the theoretical framework of the "Discourse Representation Theory", which extends Richard Montague's pioneering model-theoretical approach so as to be capable of doing justice to the semantic dependencies among different sentences in a discourse and to provide interpretations not only of individual sentences, but also of discourses as wholes.[15] In this respect Discourse Representation Theory marks a clear break with received formal semantics.

Hence nowadays, formal semantics and philosophy of language incorporate the crucial insight that the propositional content of an assertive utterance is underdetermined by the linguistic meaning, or the semantic content, of the sentence uttered. The information a context-sensitive sentence conveys goes well beyond its conventional significance. Additional content-determining factors have to be taken into account to complete the content, such as, first and foremost, objective features of the context of utterance, such as the speaker, audience, place and time. But the content-determining factors also include general conversational rules and background assumptions known to be shared by the participants in a conversation. Very often, obvious facts about the conversational context interact with the meaning of the sentence uttered to determine the proposition expressed. We have to recognize that what is said by the utterance of a sentence in a context is also contingent upon the context of interpretation. It is an important fact that the propositional content of an assertive utterance of a sentence depends both upon the context of utterance and the context of interpretation.

Let us call this basic position "contextualist semantics", or simply "contextualism". Some authors call it "expressive relativism" or "content relativism". The only thing that is relative, according to contextualism, is propositional or truth-conditional content. But these contextually determined contents have absolute truth-values. This is surely the mainstream view, and, on this view, truth is not relative in any philosophically interesting sense.

In contrast to contextualists or content relativists, truth relativists want to do justice to the idea that there are genuine disagreements over a common subject matter and that idea allegedly calls for contents shared be-

15 Cf. Kamp 1981.

tween the parties of the disagreement, contents constant across contexts. Evidently, only if there are shared contents, can one person assert the very same proposition that another denies. That is the reason why truth relativism only enters the stage after truth-evaluable content has already been fixed. Its central claim is that propositional truth itself is relative.

The proponents of truth relativism typically claim that their novel semantic analyses are just extensions of more familiar forms of relativism. In possible worlds semantics, propositions are evaluated relative to possible worlds. A given proposition may be true relative to a world w_1, and false relative to another world w_2. In tense logic, propositions are evaluated relative to times. For example, the temporal proposition that Maria is sleeping may be true relative to a time t_1, and false relative to another time t_2. Temporalism is the view that truth and falsity are properties of propositions that they can lose. Eternalists deny this emphatically. They maintain that truth is a stable property; if something is true at all, then it is true once and for all. Frege in his „Kernsätze zur Logik" said: „Jede Wahrheit ist ewig" ("Every truth is eternal").[16] He was deeply convinced that propositions have their truth values absolutely, without relativization to anything, hence a fortiori without relativization to time. Contemporary alethic relativists, however, are not afraid of temporally indeterminate propositions. Rather, following the lead of David Kaplan and David Lewis, there is a consensus among them that truth must be relativized at least to triples of <world, time, location>.[17] But the strategy of parametrization, of adding new parameters – parameters for a standard of taste or a sense of humour, for example – into the circumstance of evaluation has been exploited further.

Consider now again the sentence type "It is raining". Truth relativists maintain that this sentence type does express a context-insensitive complete proposition, namely the proposition that it is raining, and that every utterance of this type expresses exactly the same proposition. The explanation offered for the fact that one utterance of this type in a certain context may be true, while another utterance of it in another context may be false, is that, on the relativistic view of truth, the proposition that it is

16 Frege 1876, p. 190 (175).
17 Frege 1876, p. 190 (175).

raining does not have an absolute truth value, but only a truth value relative to the context of utterance. The truth of one and the same proposition is relative to different contexts of utterance.

There are some interesting differences between various versions of truth relativism. On Max Kölbel's version, the truth of a proposition is relative to what he calls a "perspective", while on John MacFarlane's version truth is relative to what he calls a "context of assessment". MacFarlane puts emphasis on the distinction between contexts of assessment and contexts of use or utterance, and argues that his account is superior to the one proposed by Kölbel precisely because the latter does not recognize that the relevant context to which truth must be relativized is the context of assessment. For our present purposes, we can ignore the subtle differences between these suggestions and focus on their common core, which consists in the assumption that there are operative points of assessment with respect to which propositions have to be evaluated as true or false.

Remember, relativism about truth is supposed to rescue belief in faultless agreement from the charge of inconsistency. The basic idea is that p may be true relative to A's perspective while not-p may be true relative to B's perspective, although "p and not-p" is not true relative to any perspective. Since truth is relativized to different perspectives in this way, one can no longer simply deduce from the fact that p and not-p cannot both be true, that A and B cannot both assert true propositions.

Contemporary advocates of truth relativism are at great pains to distinguish their view from rival explanations of the phenomenon of faultless agreement, explanations, which, from the relativist's standpoint, do not really succeed in explaining the phenomenon but, rather, tend to explain it away. In particular, truth relativists insist that their view must not be confused with any version of contextualism. Contextualists, if they are prepared to extend semantic context-sensitivity into the sphere of moral discourse, may suggest that we should not judge simply that "Kicking babies for fun is wrong", but only that "Kicking babies for fun is wrong according to some moral framework M". But if the contents of the apparently inconsistent judgements that "Kicking babies for fun is wrong" and that "It is not the case that kicking babies for fun is wrong" are really elliptical for judgements that are actually consistent when properly expanded – "Kicking babies for fun is wrong according to some moral framework M" and "It is not the case that kicking babies for fun is wrong

according to some moral framework M*" – then we can no longer capture the sense in which the participants are disagreeing. Both could be right, but they would mean different things by their utterances and so would actually talk past each other.

But does the truth relativist's account really fare better in this regard? Does it succeed in retaining a sense of disagreement? I don't think so. On this account too both parties can agree. Surely, they can agree that "Kicking babies for fun is wrong" is true relative to moral perspective M_1, and that It is not the case that kicking babies for fun is wrong" is true relative to the different moral perspective M_2. The crucial point is that, on both the contextualist's and the relativist's accounts, the truth of the relevant propositions is determined by the conflicting moral standards. What one account presents as an additional parameter to which truth must be relativized, the other presents as a contextual factor variation in which yields variation in propositional content. Both accounts, as far as I can see, are incapable of explaining the alleged phenomenon of faultless disagreement. They stand or fall together. My suspicion is that faultless disagreement is an illusion. When the smoke has cleared, we begin to see that there are only ordinary disagreements in which at most one party can be right. Hence, a convincing formulation of the doctrine of truth relativism must be based on a different idea.

IV

Let me end with some reflections on the notion of truth adherents of truth relativism are working with. What is their account of truth? They are somewhat coy concerning this central question. Naturally, they reject the very possibility of a correspondence theory of truth. They don't believe in frame-independent truth-makers that could render our statements true. But neither, it seems, can they embrace deflationism about truth. The leading idea of deflationism is that the equivalence schema TS: "The proposition that p is true if and only if p" is definitional of the concept of truth. The claim is that the meaning of the truth predicate is given by the totality of appropriate instances of this schema, so that all that is required to grasp the concept of truth is contained in instances of it. To explain, for example, what it is for the proposition that the earth is round to be true, one can hardly do better than to point out that it is

true if and only if the earth is round. And so on for propositions generally. The trouble is that this schema does not mention relative truth at all. So if deflationism is correct, and all there is to know about truth is given by instances of this schema, then truth relativism is wrong. The relativist might propose the following schema instead: "The proposition that p is true if and if the proposition that p is true relative to a framework F". But this schema cannot be derived from TS, at least not without additional and highly contentious assumptions.

The truth schema TS, the modern propositional variant of Alfred Tarski's famous Convention T, is fundamental to our understanding of truth. Divergences from it should not be taken lightly. According to TS, truth is a monadic property of propositions. I think that is the correct view. Of course, one need not deny that one can define relational truth properties, such as being true at a world. What should be insisted upon, however, is that these relational truth properties have to be explained in terms of the more fundamental property of truth simpliciter.[18] Fans of alethic relativism, strongly influenced by the conceptual apparatus of possible worlds semantics, are inclined to reverse this order of explanation. If one is convinced that truth relative to a world is the fundamental notion, one has to conceive of ordinary truth as truth relative to a particular world – the actual world. But the actual world is not just one world among many others. It is the only reality there is. Therefore, we can simply say that the proposition that Paris is the capital of France is true without having to add that it is true at the actual world. No doubt, possible worlds semantics provides us with an extremely useful formal machinery for analyzing and understanding modal logic. But we must be careful not to overestimate the metaphysical consequences of this successful style of semantics.

So we still have no satisfactory answer to the question of what positive characterization the relativist can give of the concept of truth he employs. It might seem that there is a close linkage between epistemic analyses of truth and alethic relativism. Many anti-realists have proposed epistemic analyses of truth, analyses that attempt to define truth by verifiability, by rational assertibility, by permanent credibility, or by justifiability under ideal conditions. Obviously, truth cannot simply be identified with justification simpliciter. There are countless beliefs that were once justified

18 Cf. Cappelen/Hawthorne 2009.

for certain persons at certain times, but which later turned out to be false. Justification is tensed and so can be lost. But, at least for objectivist regions of discourse, truth is a stable property, a property that cannot be lost. Aware of our fallibility, testified by the enormous changes that have happened in the history of human thought, advocates of epistemic accounts came to believe that truth should not be bound to what is justified by present standards. Our present epistemic situation may be imperfect; it may not include all the relevant evidence. Rather, so the suggestion in the tradition of Charles Sanders Peirce, truth consists in coherence with the system of beliefs that human investigators will hold at the final stage of inquiry, in the limit of an ideal science that has all relevant evidence at its disposal. Peirce famously claimed: "The opinion which is fated to be ultimately agreed to by all who investigate, is what we mean by the truth".[19] More recently, Hilary Putnam, during his interim internal realist phase, maintained that truth is an idealization of rational acceptability. A statement is true, he said, if it would be justified under epistemically ideal circumstances.[20] It should be mentioned, however, that he later unequivocally abjured any attempt to define truth in epistemic terms and returned to a position he came to call "common-sense realism", a position that acknowledges that truth is sometimes recognition-transcendent.[21]

The ideal justifiability account of truth seems to be afflicted with several serious difficulties. Critics have raised the objection that the ideal justifiability account of truth is circular, since its main concept of an epistemically ideal situation ultimately cannot adequately be defined without reference to the concept of truth.[22] Proponents of the epistemic approach to truth have wisely abstained from making any serious attempt to specify what ideal epistemic conditions for a given belief involve. Evidently, however, in such ideal situations the possibility of error must be ruled out unquestionably. Hence one might suggest characterizing ideal conditions as conditions in which all relevant sources of error have been identified. But it seems hardly possible to understand this suggestion without a prior grasp of the concept of truth.

Be that as it may, this moderate verificationist proposal does not square

19 Peirce 1934, 5.407.
20 Cf. Putnam 1981, p. 55.
21 Cf. Putnam 1994.
22 Cf. Alston 1996, pp. 180–230, Schantz 2007.

with the relativist's intentions anyway, because a belief that is justifiably or rationally assertible in the ideal limit will have no rival and thus will be absolutely true.

So, it still seems that we have no satisfactory answer to the question of what truth is for the relativist.

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Theory-Ladenness and Relativism

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Introduction

"Do Kepler and Tycho see the same thing in the east at dawn?"[1] This problem, formulated by Norwood Russell Hanson,[2] brings forward a line of thought that can lead us straight to theory-ladenness of observation: Imagine them standing side by side watching the sunrise, but while Johannes Kepler sees the earth moving, Tycho Brahe sees a moving sun. Suppose each visual apparatus is well functioning, the only serious difference between them seems to be a difference in the theories or hypotheses they already have. This might yield the assertion that their observations are laden with theory.

If however observation, e.g. in scientific context, is dependent on already accepted theories, it seems reasonable to have significant doubt about the absolute status of scientific results: They seem to be relative to

1 Hanson 1969, p. 5.

2 Although the Kepler-Tycho example is dominant throughout Chapter 1 of his "Patterns of Discovery" it is an interesting fact that Hanson does not quite get it right. Ptolemy and Copernicus differed over two things. First, they differed over whether the sun is moving around the earth (geocentric) or the earth around the sun (heliocentric). And secondly, Ptolemy thought of the earth as a static celestial body (geostatic) while Copernicus conceived the earth as moving about its own axis. Kepler and Tycho only differed over one of these two things. They both agreed that the earth is rotating about its own axis (non-geostatic), their disagreement rested on the question of what to put in the center of the picture. But Hanson's recurrent question: "Do Kepler and Tycho see the same thing in the east at dawn?" can only be motivated by a disagreement on whether the sun is moving around the earth or the earth rotating about its own axis (geostatic or non-geostatic). Well, that had never been the contentious point between Kepler and Tycho.

a scientific community, which holds theories that specify what can be observed and by which methods observations are to be interpreted. This is why the problem of theory-ladenness often appears in relativism-related writings of philosophers like Thomas Kuhn or Paul Churchland.

The general aim of this paper is to figure out whether perception is theory-laden in a way that leads to epistemic relativism. We will thus look at different views on perception and discuss their tendency to be theory-laden. In a second step, we will figure out what follows from each of these findings for relativism. Before however we attend to this task, some clarifications are needed:

First, because of their centrality in this paper as well as their vastly differing use in other papers on the topic, we will define what we mean by the concepts of 'relativism' and 'theory-ladenness'. Second, we will explain our procedure: Why exactly do we investigate the relation between theory-ladenness and relativism only with respect to the potential implications of theory-ladenness for relativism (and not the other way round)? And why do we examine perception rather than observation?

Definitions

Relativism: Relativism asserts that different judgements can only be assessed relative to a particular, limited standpoint. In order to have a common denominator for different kinds of relativism, it makes sense to start with a rather broad and formal definition by saying that all forms of relativism have in common the claim that a *subject-matter X is relative to some framework F*.

Kinds of relativism can thus be further distinguished by considering the nature of the object X that is relativized (moral, epistemic, aesthetic, etc.) and the kind of framework F it is relativized *to* (culture, language, history, etc.).

Epistemic relativism is ususally the relevant form of relativism when it comes to theory-ladenness discussions. Here, the object relativized is knowledge. As knowledge is traditionally understood to imply both truth and justification, relativized knowledge can amount to two different kinds of relativism. While more moderate forms of epistemic relativism deal with relative justification, a stronger approach may include relativized truth (alethic relativism).[3]

3 Cf. Bloor 1991, p. 159.

In the work of some proponents, it's not easy to see whether a strong alethic or a more moderate epistemic relativism is advocated. The Strong Programme by Barnes and Bloor for example operates with a concept of knowledge that differs from the classical usage within a philosophical epistemological context.[4] As the notion of truth seems to have vanished in their definition of knowledge, alethic relativism might not be the result of their findings concerning relative knowledge; they occasionally seem to mean it is, however.

Because we want to examine what follows from theory-ladenness for relativism, it is also necessary to make some remarks about what is meant by 'theory' or 'theory-laden'.

Theory-laden: Brewer and Lambert define 'theory' as "all higher level forms of knowledge".[5] This is already a broad definition, but it is still too strict for our purpose. For example: If somebody wrongly holds the belief that no wolves live around his home, he might 'see' a shepherd dog, when in fact there is a wolf (despite his ability to discriminate the kinds). Although we might say that this is a case of theory-laden perception, many philosophers would not categorize such a belief as knowledge, since it isn't true. So 'knowledge' is not broad enough, but 'belief' isn't either: If the person in our example expected to see his neighbour's dog or thought of dogs at that moment for some reason, he might have also mistakenly taken the wolf to be a shepherd dog. Therefore, we should also take expectations, thoughts, attention and so on into account, too. Later in this paper, we will make use of a terminology that refers to 'top-down processes'. Here, an involvement of higher brain functions during perception might influence the outcome of perceptional processing in such a way, that this perception could reasonably be called 'theory-laden'.

Although these specifications are admittedly very permissive, they allow us to investigate perception on a very basic level, without excluding possibly involved processes up front.

4 Cf. Barnes/Bloor 1982, p. 22, footnote 5: "We refer to any collectively accepted system of belief as 'knowlege.'"

5 Brewer/Lambert 2001, p. 177.

Aims and Procedure

It is important to note that it is not necessary for the relativist to argue from theory-ladenness.[6] Therefore, an investigation of theory-ladenness cannot be able to prove relativism false. It can, however, shed light on the question of whether perception and observation are viable methods of justification for knowledge. If perception and/or observation is influenced by what we already hold true, a justification of knowledge that relies on these is prone to error at self-referentiality, and the door to epistemic relativism is open wide. If we want to secure the basis of knowledge on the common, unprejudiced ground that is our shared perception, we need to be sure that what we perceive is not influenced by what we expect to find.

Most theorists talking about theory-ladenness consider the theory-ladenness of observation. In this paper, however, we will try a different approach and consider the more fundamental concept of perception that lies beneath it. If a theory-neutral ground for perception cannot be found, justification provided by observation must take this into account.

When Hanson and Churchland discuss Gestalt-experiments (see below) or argue about scientifically trained people "seeing" different things,[7] it seems that they are not always talking about observation, but about what we call perception. The difference between observation and perception as we understand it seems to be the following: although perception is in play when we observe something, observing includes more than perceiving. Observation is attention driven and often takes place in experimental setups. This difference becomes apparent when we look at the resulting content. While observations can have contents like "the earth is moving about its own axis", the content of perception is tied to actual experiences and can not be about things like axes.

6 There are other strong arguments at his disposal like the so-called argument from norm-circularity. Cf. Boghossian 2006, Chapter 5.

7 Cf. Hanson 1969, p. 16: "The layman must learn physics before he can see what the physicist sees."

Cf. Churchland 1988, p. 176: "Most freshman physics students do memorize those laws, but relatively few have their perceptions much altered. The few who do are distinguished by having practiced the skills of applying those laws in a wide variety of circumstances."

In the following, we will investigate if and how theories in the broad sense mentioned above might have an influence on the content of our perception.[8] We will see that it makes quite a difference which kind of approach concerning the content of perception we will take. If we construe the content of our perception conceptually, theory-ladenness will be harder to avoid than with a non-conceptual understanding of this content.

To illustrate this, consider Gestalt-experiments, like the Necker-Cube, la jeune fille – la vieille femme or the duck-rabbit-head. While in fact looking at the same object, two subjects can differ not only in respect to what they say the object is, but also in respect to what they see, at least in some sense. As Hanson describes it:

> […] in Köhler's famous drawing of the Goblet-and-Faces we take the same retinal/cortical/sense-datum picture of the configuration; our drawings might be indistinguishable. I see a goblet, however, and you see two men staring at one another. Do we see the same thing? Of course we do. But again we do not.[9]

As both are looking at the same drawing, this drawing causes the same retinal/cortical/sense-datum pictures, and in this sense they do see the same thing. When asked to report what it is they see however, one report is about two men staring at each other, while the other is about a goblet. If one was expecting to see a goblet because someone told him to look at "that drawing of a goblet", his expectation of what he would find might have altered his perception.

A proponent of non-conceptual content of perception could point out that what they actually "see" is the same, it is only their reports that differ because they interpret their perception differently.[10] A proponent of conceptual content won't get away that easy. The fact that their observational reports contain different concepts suggests that these concepts are also part of – accordingly differing – experiences.

8 We will focus on visual phenomena as most examples and arguments concerned with theory-ladenness concentrate on visual experiences. But we are quite confident that whatever conclusion these investigations on vision lead to, they will also hold for experiences with other senses.

9 Hanson 1969, p. 12.

10 Hanson thinks this approach cannot be carried through. He insists that the lack of a conscious mental process shows the absence of interpretation (cf. Hanson 1969 p. 11). But 'conscious mental process' is not a good criterion for 'interpretation' as Gilbert Ryle 1949 has already shown.

There are well-known arguments for and against both positions, some of them very fundamental. While proponents of conceptual content are usually worried about how non-conceptual content could be judgment justifying content at all, proponents of non-conceptual content emphasize the limitations that concepts bring with them and that these limitations do not accord with perceptual reality (consider fineness-of-grain-arguments and belief-independency-arguments).

Because proponents of either thesis about the content of perception will have to confront arguments suggesting theory-ladenness differently, we will take the bull by the horns, split this paper twofold and consider both approaches; part 2 of this paper will thus deal with conceptual content of perception while part 3 will deal with non-conceptual content of perception.

Once we have established the extent of a possible theory-ladenness on either account of perception in the respective parts, we will follow each one up with an examination of how this influence might yield relativistic claims. A summary of our findings can be found in the fourth and final part of this paper, smartly dubbed "conclusions".

Conceptual Content of Perception

We have seen that gestalt-experiments are difficult phenomena to explain for a proponent of conceptual content of perception. Different concepts in observational reports suggest different experiences. But this is not only the case for gestalt-experiments, but for all cases of everyday perception as well.

To back up this claim, let's pick a simple example mentioned in similar style by John McDowell[11] in his essay "Avoiding the Myth".[12] Looking at the same object (a bird), Dr. Ernie sees a cardinal while Mr. Bert just sees a bird (because Mr. Bert doesn't know anything about cardinals). The content that would figure in their experience would vastly differ. While

11 In this section we will focus on John McDowell as proponent of conceptual content of perception. This is due to the fact that he is one of the most prominent proponents of this thesis in actual discourse and seems to be sensitive to theory-ladenness-problems. It might be that this sensitivity gave rise to his new approach to conceptual content of experience, which will be discussed in this paper.

12 McDowell 2009.

Dr. Ernie's is about a cardinal, Mr. Bert's is about a bird. How can we avoid that, looking at the same thing, Dr. Ernie's and Mr. Bert's experiences have different contents?

One of the sources of this 'same object – different content'-problem is the way we identify the concepts that figure in the content of experience. In our example we stipulated that Mr. Bert's content of experience contains the concept 'bird' while Dr. Ernie's contains the concept 'cardinal'. But is that necessarily so? Can't we state that Mr. Bert's and Dr. Ernie's contents of experience both contain the concept 'bird' and that Dr. Ernie only further infers that this bird is a cardinal? Although this way of arguing seems to be attractive at first sight, we have to acknowledge that, for standard uses of the verb 'infer', Dr. Ernie no more infers that it is a cardinal he is seeing than Mr. Bert that it is a bird. So both concepts, 'cardinal' and 'bird', are non-inferentially in play (where it seems that 'non-inferential' in this context means 'without conscious interpretation/inference-processes'). McDowell mentions this example to introduce a revision of major parts of his former theory of perception presented 1994 in "Mind and World".[13] He drops the idea that every concept that figures in a non-inferential content is a concept that also figures in the content of experience. Because if he drops this idea, it remains possible for Mr. Bert's and Dr. Ernie's experience to have the same content (e.g. that there is a bird) while both concepts 'bird' and 'cardinal' are non-inferentially in play. In McDowell's words:

> On my old assumption, since my experience puts me in a position to know non-inferentially that what I see is a cardinal, its content would have to include a proposition in which the concept of cardinal figures: perhaps one expressible, on the occasion, by saying "That's a cardinal". But what seems right is this: my experience makes the bird visually present to me, and my recognitional capacity enables me to know non-inferentially that what I see is a cardinal.[14]

But if we follow McDowell and don't decide which concepts figure in contents of experience by whether they are non-inferentially acquired or not, then by what criterion do we judge concepts to be constituents of experiential contents? What are contents of experience like in comparison to other non-inferentially acquired contents?

13 McDowell 1996.
14 McDowell 2009, p. 259.

McDowell characterizes contents of experiences as those that can only consist of concepts of common and proper sensibles. Or, in his words: "We should conceive experience as drawing on conceptual capacities associated with concepts of proper and common sensibles."[15] And: "The common sensibles accessible to sight are modes of space occupancy: shape, size, position, movement or its absence."[16] So neither Mr. Bert's nor Dr. Ernie's content of experience would contain concepts like 'bird' or 'cardinal'. It is difficult to say exactly what kind of content their experiences have, and maybe this is one of the motives for McDowell to revise propositional content of experience as a whole. But even if we put this difficulty aside, it should be emphasized that if this conception of experience is to protect us from theory-laden perception at all, it has to conceive these concepts of common and proper sensibles as something that all mankind shares (or at least, the main part).[17] It remains an open question where we get these shared concepts from.

So it seems that the conceptual content of experience can be conceived in a way that doesn't have theory-ladenness as a result. But most proponents of experiential conceptual content are also proponents of experiential propositional content. And it seems that this makes, again, room for considerations in favor of theory-ladenness.

Propositional content is content that could also figure in a judgment. If I judge by way of seeing that there is a pink orange in front of me, then my experience could have had the same content: It would have been the experience that there is a pink orange in front of me. Now it is quite obscure how contents of the kind that figure in judgments could just consist of concepts of proper and common sensibles. And this is not the only reason why propositional content can seem less attractive than purely conceptual content. Propositional content just picks out little information from the manifold of experience. If the content of my experience is that there is a pink orange in front of me, then my content is not about

15 McDowell 2009, p. 260.

16 McDowell 2009, p. 261.

17 It should be mentioned that McDowell himself is, in a very broad sense, a proponent of theory-ladenness of perception, while trying to grant that while looking at the same object (under the same circumstances) the content of our experience is the same. This may sound contradictory but is due to his old aim of trying to combine the spontaneity of mind with the passivity of perception in the receptivity of experience: "[…] capacities that belong to the higher cognitive faculty must be operative in experience" in McDowell 2009, p. 260.

what this orange is lying on, what form it has or what is to the left or right of it. If the manifold of experience should be part of my propositional content of experience, then it needed to consist of an infinite number of propositions. That doesn't seem very likely.

What does all this have to do with theory-ladenness? If the content of experience consists of one or maybe a few propositions, these propositions will differ with the background, especially the attention, of the perceiving subject. And if attention has an effect on the content of our experience while we are looking at the same object then our experience is theory-laden. This might not be a very strong form of theory-ladenness, because the propositions themselves are not theory-laden, only their selection is. But the manifold-to-proposition-problem is maybe reason enough to prefer purely conceptual content over conceptual and propositional content. Is the former way to conceive experiential content consistent? Can contents be conceptual without being propositional?

We already mentioned that McDowell revises experiential propositional content as a whole[18] so for him it seems to be an option to talk of conceptual contents that are not propositional. He therefore introduces a new conception of what it is for something to be conceptual. Concepts are no longer characterized as conscious and articulable, but as intuitional.[19] It is far from obvious that this notion of 'concept' fits the actual discourse about what it is to be conceptual or, even worse, that it can satisfactorily explain the opposition of experiential conceptual and experiential non-conceptual content (if we forget for a moment that McDowell says it does). But if such a broad concept of concept and a restriction of the concepts that can figure in contents of experiences to proper and common sensibles is at hand, experience can be conceived as theory-neutral.

To sum up, positions that conceive the content of experience as conceptual lead to theory-ladenness considerations. The only way to get a clear separation between observation and theory, if one wants to secure something like this, is to restrict the concepts that can figure in experiences to concepts of proper and common sensibles and at best (but this is not obligatory) to conceive the content of experience as purely conceptual (and non-propositional).

18 Cf. McDowell 2009.
19 Cf. McDowell 2009.

Conceptual Content of Perception and Epistemic Relativism

So far we have seen that only some versions of conceptual content of experience can be described as producing theory-neutral content. These versions bring various inconveniences along with them and might even turn out to be unsustainable. But if they are sustainable, they can secure a neutral and common element in our perceptions. Our resulting beliefs could still be affected by our theories. But the common element would make them commensurable, so that the requirements of justification the absolutist needs could probably be met.

Other versions of conceptual content of experience seem to produce content that is affected by the theories (knowledge, thoughts) the perceiving subject has. Does this affection lead to epistemic relativism? Surely, in some sense our perception is relative to our framework, but does that make knowledge relative?

We described two different versions of experiential content, where an affection is in play. In one version, the content of experience is theory-laden because of the differing concepts that can be in play (bird and cardinal). The other version furthermore conceived experiential content to be propositional and thereby produced content whose selection is due to theory.

To get an idea of how this theory-ladenness connects with relativism, let us start with the first version: the mysterious case of Dr. Ernie and Mr. Bert. This case isn't at all that mysterious, because their beliefs don't contradict each other and may both be categorized as (everyday-) knowledge. The question is whether this kind of theory-ladenness may cause or justify beliefs that cannot both be true at the same time. The example of Kepler and Tycho suggests it does: If their contradictory concepts of the sun (that get their meaning from their theories) are immediately in play as they are looking at it, they really see different things and are equally justified in holding their current beliefs. That means perception would fail as the theory-neutral element of scientific observation and justification of theories.

We may distinguish different situations in which one uses perception for justification in order to find the framework in play for a resulting relativism: If somebody looks at something, the beliefs arising from his

perception would be dependent upon the concepts available to that person. Justification of everyday-knowledge could therefore be seen as being relative to conceptual capacities. If that person however needs to defend his beliefs or doubts them for some reason, the experienced content has to be compared to other beliefs and its justification is therefore not only dependent upon concepts, but upon theories and beliefs that are broadly accepted by the society he is a part of, as well. On the highest level of discourse, this would lead to the kind of relativism, which claims that justification of scientific knowledge is relative to a scientific community. It is important to note that this doesn't offhand lead to alethic relativism: Truth may be absolute when justification is not. But we would still have an epistemic relativism at hand: if justification is relative, then knowledge itself is as well.

Let's turn to the second version, where experiential content consists of concepts of common and proper sensibles and is propositional. If we assume that propositions can consist of concepts of common and proper sensibles, perception would still be theory-laden, although not necessarily in a way that leads to relativism. That is because common sensibles still secure the equality of experiential content. The only thing affected by propositional content of experience would be the picking-out of the manifold of experience. If it is still possible to direct attention to the same aspects of the issue in question, nothing could be followed that would lead us to relativism.

Non-Conceptual Content

Now that we've examined arguments for theory-ladenness with conceptual content of experience, let's take a look at non-conceptual content of experience.

As Athanassios Raftopoulus and Vincent C. Müller pointed out, we have two major approaches at our disposal here.[20] The first would be a traditional phenomenological approach, while the second would be a neuroscientific one. We will stick to this classification and consider both approaches.

20 Cf. Raftopoulos & Müller 2006.

The phenomenological approach considers perceptual situations, by 'observing' whether beliefs or knowledge have an influence on perception. As these investigations are purely phenomenological, there are only few methods to 'observe' this relationship: introspection, interrogation and examination of behavior modification.

More recently, the neuroscientific approach has gained more influence. This approach enables us to take a closer look at the fundamental neurological brain-processes that take place during perception. But let's start with the phenomenological approach and look at simple situations of perception as well as cases of optical illusions.

(I) *The phenomenological approach*

In simple situations of perception, if two people see an X and both have different beliefs about what they see, one might say that they see something different. But there are also good reasons to deny the latter and only speak of different observations or associations regarding the perceived object.

Let us take a closer look at cases of optical illusions, like the bent stick under water. Here we can see that the belief or even the knowledge about the stick being in fact straight doesn't change its bent appearance. This seems to show that perception is not theory-laden.

Other cases of illusions however *do* suggest that there are altering influences on perception. If we consider the Müller-Lyer-Illusion and its research results for example, we know that the illusion only appears to those perceivers who are familiar with edges and corners.[21] By contrast, if the perceiver is a little child or has never lived in areas with many corners or edges, like on the North pole or in a desert, the illusion remains absent and the two lines appear to be of equal length. Whether such an influence on perception counts as an influence by higher-order cognitive states however, is once again controversial. Although defenders of theory-neutral perception acknowledge these cases, they don't feel the need to take these as a sign for penetrability of perception by beliefs or knowledge.

21 Cf. Ahluwalia 1978.

Raftopoulos for example mentions "[…] general, reliable regularities about the optico-spatial properties of our world hardwired in our perceptual systems".[22]

Furthermore, he states that if one takes knowledge to be affecting perception in these cases, one has to admit that this knowledge would be an unusual kind of "implicit" knowledge, that is only available "for the processing of the retinal image, whereas explicit knowledge is available for a wide range of cognitive applications".[23]

So this special kind of 'implicit knowledge' (or implicit assumption, as it might be called more correctly, because it doesn't imply truth) is not accessible to the person whose perception is affected by it and thus can't change even in the light of other opinions or knowledge that person has.

This way of defending a theory-neutral position might not be as convincing as it appears at first glance. Churchland tries to point out that the hard-wiring of assumptions into our perceptual system does not make perception itself any less theory-laden[24] – it just relocates the theory into our hardwired visual system and therefore presents the influence as a universal dogma. But then again, Raftopolous claims that it would still constitute a common ground for perception, and perception itself can thus considered to be as theory-neutral as can be.

Other often-quoted cases for theory-ladenness are those of expert perceivers. For example wine and art experts or chicken sexers[25] show extraordinary abilities to distinguish their respective objects. Based on these examples, early supporters of theory-ladenness concluded that these people learned a specific way of seeing something, which shows the penetrability of perception by knowledge or by learning.

But additional studies provided further information and explained this expertise. Although most experts cannot state exactly how they separate originals from copies, males from females and so on, these studies revealed that they have learned to detect perceptual nuances, which allow them to acknowledge characteristics of objects that others are unaware

22 Raftopoulos 2001 b, p. 189.
23 Raftopoulos 2001 b, p. 189.
24 Cf. Churchland 1988.
25 Cf. Biederman and Shiffrar 1987; Pylyshyn 1999.

of.[26] Based on these results, proponents of the impenetrability thesis deny that those experts have learned a new way of *seeing*, as these expert-abilities aren't due to additional or different visual data. Instead, those abilities kick in only *after* the perceptual system has done its part.[27] This is similar to the rabbit/duck case, only that it isn't an ambiguous, but a task[28] situation. The focus of attention highlights certain aspects of the information already presented by the visual system to identify the visual object *as* something.

(II) *The neuro-scientific approach*

With the progress of neuroscience and new research methods, especially neuroimaging technics, possibilities arose to learn more about how the brain and its internal processes work.

Nowadays, we can measure brain-responses (ERP) and generate graphical brain images (PET) of a person, while he is in a perceptual state. These methods can also be used to discover activities of so called cognitive 'top-down processes', which influence perception. The term 'top-down processes' is used to refer to all brain-processes that involve stimulation of higher brain functions, such as inductive and deductive reasoning, thinking, problem solving, other conscious and spontaneous states, like object identification and object remembrances, etc.

Additionally, we speak of higher-order cognitive influence on perception, whenever these top-down processes influence the result of the lower-order cognitive processes to such an extent, that one can't be sure whether what one sees is congruent with reality (what there is to see) – because what one sees would then be highly dependent upon what one believes.

But, alas, there is no agreement about how to interpret the collected neuroscientific data.

Some scientists speak of an encapsulated visual system called "early vision" that is not penetrable by higher-order cognitive processes,[29] others are of the opinion that there is no impenetrable visual system and that the

26 Cf. Pylyshyn 1999.
27 Cf. Pylyshyn 1999 and 1986.
28 "Task-Situation" - because the perceiver has the task to identify certain aspects.
29 Cf. Pylyshyn 1999.

occurance of top-down processes strongly suggests a modification of our visual output.[30] To shed light upon the topic, let us first say something about the disputed 'early visual system', its 'in-' and its 'output', before we turn towards some of the neuroscientific research and its results.

The 'early visual system' should be understood as a compilation of all (lower- and intermediate-level) visual processes that are responsible for processing the input and producing the output.

The term 'input' refers to data, which constitute the basis of all visual processes. Information of such a kind inherits both retinal images and information from other sources (like the vestibular system for spatial orientation). Note, however, that not everything that hits our retina counts as 'input', as the focus of attention is considered to precede the early visual system. A full statement of what exactly this 'input' is composed of, is an empirical task which remains to be accomplished.[31]

Accordingly, the term 'output' refers to the non-conceptual product of the computation of the visual input, and thus to the information that is the basis for all further cognitive processes.

Thus, if one talks about the penetrability or the impenetrability of the visual system, one argues for or against a visual system that is 'encapsulated' and thus separated from higher-order cognitive processes.

Let us now make use of this terminology and take a look at some neuroscientific research and its results.

Cases of 'visual agnosia' – a visual dysfunction - have gained a certain prominence, so let's examine these first. Patients diagnosed with visual agnosia usually show deficits like being unable to recognize familiar objects or faces and they furthermore have difficulties discriminating simple shapes. Yet, they don't suffer any intellectual loss and still perform many other visual and object-recognition tasks. For example one patient with visual agnosia could still recognize the usage or characteristics of the perceived object by focusing on different features of it, but he wasn't able to entirely and spontaneously recognize an object upon seeing it. He could, however, slowly and laboriously puzzle out what he saw by using this technique.[32] This fact led Glyn W. Humphrey and Jane Riddoch[33] as

30 Cf. Churchland 1988.
31 Cf. Howard 1982; Pylyshyn 1999.
32 Cf. Pylyshyn 1999; see also Farah 1990, Humphreys and Riddoch 1987.
33 Cf. Humphreys and Riddoch 1987.

well as other scientists[34] to the conclusion, that only after an object has been 'perceived', the recognition-process begins. In perceptual situations of unimpaired subjects these recognition processes work so fast, that it appears as if they were a part of perception.

An important contribution in this context is the argument of descending pathways[35] originally put forward by Paul Churchland.

Since cell-staining techniques revealed that "[…] descending pathways from the higher level of processing back to the earliest processing systems at the retina"[36] exist, Churchland argued that there has to be a much bigger cognitive influence on perception than just the decision which output is the most suitable in ambiguous perceptual situations, as in the case of the rabbit/duck-head.[37]

Raftopoulos however has a quite different explanation for the existence of descending pathways at hand. As recent neuroscientific experiments have shown, areas of the brain used in perception are also used for other tasks – imagery, for example.[38] Additionally, when participants of the experiment were asked to find specific patterns or objects in the images presented, these so called attention-driven tasks modulated these same areas – but only after the initial visual stimulus had been transmitted as output. The standard output of the visual system had not been changed in any way. Thus, the descending pathways must exist, if these tasks are to be executed voluntarily by higher-order cognitive states, but do not necessarily constitute a cognitive influence on perception, as might be suspected.

Non-Conceptual Content and Epistemic Relativism

Summing up, it seems easier for proponents of non-conceptual content of experience to escape the problem of theory-laden perception. As far as relativism is concerned, a common element in perception gives a neutral ground, which different theories have to adhere to. Although Churchland rejects the notion of encapsulated visual systems on the grounds of thinking of them as endorsing a universal dogma, Raftopoulos' argument

34 Cf. Farah 1990.
35 Cf. Raftopoulos 2001a.
36 Raftopoulos 2001a, p. 435.
37 Cf. Churchland 1988; Raftopoulos 2001a.
38 Raftopoulos 2001a.

about these systems still providing a common ground seems to be sound; the common element in perception seems to be (for us humans at least) retained and the danger of falling to relativism evaded.

According to Raftopoulos, the mere existence of descending pathways to the visual system does not constitute a sufficient ground to base a theory-ladenness of perception on, as it remains to be shown that these pathways have an impact on the original visual output, rather than just providing the ability for attention-driven tasks, which utilize the same areas of the brain only after the original output has been delivered and thus a common ground provided.

The problem might not have been evaded fully though; proponents of non-conceptual content of experience must still account for the problem formulated earlier, which concerns how this non-conceptual content can be judgment justifying content at all. The problem of theory-ladenness and relativism might return as soon as one tries to map the non-conceptual content of experience to a conceptual scheme – but a closer examination of this relation is outside the scope of this paper.

Conclusion

In this paper, we have examined whether a theory-neutral ground in perception could be established while considering two major approaches regarding the content of it.

In the case of perception with conceptual content, we have seen that it is quite difficult to escape the problems of theory-laden perception. As different perceivers have different concepts to employ in perception, the content of perception would vary accordingly. Possible solutions to these effects of conceptually construed perception seem to be equally difficult; McDowells theory of "common sensibles" comes to mind. How justification of knowledge is to be put down to the level of common sensibles, remains an open question. Without a solution to these problems however, perception is sure to be theory-laden and different perceivers really do perceive different things. Different beliefs would be based on subjective perceptions and thus equally justified and the absence of a theory-neutral element seems to make it impossible to escape relativism.

If perception is construed non-conceptually, these problems seem to be easier to avoid. The mere existence of descending pathways in the

wiring of our brains did not prove an influence on perception by higher order cognitive states, as the stimulation that stems from these pathways kicks in only after the initial perception has been delivered. If an encapsulated visual system is not accepted due to it amounting to no more than a hard-wired dogma, it could still prove to be the common element in perception needed in order to reject relativism – although this includes a restriction to non-conceptual perception which might cause other severe problems.[39]

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Constructed Worlds, Contested Truths

MARIA BAGHRAMIAN

The world is made up of not only natural kinds but also artefacts, stuff that we human beings, individually or communally, construct. Chairs and tables, airplanes and buildings, are our constructs; they are conceived by individuals or groups and are also built by them. But the scope of our construction is not limited to physical artefacts; the social space is also populated by our constructions. A university, to take but one example, is much more than its buildings, its very existence depends on rules, agreements, conventions, and covenants constructed and entered upon by human beings. It's our joint intention, persisting through time that gives reality to institutions of higher education and their functioning through time. This much is platitudinous and not seriously in dispute. Major philosophical debates begin firstly when we try to draw a dividing line between natural kinds and artefacts and secondly in any attempt to adjudicate on the ontological status of our social constructs.

Social constructionism, or constructivism[1], defined broadly, maintains that a diverse range of objects – among them emotions, gender, race, sex,

1 There is great deal of confusion in the terminology used in this area. Some authors present social constructionism as a sociological theory (Berger/Luckmann 1966) and constructivism as a psychological, individualistic, one (John Piaget). Boghossian uses 'constructivism' to discuss what I call 'social construction', but this is a terminological difference only and I believe the target of his criticism is what many have called 'social constructionism'.

sexual orientation, mental illness, technology, and even facts, reality, and truth – are products of explicit or implicit agreement by social actors and hence are socially constructed by them.

It has frequently been argued that social constructionism is a relativistic doctrine. In fact Paul Boghossian's book *Fear of Knowledge* is subtitled, *Against Relativism and Constructivism*. Sokal in his famous science wars attack on relativism also targets constructionist views of science. Relativism, at first blush, involves the claim that values, moral and cognitive, and even objects depend for their existence on an evaluative or ontological context, in other words, their existence is not sui generis but context-dependent, and hence relative to particular frameworks of evaluation. Once we allow the possibility of social construction in any given domain – values, norms, theories, objects, institutions, facts, etc. – then the possibility of relativising these constructs to the context and conditions of their construction arises. At least part of the reason is that, Boghossian claims,

> to say of something that it is socially constructed is to emphasize its dependence on contingent aspects of our social selves. It is to say: This thing could not have existed had we not built it; and we need not have built it at all, at least not in its present form. Had we been a different kind of society, had we had different needs, values, or interests, we might well have built a different kind of thing, or built this one differently. The inevitable contrast is with a naturally existing object, something that exists independently of us and which we did not have a hand in shaping[2].

Such contingency and dependence is an important feature of relativism, so the connection between relativism and social construction is assumed without much argument.

Beyond this rather elementary point, however, the exact relationship between these two philosophical positions has often been left unexamined.[3] This paper attempts to fill a gap in the already extensive literature on relativism by examining the relativistic consequences of different social constructionist claims. In particular, I will argue that the relationship between social constructionism and relativism is more complicated than originally assumed and that even the more radical forms of social con-

2 Boghossian 2001.

3 One exception is Paul Boghossian article 'What is Social Construction?'. I will return to this work in the course of my paper, however, it should be stated that Boghossian's very strong anti-relativism presupposes the connection between relativism and social constructionism rather than explaining it.

structionism, the ones that are the target of vocal anti-relativists such as Boghossian, do not have the relativistic consequences often attributed to them.

Claims of social construction are motivated by different philosophical considerations and take a variety of forms – ranging from the wholly unobjectionable to the wildly implausible. Their common starting point is the thought that some things come into existence by virtue of human decisions and collective intentions, but constructivists disagree on the range of the 'objects' of construction and the underlying motivation for their creation. In what follows I will explore three constructivist claims and examine their connections with relativism.

The Construction of Social Facts

Certain institutional facts uncontroversially come to exist as a consequence of agreements and decisions taken by agents acting within specific social settings. Money, newspapers, the game of chess, universities are examples of institutional facts. What is needed for the construction of social institutions and social facts is collective intentionality or 'we-intention'. Social facts come about when 'we impose rights, responsibilities, obligations, duties, privileges, entitlements, penalties, authorizations, permissions […] in order to regulate relations between people'.[4] We create a social reality when through collective actions, via our collective intentionality, we impose functions on entities that cannot perform these functions without that imposition.[5] Institutional facts presuppose human intentionality and in this they differ from brute facts which are wholly mind-independent.[6]

Searle uses the formula '*X counts as Y in context C*' to explain what counts as a social object. The necessary components of the account are:

1. Certain physical objects

4 Searle 1995, p. 100.
5 See Searle 1995, p. 41.
6 John Searle 1995 uses this terminology to distinguish between 'brute facts', which can and do exist independently of human beings and their institutions and 'institutional facts', which do not.

2. Certain cognitive acts or states in virtue of which such physical objects acquire certain special sorts of functions
3. These functions themselves
4. Contexts in which the given cognitive acts or states are effective.

1 is a requirement because institutional facts exist, so to speak, on top of brute physical facts.[7] Their existence presupposes some brute facts. 2 and 3 are crucial to the account because social institutions are primarily defined in terms of their functions and powers. For instance, money gives us the power to buy things, newspapers function as a way of disseminating up-to-date information and shaping public opinion. Social facts are not uniform in the operation of their functions. Some social facts perform their functions in virtue of the physical properties of the objects constituting them, but this is not true of an important subclass of social facts, namely so-called institutional facts. For instance, the physical properties of a piece of paper are not enough to give them the power of purchase in a market place. The piece of paper would be recognized as a bill only within the norms and constitutive rules surrounding a currency, rules that presuppose the collective intention and willingness to operate according to certain financial norms and regulations.

Social facts and their functions, according to Searle, are observer relative. A piece of metal, for instance, will count as a coin with power of purchase only relative to a particular institutional and historical context. Change the context and the piece of metal no longer has the function and power of money and hence ceases to be a token of this particular type of socially constructed object (condition 4). Without a humanly constructed and contingent context of rules, expectations and agreements, newspapers, universities or money would not exist, or may exist with a very different set of characteristics and attributes. In that sense, institutional facts are context dependent and hence relative. But such relativism is in no way pernicious. You can have absolute truths about universities or money, while accepting that the institution itself is contingently dependent on human intentions.

Searle, a Realist (with a big R) about the physical world and its brute facts, wishes to extend realism to the 'socially constructed world' with its

7 See Searle 1995, p. 35.

socially constructed facts. For a Realist, both the natural and the social facts can act as truth makers and some version of the correspondence theory of truth would be applicable to our descriptions of both the natural and the social world. Money and banks, newspapers, laws and courts are just as real in this perspective as rocks and stones but their reality is derivative, it depends on the collective intentionality of those who have brought them into existence and have sustained them through time.

Realism concerning observer relative and observer dependent facts and entities faces a number of challenges, particularly when it comes to the ontological statues of 'real' but socially constructed facts. Do these socially constructed facts supervene on the collective intentions that gave rise to them? Are they in some way reducible to the collective intentions of the participating actors? Reductionism of the latter type is favoured by many realists, but it is difficult to see how it could be achieved when the target of reduction is the dispersed and transient phenomenon of collective intentionality. Searle insists that we-intentionality cannot be reduced to the I-intentionality of the individuals responsible for creating a given social fact, but once such reduction is eschewed, the only options left are either to fall back on some form of anti-realism or to treat we-intentionality as a brute fact. Searle chooses this second alternative, but as many commentators have pointed out, the solution seems ad hoc and merely a device to retain realism about both social facts and physical facts.

Thus, although Searle's characterization of social facts in no way commits him to the views targeted by anti-relativists such as Paul Boghossian, the lack of clarity surrounding the idea of collective intention and its ontological status do not allow for straightforward realism about social reality.

Social Construction of Theories

It is common in philosophical discussions of constructionism to distinguish between the social construction of theories – ways of thinking about the world, representing, or modelling it – as opposed to the construction of objects and institutions.[8] The thought is that there is a differ-

8 See Haslanger 1995, Andreasen 1998, Hacking 1999, Mallon 2003, 2004.

ence between the construction of 'ideas' vs. the construction of 'objects',[9] or between the epistemological as opposed to the metaphysical senses of 'construction'.[10]

The point here is different from the claim made by Ron Mallon, in his *Philosophy Compass* article 'A Field Guide to Social Construction'. Mallon warns against what he sees as an unfortunate but common confusion. He says:

> Many constructionist claims that are apparently about objects can be reinterpreted as primarily about theories. This reinterpretation allows a deflationary reading of many of the most provocative constructionist claims – claims that are putatively about objects. On this deflationary reading, these claims stem from the (wilful or accidental) conflation of a theory or other representation of a thing with the thing itself. While it is quite surprising to think that putatively natural phenomena like sex or race or quarks are the result of our culture or decisions, it is not nearly as surprising to think that our theories and beliefs about these and other phenomena vary sharply from culture to culture.[11]

As an example Mallon cites Laqueur's book *Making Sex* (1990) and his claim that there is an "unstable female body" but says that upon investigation it turns out that the claim is neither about sex nor the female body but about the theories we produce and entertain regarding the female body.[12]

Although, inevitably, there is a certain degree of confusion in discussions of constructionism about facts *vs.* beliefs, I think there is a more significant philosophical point at stake here, one that arises out of conflicting philosophical intuitions and cannot simply be dismissed as a sign of conceptual confusion – a point that goes to the heart of the debate between the so-called metaphysical realists and post-Kantian anti-realists.

The thesis that our beliefs about the world and the descriptions we use

9 See, for example, Hacking 1999, pp. 21 ff.

10 Boghossian 2001 writes: "It is crucial, therefore, to distinguish between a constructionist claim that's directed at things and facts, on the one hand, and one that's directed at beliefs on the other, for they are distinct sorts of claim and require distinct forms of vindication. The first amounts to the metaphysical claim that something is real but of our own creation; the second to the epistemic claim that the correct explanation for why we have some particular belief has to do with the role that that belief plays in our social lives, and not exclusively with the evidence adduced in its favour. Each type of claim is interesting in its own way".

11 Mallon 2007, p. 96 f.

12 See Mallon 2007, p. 105 Fn. 9. Similar concerns have been expressed by Boghossian and Hacking who bemoan the careless move from the epistemological to the ontological.

to talk about it, including our scientific theories, are social constructions may at first glance appear uncontroversial. After all, it is a truism that we construct theories, for any linguistic representation about the world centrally involves the very human act of language-use. It is also true that science is a social activity and that scientists follow norms and procedures that are sanctioned by their institutional practices; in that sense, the activities of the scientific community have the imprint of their group thinking. Moreover, it is undoubtedly useful to be aware of the consensual nature of scientific practice and to take account of the connections between science and other aspects of our lives, politics and economics in particular. But none of these concessions to the sociologists of science should compel us to move from truisms about the process of scientific enquiry to the startling conclusion that *what* scientists discover or investigate are mere social constructs. However, this is not what the claims about the social construction of theories, as opposed to the social construction of facts, the topic of the next section, amount to. Although, undoubtedly, a level of confusion exists in the literature, what critics have failed to note is the underlying profound philosophical disagreement that separates the constructionists about theories from their opponents. One important feature of this disagreement is the denial of the very distinction between the epistemological and the metaphysical, a feature of the stronger forms of post-Kantian anti-realism as well as some strands of pragmatism and neo-pragmatism. The neo-pragmatist version of this philosophical orientation is best defended by Richard Rorty, for instance, when addressing the question of whether a statement such as 'dinosaurs roamed the earth' can be seen as eternally and mind-independently true. He says:

> Once you describe something as a dinosaur, its skin colour and sex life are causally independent of your having so described it. But before you describe it as a dinosaur, or anything else, there is no sense to the claim that it is "out there" having properties.[13]

Boghossian objects to this line of argument by protesting that the very idea that facts about dinosaurs are a *consequence* of scientific theorizing is absurd. Scientific theories do not make it true or false that dinosaurs existed; the causal nexus runs in the opposite direction. He admits that 'science made it true that we *came to believe* that dinosaurs and quarks

13 Rorty 1998, p. 87.

exist' but this does not mean that science made it true that dinosaurs and quarks exist because 'science cannot construct those things; at best, it can discover them'.

However, Boghossian's facile dismissal of a set of strongly held philosophical intuitions, what he calls 'Kant's discredited transcendental idealism', would not convince the many philosophers who, to varying degrees, share these intuitions. The guiding idea of Kant's transcendental idealism is that although we can and should accept the reality or existence of a mind-independent world, or 'the thing in itself', and that we can even subscribe to what Michael Devitt calls 'Fig Leaf Realism' all we can know, in any detail, is the phenomenal world or the world as represented to us through our perceptions or conceptions. What Thomas Nagel memorably called 'the view from nowhere' is not accessible to epistemic agents like us who are always and inevitably perspectival in their epistemic orientation towards that world. Once we take the Kantian philosophical intuition seriously, as many anti-realists do, it becomes easy to accept that all our claims about the world bear the imprint of the human mind and that "the trail of the human serpent is […] over everything".[14]

Nelson Goodman offers the strongest version of this neo-Kantian strain of constructivism. The root idea of Goodman's approach is a rejection of Realism with a capital R, the view that there is a ready-made world with objects and properties that are independent of our descriptions.[15] Goodman, crucially, argues that symbols have a formative function because "we are confined to ways of describing whatever is described".[16] A vast variety of versions in science and in arts, is also reflective of our insights and interests. We cannot test a version by comparing it with a world undescribed, undepicted, unperceived.[17] We can hold on to the idea of an underlying world bereft of all descriptions, depictions, etc., if we like, but on the whole, it is a world well lost. Most importantly, "we can have words without a world but no world without words or other symbols".[18]

14 James 1907, p. 64.

15 For examples of such realism see Wittgenstein in the Tractatus, some formulations of Russell's scientific realism where he talks about facts and Frege's Platonism.

16 Goodman 1978, p. 3.

17 See Goodman 1978, p. 4.

18 Goodman 1978, p. 6.

Worldmaking is carried out in many different ways. Chief among them are:

(a) Composition and decomposition: putting together and taking apart.

This primarily is a linguistic/conceptual activity for it is normally effected by the "application of labels: names, predicates, gestures, pictures, etc."[19],

(b) By giving differing weightings to the same classes present in each world, e.g. what count as relevant or irrelevant, which one is emphasised,

(c) Ordering, e.g., twelve-tone scale, vs. eight-tone scale, ordering of brightness in colour, ordering of hues,

(d) Deletion and supplementation, weeding out of some elements and adding or filling in of other elements,

(e) Deformation, which depending on point of view may be seen as correction or distortion.

Goodman allows that we require criteria for success in our world making projects as well as standards of evaluation for their varying outcomes. Truth, he admits, remains relevant to assessing those versions that have a linguistic or verbal form, but he thinks truth should not be defined as correspondence or agreement with the world. His own preferred view is a combination of coherence and epistemic accounts where

> A version is taken to be true when it offends no underlying beliefs and none of its own precepts. Among beliefs unyielding at a given time may be long-lived reflections of laws of logic, short-lived reflections of recent observations, and other convictions and prejudices ingrained with varying degrees of firmness. Among precepts, for example, may be choices among alternative frames of reference, weightings, and derivational bases.[20]

In this way, Goodman parts company with the relativists who either relativise truth and falsity to contextual factors or, following Rorty, simply

19 Goodman 1975, p. 62.
20 Goodman 1978, p. 17.

deny its significance.[21] However, Goodman's famous example of a world-making enterprise seems like a field-guide for social constructionism. He tells us:

> Now as we thus make constellations by picking out and putting together certain stars rather than others, so we make stars by drawing certain boundaries rather than others. Nothing dictates whether the skies shall be marked off into constellations or other objects. We have to make what we find, be it the Great Dipper, Sirius, food, fuel, or a stereo system.[22]

Goodman has been accused of confusing the elementary distinction between use and mention. The charge is that he confuses the fact that we make the word 'star' and we create the concept *star*, but we don't make stars. Similarly, we make true sentences such as 'Sirius is a star' but we don't make it true that Sirius is a star.[23] But this response begs the question against Goodmanian irrealism, for the very idea of the distinction between use and mention, once it's seen as something more than a mere linguistic device, presupposes the idea of a ready-made world full of stars and constellations and such like and a language separable from it, presuppositions that the anti-realists deny.

If the above is correct, then social constructionism about theories may be seen as a version of anti-realism and at best leading to conceptual relativity which relativises ontology, or what there is, to paradigms (Kuhn), theories (Quine), or concepts (Putnam). This form of relativism, however, falls well short of advocating the culture dependence of truth, rationality and knowledge. Interestingly, many conceptual relativists, Quine and Putnam in particular, have in fact argued strongly against cultural relativism and its absurd and self-defeating conclusions.

Conceptual relativism, of course, faces serious criticisms, the most prominent of which revolves around its attendant incommensurability. Donald Davidson, for instance, has famously argued against the coher-

21 Boghossian offers a number of criticisms of what he calls the 'cookie cutter' relativism of Goodman but the criticisms are effective against a reading of Goodman that make him appear even more unreasonably relativistic than he actually is. For instance, Boghossian asks how we could have created objects that predate us, but I think this criticism presupposes objectual interpretation of Goodman in ways that was not intended.

22 Goodman 1984, p. 36.

23 This criticism has been levelled by Hilary Putnam, among others, who in recent decades has been more favourably disposed towards Goodman than many other philosophers.

ence of the very idea of a conceptual scheme.[24] Briefly put, for Davidson something counts as a language, and hence a conceptual scheme or a theory, only if it is translatable. Relativism presupposes a radical divergence between alternative conceptual schemes, but Davidson makes it *a priori* impossible for languages or paradigms to be incommensurable or untranslatable. According to him, the idea of a language forever beyond our grasp is incoherent in virtue of what we mean by a system of concepts and a worldview allegedly governed by a paradigm radically different from ours will necessarily turn out to be very much like our own. Elsewhere I have argued that Davidson in fact does not succeed in his attempt to prove the incoherence of the idea of conceptual schemes.[25] The aim of this paper is not to argue against various forms of relativism but to lay bare the connections between different versions of social construction and their putative relativistic consequences.

The Social Construction of Facts

The third and strongest version of social constructivism is at times expressed as an extension of 2, hence the justified complaint by Boghossian about a possible confusion between the construction of theories and the social construction of facts. But the two versions of constructionism could and should be kept separate. In its most provocative versions, the claim is that the world as studied by scientists is itself a social construction. The view utilises anti-realist considerations, similar to those outlined above, but additionally maintains that the recognition of the constructivist features of our theories should lead us to accept that the very facts those theories purport to describe are human constructs. This brand of constructionism also highlights the social determinants of scientific practice, something that anti-realists such as Goodman and Rorty did not bring into their arguments. The contents of theories, it maintains, are determined by the self-interest of the powerful (the wealthy, the white, the male) in retaining their power. Charles Mills, for example, suggests that the borders of racial categories were decided in such a way as to "establish

24 See Davidson 1984, p. 190.
25 See Baghramian 1998, 2004.

and maintain the privileges of different groups. So, for example, the motivation for using the one-drop rule to determine black racial membership is to maintain the subordination of the products of 'miscegenation'".[26]

Other examples of this stronger form of social constructionism can be found in the work of Karin Knorr-Cetina who states: "My version of the thesis [of constructivism] has been that science secretes an unending stream of entities and relations that make up 'the world'".[27] And even more strikingly in Bruno Latour, who proposes that scientific facts are "constructed" rather than "discovered" in the laboratory and that students of science and technology must not assume a ready-made divide between the natural and the social world, and that they must give "agency" not just to humans but also to things.[28] According to Latour, the terminology of discovery will "convey the misleading impression that the presence of certain objects was a pre-given and that such objects merely awaited the timely revelation of their existence by scientists."[29] He also rejects the distinction between 'nature' and 'society'; instead, he maintains that our world is filled with "hybrids", "quasi-objects" and "networks", that is, with entities that cannot be clearly classified as either natural or social. The AIDS virus, for instance, "takes you from sex to the unconscious, then to Africa, tissue cultures, DNA and San Francisco".[30] Natural entities have "historicity" just as we do, and this is equally true of scientific experiments which should be seen as "events", for instance, once Pasteur experimented on lactic acid ferment, and the Academy accepted his results, the identity of the ferment, Pasteur and the Academy, changed forever: So "we should be able to say that not only the microbes-for-us-humans changed in the 1850's, but also the microbes-for-themselves. Their encounter with Pasteur changed them as well".[31] And adds: "We do not wish to say that facts do not exist nor that there is no such thing as reality. […] Our point is that 'out-there-ness' is the *consequence* of scientific work rather than its *cause*."[32]

26 Mills 1998, p. 48, quoted from Mallon 2007.
27 Knorr-Cetina 1993, p. 557.
28 See Latour 1979, 1993, 1999.
29 Latour 1979, pp. 128–9.
30 Latour 1993, p. 2.
31 Latour 1999, p. 146.
32 Latour 1979, p. 182.

This brand of social constructionism is motivated by the famous Quine/Duhem underdetermination thesis, to the effect that the available empirical evidence is not sufficient for determining the truth or even the probability of a scientific theory. Andrew Pickering, for instance, argues that since "choice of a theory is underdetermined by any finite set of data […] it is always possible to invent an unlimited set of theories […] capable of explaining a given set of facts".[33] This is where the scientists' judgments, as individuals and groups, make a decisive contribution to theory choice. The underlying thought is that scientific method, by itself, is not sufficient to determine theory choice. Scientists are obliged to rely on their judgments and such judgements are inevitably coloured by social, historical and personal conditions, as well as by the prevailing cultural norms and values. The thesis of underdetermination points to a logical gap between theory and evidence, the social constructionists claim that this gap is often filled by values as well as economic and political motives and interests.

One line of argument against underdetermination and its use (or overuse?) in justifying social constructionism has been proposed by Paul Boghossian. He asks:

> Is it really true that we could never have more reason to revise one of our theories rather than another in response to recalcitrant experience? Consider Duhem's example of an astronomer peering through his telescope at the heavens and being surprised at what he finds there, perhaps a hitherto undetected star in a galaxy he has been charting. Upon this discovery, according to Duhem, the astronomer may revise his theory of the heavens or he may revise his theory of how the telescope works. And rational principles of belief fixation do not tell him which to do. The idea, however, that in peering at the heavens through a telescope we are testing our theory of the telescope *just as much* as we are testing our astronomical views is absurd. The theory of the telescope has been established by numerous terrestrial experiments and fits in with an enormous number of other things that we know about lenses, light and mirrors. It is simply not plausible that, in coming across an unexpected observation of the heavens, a rational response might be to revise what we know about telescopes! The point is not that we might *never* have occasion to revise our theory of telescopes; one can certainly imagine circumstances under which that is precisely what would be called for. The point is that not *every* circumstance in which something about telescopes is presupposed is a circumstance in which our theory of telescopes is being tested, and so the conclusion that rational considerations alone cannot decide how to respond to recalcitrant experience is blocked.[34]

33 Pickering 1984, pp. 5–6.
34 Boghossian 2001, pp. 8–9.

Boghossian's rejection of the consequences of the indeterminacy argument could sound hollow to the constructionists. They would readily admit that, as a matter of current practice, Boghossian is right to claim that that 'not *every* circumstance in which something about telescopes is presupposed is a circumstance in which our theory of telescopes is being tested' but this reluctance to call into question the prevailing theoretical presuppositions is exactly the point that the constructionists wish to highlight. Our blindness to possible shortcomings in our cherished view is a symptom and not an excuse for our unwillingness to question them.

It is this third and strongest form of social constructionism that most frequently incurs the charge of relativism and is the target of anti-relativists such as Boghossian and Sokal. What is not quite clear, however, is which of the many doctrines falling under the title 'relativism' should be identified with social constructionism and why.

As Ian Hacking has pointed out, one main point of claiming *X* is a construction, is to claim that "*X* need not have existed, or need not be at all as it is. *X*, or *X* as it is at present, is not determined by the nature of things; it is not inevitable".[35] In effect, the social constructionists are claiming that a certain category of objects, theories or maybe even 'facts' are not 'inevitable'. And the idea that scientific theories as social constructions are not 'natural' or 'inevitable' became central to the so called "science wars" of the 1990s. The question, however, is how to understand this notion of evitability.

There are two guiding ideas behind this evitability thesis, firstly, as Ron Mallon puts it, the thought that theories might have been different had human cultures or decisions been different and secondly, and quite crucially, that what these theories are has as much to do with social forces, power structures, economic interests as with how things are at the level of brute facts postulated by realists. More generally, social constructivists understand science as determined by the specific, historically contingent interests and goals of the communities in which it is pursued. After all, most philosophers, sociologists and biologists nowadays accept that race is more of a cultural construct than a natural kind. Why should this not prove to be the case for other 'natural kinds'? So, a second common feature of social constructionism is the emphasis placed on phenomena

35 Hacking 1999, p. 6.

that are contingent upon human culture and human decisions. However, are these theses sufficient for establishing the frequently made claim that social constructionism is a relativistic doctrine? The answer would of course depend on what we mean by 'relativism'. Relativism, like constructionism, is a very broad church and the exact relativistic claims embodied within or implied by social constructionism are far from obvious.

Relativism is frequently offered as a resolution to the problem of contested and irresolvable claims to truth, knowledge and judgements of value. Faced with incompatible beliefs and norms, held with equal conviction, and in the absence of an overriding independent justificatory framework, we are pushed to the two extremes of scepticism (the Pyrrhonian option) or relativism (the Protagorean option). To put it slightly differently, presented with the contested pair of beliefs P and ¬P, the sceptic abandons all claims to knowledge, while the relativist accepts the truth of both by making them context-dependent. Relativism is variously expressed as an epistemic, quasi-logical or semantic doctrine. We will look at each of these doctrines in turn.

Relativism I. The epistemic thesis

Relativism is frequently expressed as a thesis about the status of our knowledge claims and our attempts at justifying them. Paul Boghossian, for instance, defines it in terms of a "doctrine of equal validity", where "there are many radically different, yet, 'equally valid' ways of knowing the world, with science just one of them".[36] According to Boghossian, constructivists call into question objectivist and realist conceptions of knowledge through the following interconnected theses, any one of which would render *Equal Validity* plausible:

(1) "The world which we seek to understand and know about is not what it is independently of us and our social context; rather, all facts are socially constructed in a way that reflects our contingent needs and interests" (*Constructivism about Facts*),

36 Boghossian 2006a, p. 2.

(2) "Facts of the form – information E justifies belief B – are not what they are independently of us and our social context; rather, all such facts are constructed in a way that reflects our contingent needs and interests" (*Constructivism about Justification*),

(3) "It is never possible to explain why we believe what we believe solely on the basis of our exposure to the relevant evidence; our contingent needs and interests must also be invoked" (*Constructivism about Rational Explanation*).[37]

Boghossian, on this occasion, runs a variety of relativist theses together – constructionism about facts, constructionism about beliefs and the contextual character of justification and brings them all under the umbrella of the equal validity view. Nevertheless, the passage still captures an essential feature of the epistemic form of relativism: epistemic relativists call into question the very possibility of unique, context independent and objective epistemic access to the world.

Relativism II. The quasi-logical thesis

As we saw above, relativism, at least since Protagoras, has been a reaction to the phenomenon of disagreement in our judgements. Faced with two equally plausible beliefs A and non-A and no decision procedures for choosing between them, we can either take the extreme option of dialetheism and embrace the contradiction, A & ¬A, or suspend belief on both options, as the Pyrrhonian sceptics recommended, or reconstruct the clash in such a way that would remove the possibility of a straightforward contradiction. Attribution of faultless disagreement to the disputants is in line with this second option.[38] A and B faultlessly disagree with each other when (1) A states P and B states its contradictory non-P, and (2) to the best of our judgement neither A nor B has made an incorrect statement.[39] One way of presenting cases of faultless disagreement is to adopt relativism about truth. Some number of philosophers in recent years have argued that the truth of a proposition is relative to a standard of assessment and that different standards of assessment may assign dif-

37 Boghossian 2006 a, pp. 22–3.
38 See e.g. Wright 2006.
39 See Kölbel 2003, Wright 2006.

ferent truth-values to the same proposition. I call this approach 'quasi-logical' because relativism is offered to resolve or dissolve the apparent conflict between seemingly contradictory beliefs or assertions.[40]

Relativism III: The Semantic Thesis

Relativism can also be construed as a claim about the semantics of certain classes of assertions. Replacement relativism, formulated by Gilbert Harman is a well-known version of this approach. The claim is that sentences that may appear to have a *monadic* truth property, such as 'the earth moves', once analysed correctly, could come to be seen as expressing relational truths of the form *x moves relative to frame of reference F*. As in cases of faultless disagreement, the rationale behind the move is to obviate the threat of a blatant contradiction by showing that the seemingly contradictory pairs of proposition, A and non-A, actually are not in logical conflict with each other. The strategy is to *replace* the non-relativised sentences with relativised ones, and to reinterpret monadic-seeming predicates, such as 'is true', with dyadic or even triadic ones such as 'true according to perspective F'. Boghossian formulates the template of replacement relativism as follows:

> Relativism about a monadic property P is the view that:
>
> (A) "x is P" expresses the proposition x is P which is true if and only if x has the monadic property expressed by "P".
> (B) Because nothing has (or can have) the property P, all such utterances are condemned to untruth.
> (C) The closest truths in the vicinity are the related relational truths of the form:
> x is P relative to F
> where "F" names some appropriate parameter.

40 Rosenkranz, for instance formulates the view this way: "P may be true relative to A's perspective while ~P is true relative to B's perspective, even though P & ~P is not true relative to any perspective. Once truth is relativized to perspectives in this way, one cannot simply infer from the fact that P and ~P cannot both be true, that A and B cannot both assert something true." (Rosenkranz 2008, p. 228).

(D) If our P-utterances are to have any prospect of being true, we should not make judgements of the form:
x is P
but only those of the form:
x is P relative to F.[41]

Which, if any, of the templates 1–3 for formulating relativism can be used to establish the relativistic credentials of strong social constructionism? I'll look at them in reverse order.

The social constructionists' claims could be seen as a species of relativism (III), if the social constructionism entailed the replacement of propositions expressing non-relational truths with those expressing relational ones. It would be useful to narrow down our discussion to one specific example, which could be used as a test case. Bruno Latour's infamous example of tuberculosis gives us a useful statement of a specific constructionist claim.

Latour, as we saw, had argued that the attribution of tuberculosis and Koch's bacillus to Ramses II is as anachronistic as claiming that his death was caused by a Marxist upheaval, or a machine gun, or a Wall Street crash[42] because 'x died of tuberculosis' (T) is true or false only within the framework of the scientific discourse where tuberculosis has an established role. Latour's claim may seem like a prime candidate for relativistic interpretation if we take it to imply that the truth or falsity of (T) depends on and hence is relative to a particular framework. Could we use 'replacement relativism' to give a correct analysis of this claim?

As I understand Latour's claim, if I understand it, it is that (T) is false, or at best indeterminate depending on how we parse out the term 'anachronistic', because the sentence 'x died of tuberculosis' gets its meaning, and hence its truth value, within a conceptual framework where the terms 'tuberculosis' and 'Koch's bacillus' have a role to play and false (or indeterminate) otherwise. Such a conceptual framework was not applicable prior to the Nineteenth Century and therefore (T) is false (or indeterminate). The closest truth in the vicinity of (T) is not so much a relational or dyadic truth but one that construes tuberculosis as an artefact that came

41 Boghossian 2006 b, p. 20–1.
42 See Latour 2000, p. 248.

into existence at a specific time because of the actions of a group of scientists. Truth remains a monadic property but its conditions of application changes to something like

> Tuberculosis came into existence, in part, through the actions of Koch.
> Ramses' death predates these actions.
> Therefore, Tuberculosis could not have been the cause of Ramses' death.

Understood in this sense, Latour can be accused of profound errors about the role of scientific discoveries and the meaning of truth evaluable sentences, but he is not making a relativistic claim about truth.

Similar considerations apply to the template used to express relativism (II). Could we construct Latour's claim as an instance of faultless disagreement? I think, once again, the answer has to be in the negative. To see this we need to take a step back and ask: What is it that the strong social constructionists argue for? There seem to be three key points involved in Latour's version of constructionism:

1. The so-called 'facts' are not inevitable
2. Facts are not different from artefacts
3. Social, political and economic interests play a major role in the construction of facts.

1–3 are open to debate on a variety of grounds but do not necessarily lead to disagreements that have even the appearance of faultlessness. Consider once again Latour's claim that 'Ramses II died of tuberculosis' is not true. The relativist would need to argue that 'Ramses II died of tuberculosis' is true at the context of utterance of those living after 19th century and false before that. Although this interpretation may seem to give a prima facie plausible explanation of the alleged relativism, it does not seem to fit with what Latour is suggesting. Once again, Latour seems to argue that we have no basis for arguing that Koch's bacteria existed before it was "discovered" and thereby it could not be implicated in Ramses' death. Once again, this is a rather crazy view but not an instance of relativism. The initial instinct that most philosophers would have is to retort,

with some annoyance, that Latour is in grip of a serious confusion and is simply failing to acknowledge the distinction between the natural kind object bacteria which was the cause of Ramses' death and the concept of bacteria that came into use when Koch discovered the organic agent he called a bacterium. But this very distinction is exactly what Latour is rejecting. His position amounts to the denial of the intelligibility of talking about bacteria as a time-less natural kind, for as he we have seen, he does not subscribe to a hard and fast distinction between natural and social kinds, nor to the distinction between what has existence independently of us and that which cannot exist without we-intentionality.

Finally, what of Boghossian's *Equal Validity* version of relativism or relativism (I)? Is social constructionism a form of relativism because of the argument that facts constructed by agents in different social contexts should be given equal credibility? Boghossian certainly thinks so. He calls the thesis of equal validity "radical and counter-intuitive" because it denies *fact-objectivity* or the common sense idea that with respect to factual questions, "there is a way things are that is independent of us and our beliefs about it."[43] According to Boghossian, in both scientific and non-scientific enquiries we privilege and defer to a "variety of techniques and methods – observation, logic, inference to the best explanation and so forth, but not tea-leaf reading or crystal ball gazing".[44] We take these methods to be the only legitimate ways of forming rational beliefs and don't give equal credence to those methods which we think acquire their inspiration from superstition. Although, like Boghossian, I subscribe to the universality as well as the essential superiority of the rational methods of enquiry, I believe that his argument in this particular case begs the question against the strong social constructionists because the notions of objectivity and mind independence are in fact the key contested ideas of this debate and therefore cannot be presupposed in establishing the incoherence of relativism or constructivism. But it is not the aim of this paper to defend the equal validity claim, which, for different reasons than Boghossian's, I too consider intellectually bankrupt. Rather, the aim is to see if we can find a match between social constructionism and any one of the more prominent versions of relativism. The problem is that

43 Boghossian 2006 a, p. 3.
44 Boghossian 2006 a, p. 4.

Boghossian's claim regarding the relativistic implications of social constructionism is far from obvious. Latour, once again used as the mouthpiece of radical constructionism, in effect, proposes a revision of the very presuppositions of science rather than attempting to relativise its truth. This revisionary position, however, does not accord equal validity to the realist and constructionist conceptions of science, rather, it denies the legitimacy of the sort of objectivist view that Boghossian defends; what Latour is trying to show, in his own words, is the "lack of scientific certainty inherent in the construction of facts. […] I intended to *emancipate* the public from a prematurely naturalized objectified fact." This emancipatory act, however, is presented as an absolute claim about science derived from empirical data. Latour grounds the constructionist thesis on observations of what happens in a laboratory and presents it as a corrective measure to what he sees as erroneous preconceptions about what scientists actually do and not as a claim about the equal validity of the objectivist and constructionist methodologies.

But maybe Boghossian has a different argument in mind and strong social constructivism, if not exactly a form of relativism, should be seen as conducive to relativism in a different sense. It is clear that the mere fact that an object is socially constructed does not render our knowledge of or beliefs about it in any sense relative. To see this, compare the study of a socially constructed bacterium to the study of objects such as stamps, which uncontroversially, owe their existence to human intentions and particular social structures (including the existence of a mail service). The 'science' of philately, which involves not merely the act of collecting stamps but actually making them objects of rigorous investigation, is not seen to give us relativised claims to knowledge merely because the objects of its investigation are socially constructed. In the same way, even if we accept that bacteria are social constructs, this would not turn a scientific investigation of them into a relativistic enterprise. Thus, something more than the mere claim that so-called 'natural kinds' are socially constructed is needed for establishing relativism or equal validity. I think the following reconstruction of the constructivist argument shows how relativism could come into the picture.

(a) Scientific activities, including theory construction, laboratory experimentation and the development of a referential apparatus for talking about theoretical entities are all, at least in part, products of social interactions and are imbued with social norms.
(b) The objects that scientists study are the products of these socially informed norm governed theoretical frameworks.
(c) Such norms can vary between different social and historical settings and hence what they produce, so called scientific facts, are relative to their social and institutional settings.

Understood in the above sense, social constructivism could be seen as making a claim of double dependency. First it embeds the theoretical and practical activities of the scientists within a potentially changing context of social norms and actions and then claims that the objects of science are produced, rather than discovered, by these activities. What is being relativised here then is not so much the constructed objects but the theories that underpin them. The crucial relativistic move here occurs in (c) with the claim that the norms used by scientists vary across different social and institutional settings. The claim is reminiscent of a famous statement by Barry Barnes and David Bloor, targeted by many anti-relativists, including Boghossian, "there is no sense attached to the idea that some standards or beliefs are really rational as distinct from merely locally accepted as such".[45] Boghossian calls this the cultural construction of reason and its relativisation, but the use of the term 'construction' in this context seems unwarranted and even misleading. To claim that standards of good or bad reasoning vary across different social settings and contexts is integral to many forms of relativism but is not necessarily a constructivist move. For constructivism to be relativistic in an interesting sense it should be distinguishable from the type of relativism that cultural anthropologists such as Edward Westermarck have been offering since the beginning of the early 20th century. In other words, the thesis should be distinguishable from the standard relativistic claim that criteria of rationality or standards of reasoning vary with social and cultural

45 Barnes/Bloor 1982, p. 27.

conditions. Conversely, in order for the charge of relativism levelled at constructionism to be more than mere name calling, then the relativism involved in constructivism should be spelled out more carefully.

I have argued against Sokal, Boghossian and other vocal anti-relativists that the social constructionism, in its various forms, does not fit readily into the models of relativism they have been targeting. It has not been my intention to defend either social constructionism or relativism. Indeed, I reject most versions of both views. Social constructionism about facts is outrageously implausible and to couple it with relativism makes an easy pray of relativistic doctrines. Relativism, I agree with Boghossian, ultimately is an unsustainable philosophical position but we do not need to reduce it to the absurdities of radical constructionism in order to show its failures. What both approaches have in common is their negation of objective and universal standards and norms for establishing the truth, rationality and reasonableness of scientific claims, but this denial of reason, although a consequence of relativism, should not be equated with it, there is more to flight from reason than claims to relativity. Bizarrely, Bruno Latour in recent years has come to express exactly the type of worry I have about the irrationalist consequences of both relativism and some versions of constructionism. So the last word should go to him and his recent recantation of constructionism. His concern grew out of the realization that the postmodernist critics of science are now finding themselves in the company of the very powers they had set out to fight, e.g. right wing politicians trying to deny global warming, as well as mad conspiracy theorists undermining the very idea of science. He now is worried that the real threat is no longer with those who believe in objectivity and facts "but from an excessive *distrust* of good matters of fact disguised as bad ideological biases".[46] "I am worried", he says,

> to detect, in those mad mixtures of knee-jerk disbelief, punctilious demands for proofs, and free use of powerful explanation from the social neverland many of the weapons of social critique. Of course conspiracy theories are an absurd deformation of our own arguments, but, like weapons smuggled through a fuzzy border to the wrong party, these are our weapons nonetheless. In spite of all the deformations, it is easy to recognize, still burnt in the steel, our trade mark[47].

46 Latour 2004, p. 227.
47 Latour 2004, p. 230.

Latour's recantation goes to the heart of the worry I have on giving up the objectivist conceptions of knowledge, truth and justification. Contrary to the arguments made popular by the post-modernists, to give up on reason is to deprive ourselves of the very possibility of effective critical engagement.[48]

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Relativism, Meaning and the New Sociology of Knowledge

HUBERT KNOBLAUCH

Introduction

As already the famous example of the first philosophers shows, the problem of the relativity of knowledge lies at the very heart of any reflection on knowledge. It seems that only since the advent of the modern sciences knowledge appears to be reliable, stable and, at least potentially, accessible to anyone. The picture of a modern society carried into the future by knowledge was not least drawn by modern sociology. In fact, sociological authors like August Comte with his 'law of three stages' are among the major propagandists of the idea that positive scientific knowledge leads to a new form of society leaving metaphysics and religion behind. Knowledge, in this sense, is tantamount to positive knowledge which accrues, builds up and, to follow the narrative of the enlightenment belief in progress, grows into an ever better future.

As the belief in progress was slowly loosing plausibility by the "dialectics of enlightenment" and as its implicit religious motivation as a secularized Christian eschatology was unveiled,[1] also the distinctive difference between scientific knowledge and other forms of knowledge was questioned. Paradoxically, the doubts in knowledge have been voiced by those who belong or belonged to the most fervent apologists of truth. Thus, Edmund Husserl, the mathematician and philosopher, who pledged for even philosophy to become a "strict science", i.e. phenomenology,[2] ended

1 Cf. Löwith 1949.
2 Cf. Husserl 1910/11.

up with realizing the many ways in which scientific knowledge lost its ground in the more basic knowledge of the "life world"[3]. "Life-world", in fact, refers to a concept of non-scientific knowledge which has been addressed in many strands of philosophy from Vico to Pascal, Herder and Nietzsche. Husserl, however, gave the notion a peculiar twist in relating it to and inserting it in science itself. As opposed to the positivist idea excluding anything as knowledge which does not come up to its standards, Husserl tried to also account for the "non-positive" and even "non-rational" forms of knowledge in science and by science; even more, he took these forms as being essentially implied in the ideas and activities of science.

As Husserl was well aware, the reflection on knowledge always implies the question on the sociality of knowledge. Even positivism, although assuming a strong relation between subjective perception and objective reality, concedes the necessity to intersubjectively "verify" knowledge. For this reason, the sociality of knowledge has been an issue in the study of science for quite some decades, and in recent years, even philosophers seem to re-discover this issue (sometimes reducing knowledge to "cognition"). The sociality of knowledge, however, has been also an issue in a line of thinking which seems to escape both, modern science studies as well as the late re-discovery of the sociality of knowledge in philosophy. Starting with Mannheim and Scheler, the sociology of knowledge already addressed the sociality of scientific knowledge. Moreover, the sociology of knowledge also related scientific knowledge to other forms of knowledge and accounted for their differences and similarities in sociological terms. Within the frame of the sociology of knowledge, the problem of relativism has been formulated in a most explicit way, and it was also the sociology of knowledge which harked back on the absolutist program of Husserl. It was particularly Alfred Schütz who drew on Husserl's phenomenology as a foundation for the social sciences and who accompanied him in his turn to the "life-world". Due to Schütz the sociology of knowledge lived on after the Second World War and became the core of the huge constructivist movement in the 1960s. The "Social Construction of Reality", the first explicit constructivist theory in the social sciences, labeled as the "new sociology of knowledge", has been written by two

3 Cf. Husserl 1939.

students of Schütz, Peter Berger and Thomas Luckmann.[4] Despite the massive and continuing influence of Schütz and social constructivism on the social sciences, the anglosaxon speaking universe of social scientific discourse obviously got the sociology of knowledge out of its mind. In the German speaking world, this tradition lingered on and was, as we shall see, renewed in a way I shall designate as "communicative constructivism".

As the sociology of knowledge and her relation to social constructivism are not part of the standard knowledge of the social sciences in the anglosaxon speaking universe of discourse, I shall shortly sketch in a very rough way the reasons why and the ways how the sociology of knowledge approached and discussed the problem of relativity. This narrative (which is partly overlapping now with my own biography) must and can remain sketchy for it builds on an impressive number of publications (and even summarizing overviews even if not always in English) on which one may draw.

In doing so, I shall, first, cover the tensions between an absolutist foundation of the sociology of knowledge and its confrontation with the critique of absolute relativism. As a consequence, second, the sociology of knowledge founded a constructivist program which tried to evade the problem of relativism by its intensive search for universals. The universalist program was importantly based on assumption on language. As Habermas' theory of communicative action demonstrates, this assumption appears too restrictive. Therefore, third, I propose a notion of communicative action as basic element of the social construction which, thence, turns into communicative construction of reality. After a short sketch of the notion of communicative action, I turn finally to its consequence for the problem of relativism.

From Relativity to Absolute Knowledge (and Back)

Although the basic concept of the sociology of knowledge may be found in Weber and his analysis of the relation between religious ideas and social groups, in Durkheim and Mauss and their analysis of the correlation between categories of time, space and logic to the structures of social or-

4 Cf. Berger/Luckmann 1966.

der and, of course, in many other attempts in sociology, the breakthrough of the sociology of knowledge came with the writings of Max Scheler and Karl Mannheim.[5] Whereas the former put the sociology of knowledge within the context of his encompassing philosophical anthropology, Karl Mannheim (1922) was to become the representative of the relativism of the sociology of knowledge. His idea that knowledge is essentially dependent on its social position, the "Standortgebundenheit" of knowing and thinking is probably the most basic formulation of the social relativity of knowledge. Although Mannheim himself tried to escape relativism by various means, his sociology of knowledge was subject to a heated debate in pre-fascist Germany, as the collection of articles by Meja and Stehr from this time demonstrates.[6] As one example, Robert Curtius, the then world leading expert on romanic literature, attacked Mannheim to represent "sociologism beyond measure".[7] The lack of any certainty, its groundlessness and its total relativity was, he augured in 1932, one of the reasons for the crisis of Germany. Although it is quite doubtful that it was the relativism and not to strive for absolutism which gave rise to German totalitarianism (as Adorno and Horkheimer argued in their "Dialectic of Enlightenment" from 1949), there is no doubt that the social relativity of knowledge was one of the major insights of Mannheim's sociology. To Mannheim however, the "Standortgebundenheit" of knowledge, i.e. its dependence on the social position, was not to result in relativism. Instead, he suggested, on the one hand, the method of "relationism" by which the different positions of knowledge are related to one another in order to achieve a total view, and, on the other hand, the "free soaring intelligentsia" as the stratum of potential researchers of knowledge without particular knowledge bias. These and other instruments served him to compensate for the fact that virtually all knowledge claims were dependent on their social functions.

As much as one may doubt that these methods can solve the question of relativism, one should not forget another strand of the sociology of knowledge which became visible in the same year of the publication of Curtius' article in 1932 – even if historical fate, Hitler seizing power the

5 For a much more detailed analysis of the history and systematic structure of the sociology of knowledge cf. Knoblauch 2005/2010.

6 Cf. Meja/Stehr 1982.

7 Cf. Curtius 1932.

other year, somehow retarded its reception. It was the book by Alfred Schütz which has been translated into English as "The Phenomenology of the Social World" in 1974. (Its original German title was literally "The meaningful constitution of the social world".) As Schütz has become, meanwhile, accepted as a classical author in sociology and one of the pillars of the interpretive turn in the social sciences (which started, one should remind younger scholars, not with Geertz but only then extended beyond sociology proper), one can justify to only roughly summarize Schütz' major argument.

Starting from Max Weber's foundation of sociology in social action, Schütz realized and criticized that the basic *definiens* of action, i.e. meaning, was left undefined by Weber himself. In his last systematic book posthumously published, Weber (1978/1921) started with defining the basic categories of sociology. For Weber, action is any meaningful behavior, as social action is behavior meaningfully oriented to others. The lack of a definition of meaning is quite more than just a minor problem, for Weber's contribution was essentially to link the interpretive tradition of historicism (oriented to meaning) to the positivist tradition of sociology and the rationalist tradition of the economic action (thence the stress on nomothetic explanation in addition to historicist ideography). As the economist Alfred Schütz clearly saw, the whole thrust of Weber's innovative approach to an interpretive sociology lay on the notion of meaning. Although Weber quite impressively demonstrated empirically how non rational meaning (e.g. of Calvinist Protestantism) entered into assumedly rational action (e.g. capitalist economic action), even in his decidedly axiomatic introduction to sociology with its many definitions he left open the very definition of meaning.

It was Schütz, then, who undertook the task of clarifying the meaning of action and social action. This would not be worth mentioning in this context were Schütz not to draw on a specific tradition of philosophy in order to clarify what is meant by knowledge: Husserl's phenomenology. Having had based his thoughts first on the philosophy of Henri Bergson, with his book published in 1932 he turned to the phenomenology of Edmund Husserl in order to solve the problem left by Weber. Husserl's phenomenology was to remain the terminological and cognitive frame in which Schütz was determined to define the notion of meaning. As mentioned above, the attempt was made before Husserl's "Crisis" book (which

appeared only in 1939). This timing made for a specific take on the problem of relativism. Like Husserl, Schütz (1945) started by considering phenomenology as a "strict science". The phenomenological method, Schütz assumed, was to remedy the historical and social relativity of meaning. It was the "ultimate foundation" of meaning in the basic structures of consciousness which promised an "Archimedan principle" to sociology.

Husserl's claim to found knowledge on the basic structure of consciousness is well known. Husserl (1960/1931) assumed that the method of phenomenological reduction of one's own experiences, described in an introspective and self-reflexive way, would yield an answer to what we can consider as knowledge or, to say it in Husserl's more exact words, how knowledge of the world is constituted by the activities of the human consciousness. Although it is well known that Husserl did not claim to result at knowledge of the world itself, which, he suggested, is bracketed by the phenomenological method, following the path of Descartes he had hoped to arrive at a "certain" source and origin of knowledge, be it the "ego pole" of consciousness as the origin of the constitutional processes or the "*petites perceptions*" (a notion he adapted from Leibniz) as the basic processual elements. This idea of an ultimate foundation is quite obviously a clear answer to the problem of relativism faced by Mannheim. Instead of the dependence of knowledge on the social, Husserl made it dependent on the subject, and, assuming basic structures of subjectivity, suggested to base the objectivity of knowledge in the experiences of the subject.

The program of phenomenology triggered a movement which did not just follow the writings of Husserl but took phenomenology as a method of research.[8] As fruitful as this movement turned out to foster the phenomenological method, it led to changes, transformations and criticism of quite a number of Husserl's original assumption which were now echoed outside phenomenology too. Particularly his attempt of providing a foundation for knowledge in the activities of the knowing subject's consciousness was subject to major revisions which, themselves, turned into fruitful approaches. On the one hand, Merleau-Ponty hinted at the essential corporeality of consciousness. Consciousness, he stressed, should not be understood as immaterial but, rather, essentially part of the body

8 Cf. Spiegelberg 1981.

as embodied consciousness. Although this critique is still today voiced by practice theories, one should not forget that this criticism must be seen rather as an extension of Husserl, for Merleau-Ponty built his analysis on Husserl's manuscripts and Husserl (1960/1931) himself had stressed the role of the body for consciousness (which is covered in the German notion of "Leib" as opposed to "Körper"). A second critique, voiced by the late Heidegger and Wittgensteinians from Peter Winch to Jürgen Habermas and, again, recent practice theoreticians, objects against the one-sided role of consciousness and argues that the presumed "phenomena" are essentially (if I may use this word here) coined by cultures and, most importantly, by certain languages as well as the way their semantics and grammar guide our ways of thinking inscribed into language.[9] These culturalist objections resulted in the strong thesis of "linguistic" and cultural relativism, e.g. the Sapir-Whorf hypothesis[10] claiming that even the basic categories of knowledge, such as time, space and logics, are dependent on the specific language and, thence, differ fundamentally between languages and cultures.[11]

And finally, it was Schütz himself who, after the adaptation of Husserl's approach in his first book, „Der sinnhafte Aufbau der sozialen Welt", contributed to the critique of Husserl and distanced himself from the absolutism of transcendental phenomenology. Already in his discussion of Husserl's concept of transcendental intersubjectivity (1960/1931), Schütz (1966) complained that Husserl had ended up in a somewhat autistic (or, in Leibniz' terms used by Husserl, "monadologic") dead end when trying to explain how an individual subjective consciousness can have access to another "alien" consciousness. Schütz concluded that Husserl failed to solve the problem of intersubjectivity. This problem of "intersubjectivity" is the phenomenological version of the "problem of social order" Parsons (1937) assumed to lie at the foundation of any sociology. It concerns the question if and how, given meaning was constituted by a subjective consciousness, this meaning could be accessed, shared or related to any other subject. While Husserl believed that the social has to be, as it were,

9 This view is essentially formulated already by Wilhelm von Humboldt's (1963/1830–33) idea of an "inner form" of thinking inscribed into every language.

10 Cf. Carroll 1997/1956.

11 Although Whorf's empirical evidences (e.g. from the Hopi) have been received very widely, they are subject to severe criticism. Cf. Knoblauch 1985.

pre-constituted in the individual consciousness (i.e. transcendental intersubjectivity), Schütz started to argue that intersubjectivity and sociality cannot be founded in or constituted by individual consciousness alone unless it would somehow duplicate the world and itself. As opposed to Husserl's view, Schütz stressed that intersubjectivity and, thus, sociality is an *empirical* fact that escapes the transcendental constitution by consciousness. The notion of "empirical" here needs to be qualified for it does not mean that consciousness as the sum of experiences has to be understood as basically social. "Empirical" means, rather, that the experience of the other cannot be reduced to the "transcendental sphere" so that the other is not a phenomenon but, instead, a matter in the mundane sphere.

Schütz "mundane" solution has consequences which have been only partly elaborated. It implies that the acceptance of the other is a requirement for any form of sociality (which, as Schütz showed, builds on processes of reciprocity).[12] It also implies that the other is being experienced in a way he himself calls "communicative". While the latter's implication will be taken up in the communicative constructivist paradigm to which I will turn below, we should focus here on the consequence of Schütz' critique on Husserl for the problem of the relativity of knowledge. To be more exact, Schütz, rather, made three inferences from the critique on Husserl's phenomenology: (a) he turned from transcendental to what he called mundane phenomenology; (b) he focused on the analysis of the general structures of the mundane sphere which he called the life-world of everyday life, and (c) he explicated a program for a new sociology of knowledge. Let me shortly explain these three inferences: (a) By mundane phenomenology Schütz tried to describe experiences "within the *epoché* of the life world", that is without performing the phenomenological reduction or *epoché*, i.e. without, as he stressed, questioning the results of transcendental phenomenology in those areas not connected to the social world (which, as he asserted, remained valid in the mundane sphere). (b) The description of mundane experiences yields, as Schütz claimed, general structures. Following Husserl's late writing, he called them "structures of the life world", i.e. the world of the lived experience. The structures of the life world, then, are what is shared in all human's ex-

12 This consequence has ethical dimensions which come close to theories of recognition, i.e. by Honneth 1996.

periences. Note that experiences here include actions (as a special form of experience) so that the connection to Weber remains pertinent. (c) The final reaction to the critique on the ultimate foundation of knowledge was the creation of a new form of the sociology of knowledge. Although Schütz referred to Scheler and Mannheim, he designed the sociology of knowledge in a specific way covering the question of how "empirically" meaning can become social, i.e. "knowledge".[13] Maintaining that all knowledge is constituted by consciousness, related to consciousness and thus accessible to phenomenological analysis, empirically, for the real human being and for the sociologist, most knowledge is, as he called it "derived from the social stock of knowledge". Most meaning orienting our actions is communicated to us by others. Particularly language is the crucial medium and "repository of knowledge" which he recognized quite early.

Universalism and the Social Construction of Reality

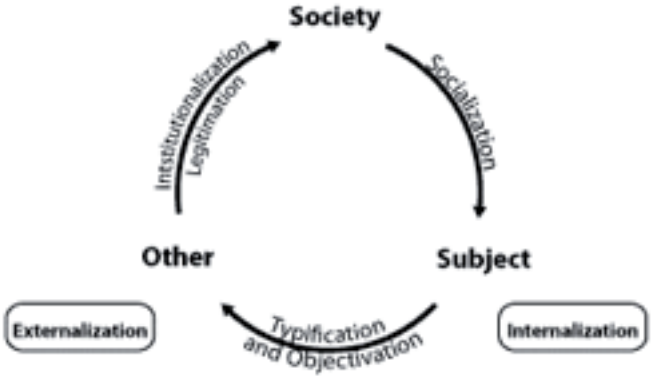
Soon after Schütz had formulated his critique of Husserl, he started an encompassing sketch of mundane phenomenology in what was to become his masterpiece, "The Structures of the Life-World". While sketching the book, he unluckily passed away, leaving a number of detailed notes.[14] It is by no means an accident that this book was finally finished by Thomas Luckmann in a congenial way (not exegetical but following the thoughts of Schütz), one of the authors who, before, had co-written "The Social Construction of Reality" with Peter L. Berger (1966). This first text on and foundation of what became known as "Constructivism" or, to be more exact, Social Constructivism,[15] follows quite clearly (and explicitly) the problems set by Schütz. In fact, the tension between the constitution of meaning by the subject and the sociality of knowledge discerned by Schütz constitutes the very principle and basic dialectical tension of

13 I must concede that neither Schütz nor later Berger and Luckmann really attempted a definition of the notion of "knowledge"; lacking any systematic study of the notion in Schütz, this is my understanding of the notion which I explicated in the second edition of my "Wissenssoziologie" in 2010.

14 These notes, "the Notizbücher", are reprinted in the first German edition of "The Structures of the Life-World II" (Schütz/Luckmann 1984).

15 As Hacking 1999 quite impressively showed, constructivism has been subject to the most varied misunderstandings, misreadings and superficial criticisms.

both, the construction of the book and of social constructivism as a theory. Quite notoriously, Berger and Luckmann framed this gap in terms of a "dialectics" between a "subjective" pole and an objective, societal pole. The former relates to the subject addressed by Weber and Schütz, i.e. conscious activities which constitute meaning of experiences, actions (as one type of experiences projected into the future) and their typifications and structures of relevancies. The objective pole, as addressed by Durkheim or Parsons, relates to the knowledge constructed socially on this "meaningful" basis as it becomes "ossified" into or "objective" as social structures, most explicitly institutions and their legitimations. Our reality is socially constructed since our knowledge – and this includes scientific knowledge as well – is socially constructed, i.e. enacted by the actions guided by knowledge. The dialectics of social construction – society is the product of actors, actors are products of society – is "synthesized", a dialectical notion not used by Berger and Luckmann, by various processes analyzed in detail and represented in the following diagram.[16]



Despite its dialectical surface,[17] the social construction of reality followed analytically the route of conciliating the constitution of knowledge and the social construction in the paths Schütz had set. As Schütz, Berger

16 I am grateful for the design of the diagram by René Tuma.

17 Berger as well as Luckmann, felt quite dissatisfied by the Hegelian and Marxian connotations of dialectics and, except from one of Berger's books, never fell back on this notion again.

and Luckmann started with an analysis of the activities of consciousness as a pre-condition for action and any form of reality for actors. Although actors "externalize meaning", the world is not constructed ad libitum, as Maturana and Varela (1991) seem to suggest in their radical constructionism. Rather, the constructions are dependent on (a) anthropological and (b) social restrictions. Ad (a): Already in his early critique of Husserl, Schütz had started to draw on anthropology and particularly the German tradition of "Philosophical Anthropology", as it was founded, among others, by Max Scheler (2009/1928).[18] Substituting the transcendental foundation of knowledge by the search for the "*conditio humana*", Philosophical Anthropology related the activities of consciousness to their potential anthropological functions, i.e. as forms of coping with environment. The relevance of this anthropological tradition is mirrored in the social construction of reality. Instead of a radical construction of everything *ab ovo*, Berger and Luckmann relate the activities of consciousness, the form of action and the construction of institutions to the 'negative anthropology' of the human body and mind, i.e. the "lack of instincts", the openness to environments, their plasticity and the "excentric positionality" of the condition humana, as Plessner (1970) framed the anthropological basis for human reflexivity. Ad (b): The deficiency of the human condition is compensated for by social institutions which substitute, in a way, culture for nature. These institutions are themselves the result of social actions and the coordination of respective meaning into (reciprocally expected) social patterns and structures of actions, i.e. institutions.

Without reconstructing the ensuing path of social construction (from the passing on of institutions to third parties, the resulting need for legitimation and the internalization of both, patterns of actions as well as their legitimations to individuals who, thus, become socialized subjects reproducing the reality constructed socially rather than restarting the construction *ab ovo*), the problem of relativity is addressed now by the new role of anthropology rather than phenomenology. Like phenomenology, philosophical anthropology was an attempt to look for the foundations of knowledge, i.e. philosophy, but it did so in the view of comparative and interdisciplinary study of the *conditio humana*. The biological, psycho-

18 The role of philosophical anthropology to Schütz has most clearly be demonstrated by Srubar (1988).

logical, sociological and cultural comparison of humans and their life-forms on an ontogenetic as well as on a phylogenetic plane would result in the discovery of general and universal features of humans including the disclosure of common structures of meaning, i.e. the life-world.

As this reference to meaning makes clear, phenomenology and the quest for the reconstruction of the subjective meaning did not disappear but changed its status. One prominent example for this transformation is the way how Luckmann (1973) integrated phenomenology into a general scientific methodology. Whereas the empirical sciences, including sociology, are considered "objective" in the sense that they collect data accessible to others, phenomenology is, to Luckmann, the science of the subjective. Given the fact that all human action is uncurably (yet not exclusively) subjective, phenomenology is needed in order to clarify this subjectivity.[19] To any science of action, therefore, phenomenology provides a preliminary clarification of its basic understanding and notions. Following the path of constructivistic theory of science, particularly Janich's "protophysic" (1985), Luckmann (1990) designated this role of phenomenology as "protosociology". Since phenomenology is an introspective method, its status differs from the "cosmological" sciences; it is less "objective" and less naïve, i.e. not accepting their "naïve realism" of the sciences, as he called it. (In this respect, the social sciences do not differ from the natural sciences.)

According to this model, phenomenology can still be considered a "foundational" method. It is now, however, juxtaposed or "related" to other methods in such a way that its inbuilt subjectivity, endangering the possible generalization of its observations, is to be corrected and complemented by two other methods: (a) the "cosmological" methods of the sciences studying the human body, on the one hand, and (b) the sciences studying the variety of human culture and social structure, on the other. The comparative physical anthropological study of the body, the comparative sociological study of cultures, and the phenomenological study of the subjective perspective could, therefore, be seen as relating to one another in a triangular way. By the notion of *triangulation* I intend to designate the method by which the findings of phenomenological introspec-

19 Schütz (1966) suggested to understand the "constitution" as a form of "clarification" of presuppositions.

tion are related to, corrected by and complementing, on the one hand, our knowledge of cultures, societies and their differences. As this comparison allows to get rid of "culture dependence" of phenomenology, the findings on the physical conditions of consciousness and culture allow for the comparison to other species.[20]

Triangulation had been no modest program. On the contrary, the correction of the structures of the life world described phenomenologically by the insights on humans and cultures were expected to yield a "mathesis universalis", the general structures of the human life world, including the common features of consciousness.[21] This claim may sound daring and overstretched today yet one should remind our postmodern and relativist contemporaries of the many attempts of "universalization" at that time, i.e. from the 1950s to the 1980s. In linguistics, for example Chomsky had laid the claim on the universal structure of language and a general structure of the mind producing it; in anthropology, Levi-Strauss laid not less daring claims on the general and universal structure of cultural knowledge while, simultaneously, the "human relations area file" program, started in 1949 at Yale University, made an attempt to collect data on all human societies, their social structures and their cultural features. Universalism was by no means a naïve and uncontested program. As mentioned above, a series of prominent researchers and philosophers questioned the assumption of universalism. The most vehement attack against universalism came from the study of language and the "linguistic relativity hypothesis" by Sapir and Whorf.[22] Following the Humboldtian theses of language and its "inner form" guiding thinking, they maintained that languages differ so basically even with respect to basic lifeworldly categories (such as time) that cultures must be conceived of as essentially thinking differently – an assumption which was well attacked by the universalists. Since they were all part of the linguistic turn, i.e. the focus on language as the major aspect of the human condition, it is no surprise that language constituted the theatre of war for almost all approaches on the relativity of knowledge.

20 One of the best examples for triangulation is Luckmann's (1979) analysis of identity which he based on a phenomenological analysis, a comparison of different cultures and the comparison to chimpanzees.

21 Cf. Luckmann 1973.

22 Cf. Gumperz/Levinson 1996.

Communicative Action and Communicative Constructivism

On these grounds, it does come to no surprise that Luckmann turned to the analysis of language and its influence on subjective orientation. The highest appreciation of language as a means of overcoming relativism, however, can be found in the work of Jürgen Habermas. In his "Theory of Communicative Action",[23] he argued that the use of language in action allows for the very possibility of a universal form of rationality. Following Apel in his transcendental philosophical assumptions about rationality, his empirical ideas about language are mainly informed by speech act theory. On the basis of the assumption that language performs three different functions (expressive, appealing, representative), he characterizes communicative action as (a) being oriented to others, (b) referring to something and (c) expressing some internal state. These three aspects parallel the "subjective", the "social" and the "objective" dimensions (or, in terms of Popper to whom he refers, "worlds") of communicative actions. Any communicative action by means of language implicitly makes certain validity claims which are related to these three aspects (truthfulness, i.e. the subjective aspect, truth, i.e. the objective aspect, and righteousness, i.e. the social aspect). The validity claim derives from the fact that communicative actions must be seen as social action. For if any (linguistic) communicative action is challenged by someone else, i.e. a next action, actors are supposed to be able to provide reasons according to the three types of validity claims inscribed in the speech act – unless they are subject to other social restrictions, such as power, social inequality etc. It is because empirically this social inequality is regularly the case that Habermas considers the validity claim as "anti-factual". Yet, provided an "ideal speech situation" with equal actors, the use of language in action would allow for communicative rationality.

Leaving aside the problem of his general theory of social action, Habermas' theory of communicative action suffers quite obviously from an enormous linguistic bias. It is, to Habermas, basically language which embodies the "power of the better argument" and, ultimately, communicative rationality. Moreover, even if he claims that language is to be considered in its use, he himself reduced the use of language to acts which

23 Cf. Habermas 1981.

follow the logocentric pattern of writing rather than those of speaking (a problem he shares with speech act theory in general). To say it in more general terms, his idea of communication is guided by logocentrism and, even more, by a model of written (or even printed) text-centeredness, ignoring not only oral speech but also other bodily forms of communication and other sign forms and codes, such as diagrams, charts, or pictures (e.g. in the case of legal or scientific evidence).

What is the consequence for the problem of relativity if we take the critique of Habermas seriously? In order to answer this question, I want, first, to ask, what happens if we abandon the idea that language entails the idea of truth, or at least that communicative action necessitates the use of language? The notion of communicative action, I want to argue, still proves useful if we extend it beyond language. In being integrated into the social constructivism, this redefinition of communicative action leads to a transformation of the "new sociology" of knowledge into "communicative constructivism" and a "communicative paradigm".[24] Since this approach is, so to say, under construction, let me first detail the basic notion of communicative action before I turn, by way of conclusion, to its consequences for the problem of relativism.

In order to avoid the reduction of communicative action to 'actions + (rather written) language', Berger and Luckmann as well as Schütz already suggest the notion of "objectivation". This notion is not only pertinent in Berger and Luckmann's (1966) work where it is the decisive form in which externalized action becomes part of the social world. (As Habermas, Berger and Luckmann then focus on language as the "most important form of objectivation"). Also Schütz (1974/1932), in his "Phenomenology of the Social World", stresses that intersubjectivity, i.e. sociality, depends on objectified meaning. While Schütz considers only those objectivations as communicative which are produced with "communicative intent" ("Kundgabeabsicht"), structuralists suggest to consider any material objectivation ("signifiant") communicative which has a reference ("signifié"), be it a letter written by hand, a sound spoken by mouth or a technical device or a visual representation on a computer screen. As semiotics has made quite clear, such objectivations must by far not be restricted to linguistic signs. Also clothes, tattoos or architectural forms can

24 Cf. Knoblauch 1995, Luckmann 1997, Knoblauch 2001, Reichertz 2009.

be considered as objectifications and even be codified into sign-systems or related to linguistic systematizations. As useful as this extension may be, as doubtful is the assumption that all these objectivations are signs, i.e. that they are embedded into systematic structures, i.e. that they are sign-systems like language. Although this may hold for the American Sign Language, it is already difficult to identify any systematic character e.g. in bodily behaviour or in pre-historic cultural objects.[25] Instead of their meaning being dependent on the structure of signs, i.e. the "system", as structuralism claims, the notion of communicative action assumes that the meaning of communication is dependent on their use, particularly on social action related to the objectivation.

In this context, the notion of objectivation as part of communicative action is intentionally ambivalent: it refers to objects as "products" of action as well as to the "production" of objectivations, i.e. the temporal process of acting as objectivation. The link between process and product is not established mysteriously. Its major reason lies in the fact that communicative action is essentially related to the body, it is a "performance".[26] Be it the articulation of a sound, the writing of a letter, the pressing of a button or, at least, a glimpse, it is the body which links any action to the world. It is because of the embodied character of communicative action that instrumentality is always part and parcel of oral, hand-written, printed or electronically mediated communicative action. Opposed to Habermas who distinguishes categorically between "instrumental" and "communicative", this notion of communicative action implies the instrumental because its corporeality has necessarily "effects" on the material world. It is by means of the body that meaning can become "socially visible". Communicative action is, as Schütz suggested, a form of "working" in the sense that it changes the material world. As communicative actions affect the common environment, they contribute to the construction of reality inasmuch as they quite literally produce objectivations, be they momentary or lasting.

25 The attempts by universalists to identify e.g. universal patterns of behaviours (cf. Ekman/Friesen 1969, Eibl-Eibesfeldt 1997) are good examples for the problems raised by the assumption of systematicity.

26 Because of the importance of the body, the notion of performance here draws on Goffman (1959/1969) and the tradition succeeding him more than on the linguistic tradition.

Yet, these "effects" in the outer world are not dependent on a kind of objective "physical" observability. Instead of being "physical" or "technical", their existence is dependent on the fact that they are experienced by others. That is to say, the criterion for objectivations is essentially social: they must be part of a "common environment"[27] to the actors participating. Someone else must be able to experience them as something the actor is experiencing too (yet not necessarily in the same, but in a typically similar way). Or, to be more exact, objectivations are due to the fact that I am experiencing someone else to experience something as something I experience (where "experience" always may be an action).[28]

Both ways, experience and acting performance presuppose the ability to experience, i.e. a subject, as well as the ability to assume the experience of someone else, i.e. intersubjectivity. The stress on action as communication related to a subject accounts for these presuppositions.[29] There is no doubt that notions as to who can be perceived by me and by others as experiencing the same as I do vary massively according to the world view, so that some people may assume (or believe) to encounter dead ancestors, angels or the Holy Spirits whereas others might reduce the world to what is positively to be described, e.g. by Carnap's "protocol sentences". The beliefs or assumptions made by the world view are included in the "meaning" of action, and it is because of the decisive relevance of these assumptions that we cannot reduce communicative action to communication or bodily performances to "behaviour". Inasmuch as the meaning of what is transmitted and "shared" socially (again by means of communicative action), this kind of meaning is referred to by the notion of "knowledge". This "knowledge" which includes the assumptions about actors, objects

27 Cf. Schütz 1974.

28 Communicative action is, therefore, always two-sided and reflexive, for the body is an object in the common environment allowing for objectivations in various modalities (visual, acoustic etc.) and, at the same time, allows to experience the common environment as well as its own objectivations in various sensational forms, visual, acoustic olfactory etc.

29 I prefer "subject" to "agency" for it better refers to the kinds of presuppositions implied in action, such as temporal continuity of experiences ("knowledge"), temporal structures, assumptions on reciprocity, all of which have been already analyzed by Schütz (1974/1932).

and actions (as well as the means of communication) constitute the subject matter of the sociology of knowledge.[30]

When referring to the performance of the body, one should remind the adult reader as to the intricate processes by which we learn to use our body in our early socialization. Be it writing with the alphabet, speaking a language or even walking upright, all these assumedly "behavioural" processes need to be learned in very long time spans. Learning to do certain things is certainly a good example for what we call action (be it induced by others, i.e. teachers, or induced by one's own "will"). While the notion of action refers to the process in which we reflexively turn to "do something" (even if what we want and if the will are dependent on knowledge, power or institutionalized discipline), the example of bodily conduct (e.g. gestures) also indicates that actions need not remain subject to our reflections. They can become part of a "habitus", an embodied knowledge. As the habitualization of conduct has inspired a vivid renaissance of the notion of "practice", the lack of any clear notion of action or subject in most theories of practice makes forget to what degree activities of consciousness are implied in "practice", such as, for example, the sedimentation of complex experience and actions into condensed forms (as analysed by Schütz 1974/1932) or the habitualization of recurrent forms of actions and the routinization of interactively coordinated habitualized actions (analysed by Berger/Luckmann 1966). When, for example, speaking, pointing or writing on a computer keyboard, we dispose of a huge range of such habitualized actions which, and this is the point, need not be reflected any more. Since we know well that the slightest problem may cast a doubt on these habits and can make us reflect, rethink the actions or even reconstruct their (right) course, it seems to me utterly misleading to refer to these habits as "unconscious". Because of the role of such habitualized processes in communication, it seems quite useful to consider communicative practices as important aspects of communicative actions.

30 Cf. Knoblauch 2005. As much as "knowledge" is transmitted, it is characterized by being adapted and adopted by the subject in order to guide their actions' orientations. Knowledge, therefore, is not an "element" but an activity; in this sense, it is opposed to what Barnes (1977, p. 2) criticizes as the "contemplative account of knowledge" that "describes knowledge as the product of isolated individuals. And it assumes that the individuals intrude minimally between reality and its representation: they apprehend reality *passively*, and, as it were, let it speak for itself […]: learning and knowledge generation are thought of in terms of visual apprehension, and verbal knowledge by analogy with pictorial representation".

As a form of social action performed by bodies, one of the major problems of communicative action consists in the coordination of different bodies. One of the solutions of this problem is, e.g., the sequential structure of conversational interaction which allows the temporal coordination of oral sounds in a way which has been intensively studied by conversation analysis.[31] Take, for example, a powerpoint presentation, a form or "genre" of communication also widely used in scientific universes of discourse.[32] Here, the body is not just an accidental feature of communication but, rather, adds to the construction of the meaning which is being discussed at those occasions which are considered to be the basic institution of scientific communication. As a form of social action, communicative action also needs to solve the problem of coordinating action's motives. One example for the synchronization of motives has been demonstrated by Schütz (1962) in his analysis of answer and question sequences. For, as he shows, answers and questions only work on the basis of an inversion of the "in-order-to motive" by one actor into a "because motive": The question posed by someone who asks in order to know something becomes the reason for the other who answers because she has been asked. Note that synchronization does not mean that actors need to have the same motives (or that they do have them reflexively) but only that motives need to correspond. Insofar as communicative actions tend to solve the problem of coordination and synchronization, they are obviously subject to institutionalization. Within the communicative framework this means that they take on certain forms with respect to the objectivations both as processes as well as products. It is these forms which stabilize the coordination of conduct and turn the synchronization of motives into fixed expectations.

The ensuing processes of institutionalization and legitimation of institutions (which has been analysed in some detail by Berger and Luckmann) provide the basis for institutional differentiation. Thus, religious experts (such as priests or prophets) are not only "institutions" but depend on certain institutionalized forms of communication (and the respective knowledge they embody) which mark religiosity (by decorum, language style, architecture etc.). Quite obviously, the differentiation of

31 Cf. Sacks/Schegloff/Jefferson 1974.
32 Cf. Knoblauch 2008.

scientific knowledge follows a similar path. Whatever the legitimatory meaning of "reason" and "reality" may be (a meaning which, as we all know, is heavily contentious within and between the various philosophies of science), one can hardly deny that science as an institutional sphere is characterized by certain forms of communicative action, involving a certain language with its specific codes, disciplinary sub-codes and translations, types of technologies and objects (i.e. the laboratory) as well as forms of actions and motives (as laid down in methodologies and theories legitimating the fields and their claims on knowledge). The importance of communicative forms for the definition of institutions is obvious even in the current process of de-differentiation: The transformation from "mode one" science organized in terms of disciplines to a "mode two" science[33] opening to the demands and requirements of non-scientific institutions (including the public) is almost tantamount to the ways how scientific forms of communication are transgressing into the forms of communication of what used to be distinct institutions, such as education, economy or politics. It is still an open question if one should describe the processes of de-differentiation as adaptation of science to other institutions, as translation or as transformation into a "knowledge society". Nevertheless, there is no doubt as to the increasing relevance of science as institution dominating the communicative forms and the legitimations of contemporary society.

Relativity, Communicative Forms and the Belief in Rationality

While I have been trying to sketch the transformation of social constructivism into communicative constructivism, we need to come back to the question as to how the problem of relativism is approached now. Let me try a tentative answer on the basis of this preliminary sketch of communicative constructivism. On the background of the increasing relevance of science in modern society, one of the most important consequences is that what counts as "knowledge" in modern society is, in principle, itself "only" a form of communication. The proliferation of "knowledge" is virtually identical with the proliferation of certain forms of communication by which "knowledge" is marked as being produced (research) or "trans-

33 Cf. Gibbons et al. 1994.

mitted". Whatever counts as legitimate or non-legitimate knowledge in a society is a form of communication – be it the art of performing the unlocking of a door, the articulation of a written text, the writing of an exam or the collaborative production of an oral exam. On these grounds rationality seems to become relative. Relative here means that it becomes dependent on the social relevance of certain forms of communication. For example the rationality of modern science, the rationality of occidental capitalism or the rationality of these words are just one, and one must add, culturally conventionalized form (i.e. genre) of communicative action, such as argumentation, narration, syllogism, calculus, seminars, lectures, experiments etc.

Let me repeat: As knowledge becomes relative to the social relevance of certain forms of communication, also "rationality" is only a title for certain communicative forms. Although all these forms are legitimated as more rational forms of knowledge and knowledge production than, e.g., a prayer, a game or a poem, they are, in this perspective, only a form of communication different from others by historically contingent reasons (that is because others had been already marked as, e.g., religious). By abandoning the universalistic assumption of linguistic approaches, does communicative constructivism, therefore, result in the relativism inherent in any constructivism?

Although I cannot offer a definite answer to this question, I would argue that it must almost logically be refuted for an answer which may be called the believe-in-rationality-thesis. The fact that any constructivism denies the possibility of accessing something real beyond the activities of those who are inquiring does not exclude the access to the processes of inquiry and construction themselves. Moreover, constructivism consists in the very attempt to reflect on its very possibility. While the transcendental solution relied on the capacity of the subject to reflect on its own preconditions and while the universalist solution allowed a form of abstract realism to be linked with constructivist principles, the notion of communicative action accounts for the very sociality of knowledge while relating it to socialized subjects. On the basis of the assumption that communicative action is objectivated, it can be made, by consequence, subject to reflection. We can point to what we see, we can talk about what we talk, we can demonstrate what we discover etc. The abilility to reflect on one's own communication is, I would argue, tantamount to what may be

called rationality: In communicating about communication we, in some way, observe the constructedness of our knowledge while, simultaneously, producing another construction. This reflexive double structure is a kind of "communicative rationality". Thus, the very fact that there are people who reflect about scientific knowledge provides for some kind of rationality of science.

As opposed to Habermas who still (even if 'counterfactually') assumes that rationality can be realized (under ideal conditions), communicative constructivism would contend that any "communication" on "communication" differs necessarily from the communication it is about. As soon as we start to, e.g., "talk about something", we construct something which is different from the something we talk about, and as soon as we "study" something, we construct something else than the "thing" we claim to study. Thus, it is no accident that the "study of science" never really catches science itself (but becomes a discipline on its own). If, however, communicative rationality cannot be realized, it is and remains "just an idea". Truth, one could say philosophically (or theologically), is transcendent to communication; sociologically one could argue that it is just one cultural "topos" of legitimation (as form of making sense of certain forms of communicative action).[34]

As a consequence, communicative rationality would be only an idea, a kind of belief inscribed in the forms of communication by which we produce rational knowledge (and legitimated by other forms of communication, such as theories of science or the kind of theory I am producing now). Rationality, in this sense, would not differ in any essential way from other beliefs (expressed in communicative forms), such as religious beliefs or everyday knowledge – except of one assumption: that communication is the medium by which we achieve a common understanding of our reality. Thus, Barnes' (this volume) suggestion to induce from the falsification of most scientific theories that all scientific may be false, presupposes that we are, at least, believing in the principle of induction. As it is only the mere belief in successful communication which founds the

34 At this point, it would not be decisive if "rationality" was an occidental form, expressed e.g. in mathematics, double entry bookkeeping or systematized musical composition, as Weber argues, or if we could identify it in different cultures (or, possibly, universally).

empirical falsification of most belief in truth and rationality, it is the reflexive insight in the relativity of knowledge which allows us to escape relativism – I believe.

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Methodological Relationism

MARTIN ENDRESS

The basic experience constituting sociology as a new scientific method is the fundamental insight that the world is not what it pretends to be. This means that everyday claims as well as scientific notions about the world and the social world as well are not to be taken for granted, they cannot be taken at face value. This experience is relativizing, and relativity therefore from the very beginning is both the constituting or generating aspect of sociology as well as one of the most important objects of research for sociology. Sociology has been one of the products of the same relativity which it tries to analyse.[1] In the history of sociology this problem of relativity as the core phenomenon of modern cultures became a problem especially within the sociology of knowledge.[2] Simultaneously it became Mannheim's central analytical problem.[3] Thus, debating "the problem of relativism in the sociology of knowledge" an analysis and discussion of the theoretical position Karl Mannheim elaborates during his Weimarian period is indispensible.

Mannheim develops a general epistemological perspective of a research methodology for the sociology of knowledge (and in this way a general sociological research strategy), and he thereby treated the problem of relativism as the core question. Due to his philosophical education and in-

1 Cf. Berger/Kellner 1981, pp. 55 f.
2 Cf. Berger/Berger/Kellner 1973 on processes of cultural pluralization in modern times.
3 Cf. Endreß 1999, 2007.

terests Mannheim's main concern was an epistemological one. This focus makes him a most valuable interlocutor as well as counterpart for philosophers. The same holds for sociologists interested in epistemological questions. Therefore it is somewhat surprising that Mannheim is referred to in this context seldomly. For example, Brian Fay in his "Contemporary philosophy of social science", published in 1996, mainly deals with the questions of particularism, perspectivism, and relativism in contemporary methodology, but he did not even mention Mannheim in his study. And if one looks at one of the most prominent anthologies in the field, the volume Martin Hollis and Steven Lukes edited on "Rationality and Relativism" nearly thirty years ago in 1982, Mannheim is mentioned only once. Barnes & Bloor here argue that Mannheim shares a "dualist and rationalist view when he contrasted the 'existential determination of thought' by 'extra-theoretical factors' with development according to 'immanent laws' derived from the 'nature of things' of 'pure logic possibilities'".[4] And they continue: "This is why he exempted the physical sciences and mathematics from his sociology of knowledge".[5] In the following, I will just give a short hint later on their approach[6] and primarily focus on Mannheim's general epistemological perspective asking whether Mannheim has anything to tell us that is of interest in this respect.

Relationism

I will try to present a three step analysis here leading to the idea of general historicity. Let me start by introducing the central thesis of my presentation: I will *neither* argue against relativism in order to strengthen rationalism, *nor* will I argue for the rationality of relativism in order to prevent it from its common criticisms. *On the contrary* I will argue for relationism as the structure of rationality in order to introduce Mannheim's answer as a third way (and to my mind the currently most plausible way) of handling the question of relativism. I would like to argue that Mannheim's general position can be systematized in the sense of *a conception of sociology as methodological relationism*. Mannheim with his thesis of an

4 Barnes/Bloor 1982, p. 26.
5 Ibid.
6 See the paper of Markus Seidel in this volume.

epistemological relationism *neither* argues for an absolute individualism *nor* does he claim or insist that there is an absolute vantage-point. Both arguments Mannheim regards as clearly contradictory and self-refuting.[7]

Mannheim, regarded from the perspective of the history of ideas, consciously takes an intermediate position between the radical position of Karl Marx, who asserts that ultimately there is a largely causal-deterministic relationship between society ("basis") and its culture ("superstructure") and, in a different sense, the likewise radical position of Max Scheler, who though conceding the societal contingency of knowledge and its thereby given relativity, does not draw the conclusion that there is in principle a total relationism, because he regards values as a priori and thereby conceives of them as essentially unaffected by societal change.[8] Mannheim takes a kind of middle position, but still quite a radical one. Even though Mannheim in a letter to Kurt H. Wolff on April 15, 1946, stated: "I want to break through the old epistemology radically but have not succeeded yet fully."[9]

Mannheim's project of a sociological analysis of knowledge introduces a distinction between the sociology of knowledge as "theory", on the one hand, and as "socio-historical research," on the other hand:

7 Cf. Mannheim himself: „Der Leistungswert wissenssoziologischer Analysen liegt also […] zwischen Sinnirrelevanz [i.e. relativism] und zwischen totaler Sinnrelevanz [i.e. absolutism], in einer bisher noch nicht gesehenen Mitte" (Mannheim 1931, p. 224) (The function of the findings of the sociology of knowledge lies somewhere in a fashion […] between irrelevance to the establishment of truth on the one hand, and entire adequacy for determining truth on the other (Mannheim 1936, p. 285)).

8 Mannheim's positioning is to be viewed not least against the background of the Weimar constellation: a) against Scheler: who is the Nestor respectively the prima donna of sociology of knowledge, b) against the sociologists-scolding in Zurich, c) against the Georgians. Mannheim operated with plain typological attributions. The conceptual polarity was obvious to him: on the one side Frankfurt sociology of critical theory: ideological criticism – society – conceptual thinking, on the other side the Georgians: myth – elitism – inner prospect. As the explicit opponents of the sociologists, he considered the former members of the circle grouped around Stefan George, the "Georgians" and their aestheticistical ideology (with exceptions, as the case of the curator of Frankfurt University Kurt Riezler shows, who had close contact with Horkheimer, Mannheim, Adolf Löwe and others and supported their appointments). A quite stable mutual repulsion: while for sociologists the Georgian worldview was an example par excellence for an ideology, for the Georgians sociology exemplarily represented the modern estranged, flattened mode of being (cf. Schivelbusch 1982, p. 15).

9 Wolff 1983, p. 203. (see Endreß 1999, p. 329).

> As theory it seeks to analyse the relationship between knowledge and existence (‚Seinsverbundenheit' des Wissens); as historical-sociological research it seeks to trace the forms which this relationship has taken in the intellectual development of mankind[10].

Defining the systematic profile of Karl Mannheim's sociology of knowledge as "a theory […] of the non-theoretical conditioning factors in knowledge"[11] "methodological relationism" first of all points to the Marxian roots of his thinking: Mannheim reverses the Marxian thesis that the ideological nature of thinking is not an unchangeable essential attribute of human reason, but rather the result of societal class structures and makes clear that the ideological nature is simply an essential attribute of human structures of thinking. This argumentation offers Mannheim the opportunity to go from a tendency to a mono-causal explanatory scheme (as it can be identified at Marx) to a basically open interpretive process of multiple relationing as constitutive for any knowledge.[12]

To Mannheim the ideological character of knowledge is an indispensable aspect of human thinking itself. The central object of his sociology of knowledge approach is to grasp and analyse the so-called "centers of systematization," that is the final and basic categorical structures of thinking essential to every human's thought within a certain milieu or socio-cultural life-world.[13] The sociology of knowledge has to identify and to uncover the reasons for the taken-for-granted character of historically specific knowledge. It has to provide an analysis of the constitution of the genesis and the validity of human knowledge, of its taken-for-grantedness forming the elementary forms of thinking and knowing within a socio-historical context. In other words: transforming the Marxian basis-superstructure-scheme Mannheim at first glance made it a relatively unspecific notion about the relationship between social structures, on the one hand, and the conditions of human life, on the other hand – basically pointing to a quasi-anthropological structure. His analysis tries to identify the socio-historical apriori; I would like to call it a weak apriori.[14]

10 Mannheim 1936, p. 264 (Mannheim 1931, p. 216).
11 Mannheim 1936, p. 264 (Mannheim 1931, p. 659, col. 1–2).
12 Cf. Schnädelbach 1996, p. 199.
13 Cf. Lenk 1984, p. 42.
14 Cf. Schnädelbach 1996.

The Relational Procedure

The best clarifications of his central concept of "relationing (relationieren)" can be found in Mannheim's article on "Sociology of Knowledge," which he wrote in 1931 for the *Handbook of Sociology*. I would like to view Mannheim's dictionary entry as his legacy to this field of research.[15] If we have a look at his writings from the article on "Historicism" (1924) to the one on the "Sociology of Knowledge" (1931), we then realize that it was due to the criticism of the contribution he presented at the 1928 congress of sociology on "Competition as a Cultural Phenomenon" and the clarifications he therefore integrated into his 1929 introductory essay on "Ideology and Utopia" in the book of the same name, that he especially focused on the problem of relativism in his article for the 1931 *Handbook of Sociology* (cf. table 1 and 2, pp. 177–81). And because this remains his most systematic contribution to the sociology of knowledge, it happens that the epistemological problem of relativism legitimately has been viewed as his dominating research interest in this field.

In this contribution we find Mannheim's concept of the sociology of knowledge in its mature, revised form, supplementing his earlier contributions. "Relationing", as "the procedure of the sociology of knowledge" is viewed, according to this text, as relating "individual ideas to the total structure of a given historico-social subject": "Relationism," Mannheim points out, "does insist […] that it lies in the nature of […] assertions that they cannot be formulated absolutely, but only in terms of the perspective of a given situation"[16] (i.e., in standpoint-bound aspect-structures). As Mannheim puts it in his paradigmatical thesis: "Relationism, as we use it, states that every assertion can only be relationally formulated. It becomes relativism only when it is linked with the older static ideal of external, unperspectivistic truths independent of the subjective experience of the observer, and when it is judged by this alien ideal of absolute truth".[17] Once again Mannheim in a formulation of 1929:

> Relationism signifies merely that all the elements of meaning in a given situation have reference to one another and derive their significance from this reciprocal

15 Following Kurt H. Wolff, "this is not only Mannheim's last statement on his conception of the sociology of knowledge …, but also his last paper in the area generally" (1993: 63 f.).

16 Mannheim 1936, p. 283 (Mannheim 1931, p. 666/223, col. 2).

17 Mannheim 1936, p. 300 (Mannheim 1931, p. 674/231, col. 1).

> interrelationship in a given frame of thought. Such a system of meanings is possible and valid only in a given type of historical existence, to which, for a time, it furnishes appropriate expression[18].

Thus "the relational procedure," for example, relates certain opinions "to a certain mode of interpreting the world which, in turn, is ultimately related to a certain social structure which constitutes its situation".[19] Mannheim thereby understands "perspective" (I would prefer to speak of aspect-structures (Aspektstrukturen)) as "the manner in *which* one views an object, *what* one perceives in it, and *how* one construes it in his thinking".[20] This definition of the "formal determination of thinking" is of systematic importance, because Mannheim here differentiates three aspects of his concept of aspect-structures: "which" = perspectivity, "what" = selectivity, "how" = constructivity. This conceptual triad can be summarized under the headline of historicity, i.e. relationality.

Furthermore, this definition refers to both the "formal determination of thinking" and to "qualitative elements in the structure of thought" like, *first*, the

> analysis of the meaning of the concepts being used; the phenomenon of the counter-concept; the absence of certain concepts; [*second*] the structure of the categorical apparatus; [*third*] dominant modes of thought; [*fourth*] level of abstraction; and [*fifth*] the ontology that is presupposed[21].

Mannheim tries to exemplify the analysis of these qualitative elements in the structure of thought, i.e. of aspect-structures, by pointing out:[22]

First: that "the concepts in their concrete contents diverge from one another in accordance with differing social positions" (e.g., concept of freedom; conservative as freedom to, i.e. positive; liberal as freedom of, i.e. negative);

Second: that "the basic categories of thought may likewise differ" (morphological categories used by conservatives; analytical methods used by liberals);

18 Mannheim 1936, p. 86. The closeness and the familiarity of Mannheim's epistemological perspective with the structuralist approach in the analysis of language as introduced by Ferdinand de Saussure (1905/10) is obvious here.
19 Mannheim 1936, p. 282 (Mannheim 1931, p. 666/223, col. 2).
20 Mannheim 1936, p. 272 (Mannheim 1931, p. 662/219, col. 2). Italics are mine.
21 Mannheim 1936, p. 272 (Mannheim 1931, pp. 662 f./219 f.).
22 Mannheim 1936: 273–9; Mannheim 1931: 663 ff./220 ff.

Third: that "the model that is implicitly in the mind of a person when he proceeds to reflect about an object" differs (e.g., forms of orientation towards the world, modes of life referring to classes, generations, status groups, sects, occupational groups, schools and so on). The typical difference is the one between organic or holistic models of thought and individualistic models of thought;

Fourth: that characteristics of perspectives are to be found "by investigating the level of abstraction, beyond which a given theory does not progress, or the degree to which it resists theoretical, systematic formulation", i.e., "the approach to the problem, the level on which the problem happens to be formulated, the stage of abstraction and the stage of concreteness that one hopes to attain, are all and in the same way bound up with social existence". Mannheim's example is Marx as "an observer whose view is bound up with a given social position" due to which he "never succeed[s] in singling out the more general and theoretical aspects which are implicit in the concrete observations that he makes", that is "the fundamental findings of the sociology of knowledge concerning the relationship between human thought and the conditions of existence in general";[23]

Fifth: that "it is dangerous to approach [the] problems [of a 'basic ontology'] naïvely, without first taking into account the results suggested by the sociology of knowledge". This fifth point directly addresses Heidegger's 'fundamental-ontology', which I will leave aside here.

As a determination of facticity in this sense, the *first process* of "*relationing*" for Mannheim constitutes the first step of every analysis in the sociology of knowledge.[24]

Historicity

This first process is followed by the *second process* of "*particularization*". With this supplementation, made in his 1931 dictionary entry, Mannheim unfolds the systematic meaning of his aforementioned formulation, used in *Ideology and Utopia*, that a relational system is "possible and valid only

23 Mannheim 1936, p. 277.

24 Thus we have to distinguish between the epistemological problem of relationism, which for Mannheim is of secondary interest, and the process of relationing as the primary research strategy of the sociology of knowledge.

in a given type of historical existence".[25] It is valid because in the "particularizing process" not only attribution as such is achieved, but here a "limitation of validity," a restriction of "its claim to validity"[26] occurs. This means that the mere determination of the facticity of a fixed standpoint is "transcended" insofar as the domain of validity of the thereby referred to aspect-structure is named: the genetic analysis is supplemented by a reflexion of "theoretical validity" (geltungstheoretische Reflexion). Insight into the constitutive particular character of knowledge does not lead to relativism because it is only valid in this way. Like Husserl, Mannheim connects the question of origin with that of validity here. Therefore, it becomes clear that Mannheim is arguing against the claims for validity, on the one hand, and stating a certain type of validity, on the other hand. We might call this type particular validity.

This second process is followed by a *third process* of "*neutralization* of situational determination".[27] Since at the first stage this process implies a tendency which "neutralizes the various conflicting points of view", at the second stage it "creates out of this neutralization a more comprehensive and serviceable basis of vision".[28] According to Mannheim, this is possible because here "a higher degree of abstractions" as well as an "increasing degree to formalize the phenomena" is reached. As Mannheim himself tries to explain it:

> Rather the neutralizations of the qualitative differences in the varying points of view, arising in certain definite situations, result in a scheme of orientation which allows only certain formal and structural components of the phenomena to emerge into the foreground of experience and thought[29].

As far as I see, it never has been noticed that Mannheim in order to illustrate his strategy of research explains it by referring to everyday conventions ("the rules of etiquette and social intercourse") as well as to everyday processes of typification. As for the latter I would like to quote Mannheim in more length:

25 Mannheim 1936, p. 86.
26 Mannheim 1936, p. 284 (Mannheim 1931, p. 667/224, col. 1).
27 Mannheim 1936, p. 302 (Mannheim 1931, p. 674/231, col. 2).
28 Mannheim 1936, p. 302 (Mannheim 1931, pp. 674 f./231 f.): „schafft aus dieser Neutralisierung eine umfassendere, tragfähigere Sichtbasis".
29 Mannheim 1936, p. 304 (Mannheim 1931, p. 675/232, col. 2).

> The qualitative understanding of the mutual relationship, which is formalized to such an extent that it becomes a 'formal sociological category' indicating, so to speak, is only the specific role of the relationship. The other party is regarded merely as an 'ambassador,' 'stranger,' or 'train conductor'. […] The formalization in such cases is itself an expression of certain social situations, and the direction which formalization takes […] is dependent on the social situation, which enters, even though in a diluted form, into the categories that we use[30].

Thus, Mannheim in the sense of the program formulated by Ernst Troeltsch "[to] overcome history through history and [thus to] level the foundation for a new creation",[31] argues with historicism against historicism. His attempt to solve the problem of relativity leads him beyond an awareness of the fundamental total relativity of *all* knowledge to a reflected relationism.[32]

Speaking from a historical point of view the problem of relativity is not a new one. It can be seen as the result of historism (not: historicism). Historism in general emerges between 1790 and 1830. It is to be associated with historians like Leopold von Ranke, Jacob Burckhardt, Johann G. Droysen, Wilhelm Dilthey; even though the term "historism" became fashionable at the turn of the 20th century in a polemic voice. Following the historical-systematic analysis of Herbert Schnädelbach one has to differentiate three types of historism:[33]

A *first type* of historism can be called the positivism of the humanities (Geisteswissenschaften), cultivating a tendency of escalating collection and listing of historical data as scientific style. This tendency is best called in German 'Faktenhuberei', i.e. anorak or wonk, and includes both a type of research typical for a special form of historical research as well as a tendency of making philosophy a type of philology that Schnädelbach once called "morbus hermeneuticus".[34] A *second type* of historism is to be identified as a philosophical position arguing for a general historical relativism. Finally, a *third type* of historism is viewing all socio-cultural phenomena as historic ones. Ernst Troeltsch, for example, is arguing for

30 Mannheim 1936, pp. 304 f. (Mannheim 1931, p. 676/233, col. 1).
31 Troeltsch 1922, p. 772.
32 According to Lenk a „Versuch einer Überwindung des historischen Relativismus durch den Nachweis seiner eigenen geschichtlichen Bedingtheit" (an attempt to overcome historical relativism by certifying its own historical relativity) (Lenk 1984, p. 41).
33 Cf. Schnädelbach 1983, pp. 51 ff. See also Schnädelbach 1974.
34 Cf. Schnädelbach 1981.

a process of a "principal historization of all human thinking about man, culture, and values (grundsätzlichen Historisierung alles unseres Denkens über den Menschen, seine Kultur und seine Werte)".[35] Thus, historism 1 as well as historism 2 are forms of degeneration or symptoms of decline of the third type of historism. Historism 3 serves a type of enlightenment. It is this third type of analysis Mannheim is striving for in his sociology of knowledge approach.[36]

Mannheim's account therefore can be called radical because he regards all forms of knowledge – thus also the knowledge in the natural sciences (Mannheim mentions "quantum theory"), as he states in his 1931 legacy,[37] as well – as in principle accessible to a sociological analysis on the basis of his research hypothesis of a universal existential connectedness of all knowledge („einer generellen Seinsverbundenheit allen Wissens"). Here his 1931's contribution radicalizes his former position excluding the natural sciences and esp. mathematical knowledge from his approach. One should point out that again Mannheim seems to be at least ambivalent in this point. But while he once states that his analysis holds for "certain assertions",[38] this contradicts passages where he indeed much more frequently says that his analysis holds for "every" type of knowledge.[39]

Mannheim's analyses are concentrated on the dynamics of knowledge. Mannheim is not only interested in the historical dynamics of sociocultural processes and processes of vertical mobility[40] due to which the problem of a sociology of knowledge may arise, but also in the "changes"

35 Troeltsch 1922, p. 102.

36 Also biographically it makes little sense to attribute a relativistic position to Mannheim, because it was one of the central items on the program of the "Free School of Humanities", founded out of the Sunday Circle in 1917/18, to oppose to "relativistic impressionism" (Karádi/Vezér 1985, p. 12). And even if the program itself was not formulated by Mannheim, yet he was one of its major figures and in their second semester in February 1918 gave the program lecture "Soul and Culture". In the other case it would have to be successfully found early Mannheim's demarcations to the program or a clear break in his thinking from his Heidelberg years (see also the dependence on Neo-Kantianism and Logos- movement and position of methodological pluralism vs. Monism of science, cf. Karadi/Vezér 1985, pp. 12–14).

37 Cf. Mannheim 1936, pp. 305 f. (Mannheim 1931, p. 676/233, col. 1–2).

38 Cf. Mannheim 1936, p. 283 (Mannheim 1931, p. 666/223, col. 2).

39 Cf. Mannheim 1936, pp. 290, 291, 296, 300, 306 (Mannheim 1931, pp. 669/226, col. 2, 670/227, col. 2, 672/229, col. 2, 674/231, col. 1, 676/233, col. 2).

40 Cf. Mannheim 1936, p. 7.

in everyday knowledge itself.[41] As he generally said: "The meaning of history and life is contained in their becoming and in their flux".[42] And Mannheim himself is aware of his historical position in time, which makes it impossible to bridge the socio-culturally created gap between living and thinking. He speaks about "the rise of a particular new attitude of consciousness"[43] (das Aufkommen einer spezifisch neuen Bewußtseinshaltung) in modern times. This attitude, leading to a type of "experimental life" (experimentelles Leben) implies a "distancing from life" (Lebensdistanzierung). And this process will take place historically, if the "unambiguity of orientation of meaning in life disintegrates"[44] (wenn die Eindeutigkeit der Bedeutungsrichtung des Lebens zerfallen ist), which, according to Mannheim, occurs in the socio-historical process when the religious world has been destroyed.[45]

Catharsis

It was Mannheim with his concept of relationism, inspired by the sociology of knowledge, who first succeeded in avoiding the methodological problem of relativism by clarifying the logical paradox of non-contradictoriness (Nichtwidersprüchlichkeit) as well as the self-refuting structure (sich selbst widerlegende Struktur) of a radical or absolute relativism.

In Mannheim's work an epistemological orientation seems to dominate,[46] which is prominently focused on the problem of relativism. Mannheim's main concern was an epistemological one, but he is not only interested in the problem of relativism: We have to differentiate two steps of his analysis, as presented, for example, in his *Ideology and Utopia*. There he distinguishes the "uncovering", the "revelation" (Enthül-

41 Cf. Mannheim 1936, pp. 3, 7.
42 Cf. Mannheim 1936, p. 26.
43 Cf. Mannheim 1930, p. [6].
44 Cf. Mannheim 1930, p. [15].
45 Cf. Mannheim 1930, p. [16]. For further elaboration of this point see Endreß on historicity (Endreß 2001: esp. pp. 78 ff.).
46 Even though this cannot be said in Mannheimian terms, because to him epistemological reflections are "oriented within the polarity of object and subject" (Mannheim 1936, p. 13). Despite this classical exposition, Mannheim himself understands the structure of "self-clarification (Selbstklärung)" as a dialectic of subject and object (Mannheim 1936, p. 49).

lung) of the situational determination (Seinsgebundenheit) and therefore ideological character (Ideologiehaftigkeit) of all knowledge [as an (anthropological) structural phenomenon] from a "possible" second and "additional" analysis of its connection with an included "epistemological attitude" (erkenntnistheoretische Haltung).[47]

Focusing his work on everyday knowledge, as well as by continuing his earlier studies on "conjunctive thinking," Mannheim intends to criticize the claims for general validity by pointing to the existential or situational boundness or determination (Seinsgebundenheit) of *all* knowledge. But while it is his aim to present a critique of our everyday knowledge, at the same time he tries to illustrate his analytical strategy using examples that refer exactly to this type of knowledge. A most prominent example is especially the proverbial "peasant boy" (Bauernjunge), with reference to whom Mannheim tries to explain the processes of "the acquisition of perspective," the processes of distancing (Distanzierungsprozesse) shifting from "the mode of thinking and speaking characteristic of [his] village […] gradually to [those characteristic of] city life".[48]

Thus, the process of revealing the concealing potentiality of certain cognitive styles (das Aufdecken des Verdeckungspotentials von Denkformen) is Mannheim's primary account, and only further investigations lead him to "the problem of what constitutes reliable knowledge"[49] (Eingehen auf die Wahrheitsproblematik): a problem, which first of all opens up the issue of relativism and relationism.[50]

With regard to the question of the status of everyday knowledge, Mannheim's approach should be seen as a critique of everyday knowledge. As Mannheim puts it in *Ideology and Utopia*, which appeared in 1929: "If, therefore, we are to rise to the demands put upon us by the need for analyzing modern thought, we must see to it that a sociological

47 Cf. Mannheim 1936, p. 78.
48 Cf. Mannheim 1936, pp. 281, 284 (Mannheim 1931, pp. 666/223, col. 1 f., 667/224, col. 1).
49 Cf. Mannheim 1936, p. 284 (Mannheim 1931, p. 667/224, col. 1)
50 See also Mannheim 1922/25: 178 [197]: "The problem of relativism, as it has become the question of our life, can only be mastered if we make it into the axis, the starting point for theory, and only afterwards ask how it could be overcome at the stage at which it confronts us" (Das Problem des Relativismus, wie es für uns heute Lebensfrage geworden ist, ist nur überwindbar, wenn wir [es] zur Achse, zum Ausgangspunkt der Theorie machen und erst nachträglich fragen, wie man [es] auf der Stufe, auf der [es] uns entgegentritt, überwinden könnte).

history of ideas concerns itself with the actual thought of society"[51] (das faktische Denken des Menschen). And Mannheim confirms this aim of his analysis in the new first chapter written for the English edition of this book in 1936, when he states that it is "the so-called pre-scientific inexact mode of thought" he intends to describe and analyse.[52]

Mannheim – following the classical idea of enlightenment – intends to present an analytical *catharsis of thinking*. He argues for a correlation of the processes of self-control, self-correction, self-illumination, self-knowledge (Selbstkontrolle, Selbstkorrektur, Selbstdurchleuchtung, Selbsterkenntnis) and thus self-extension (Selbsterweiterung) or self-clarification (Selbstklärung) by individuals and groups, on the one hand, and, one can say, of world-clarification (Weltklärung), on the other hand.[53] Sociology of knowledge as a form of critique tries to show the situational determination of thinking. Because this analysis is valid for *all* knowledge, for *all* thinking, it is also valid for everyday knowledge and everyday thinking.

Since Mannheim tries to show the existential boundedness of knowledge, his approach can be called a (hermeneutical) "deconstruction" of group-constituted knowledge. Focusing on the intersubjective constitution of meaning and knowledge, Mannheim argues that "knowledge is from the very beginning a co-operative process of group life".[54]

At this point we are in need of a clarification of the term "constitution". Mannheim himself understands his analysis, i.e., the analytical strategy of his conception of the sociology of knowledge as contrary to the analysis of ideologies as a "constitutional" one.[55] It seems that this theory of constitution turns out to be an analysis of the structural conditions of all types of knowledge. The theory or analysis of "structural conditions" is understood here as an analysis uncovering those attributes of human interactions that are not at anyone's disposal. It means to talk about conditions which constitute human's empirical reference scheme of knowledge.

51 Mannheim 1936, p. 73.
52 Cf. Mannheim 1936, p. 2.
53 Cf. Mannheim 1936, pp. 47–9.
54 Cf. e.g. Mannheim 1936, pp. 28, 29.
55 Cf. Mannheim 1936, pp. 266, 271, 279 (Mannheim 1931, pp. 660/217, col. 1, 662/219, col. 1, 665/222, col. 1). The English translation of this article reads here: "structural or noological level" (266), "essential significance" (271), and "constitute an essential part" (279).

Insofar as sociological theory is concerned with an analysis of constitution, i.e., the analysis of the origin, of the genesis of social knowledge, it must always rely on particular constitutional acts. Mannheim here especially refers to the interpretation processes of certain carrier groups (Deutungsprozesse oder -akte von Trägergruppen).

As far as this process of catharsis is a permanent one, Mannheim renews an argument at this stage of his analysis, which he first mentions in his 1924 study on "Historism". Talking about the general phenomenon of perspectivity, Mannheim in his earlier paper made clear that any "reproach of relativism" (Vorwurf des Relativismus) consequently implies "a doctrine of the ahistorical character of reason"[56] (eine Lehre von der Überzeitlichkeit der Vernunft). For systematic reasons this hint has to be interpreted as a *quasi-transcendental rejection of relativism*.[57] Criticizing the implication of a possible universal validity as accompanying necessarily any assertion of the relativistic character of a theory of perspectivity in general, clarifies the performative contradiction of any critique of relativism. Mannheim's sociology of knowledge-approach provides us with an argument guiding a transcendental reflection of the conditions of relational claims of validity.[58]

Methodological Architecture of Relationism

We are now prepared for final reflections on the architecture of Mannheim's sociology of knowledge. As stated before, Mannheim's conception of the sociology of knowledge consists of two parts:

First he thinks of it as a kind of *theory*. In this respect it is read here, i.e., as far as the sociology of knowledge is a "theory concerning the significance of the extra-theoretical conditioning factors in knowledge"[59] (Theorie über die Bedeutung der außertheoretischen Bedingungen des Wissens), it has to be viewed as an analysis of what might best be called the structural conditions of knowledge.

56 Cf. Mannheim 1924, pp. 253 f., 301.
57 Cf. Endreß 1999, esp. pp. 332 f., 341 ff.
58 See Endreß 1999, p. 341 for a clarification of Mannheim's three-digit-position: a comparison of the two notions "x in context A is y" and "x in context B is z" combines both the external particular validity of these statements as well as their internal absolute validity.
59 Cf. Mannheim 1931, p. 659/216.

Second, the sociology of knowledge, for Mannheim, is a r*esearch strategy*, putting emphasis on the processes of "sociological imputation" (soziologische Zurechnung) by identifying certain carrier groups of knowledge and focusing on particular "social forces" (treibende Sozialkräfte).[60] This understanding of his sociology of knowledge-approach as reflecting sociological conditions of knowledge, is of much more importance to us.

To put it more precisely: On the basis of a general presupposition of *distancing* knowledge thus far is taken-for-granted, and in completion of the first step of *relationing* as relating "individual ideas to the total structure of a given historico-social subject",[61] Mannheim's conception of the sociology of knowledge as a research strategy, consists of *three further steps*: on a *second step*, called "horizontal analysis" (Horizontalanalyse), the basic structure of a cognitive style must be analysed. This is the so-called static analysis. On the *third step*, called "vertical analysis" (Vertikalanalyse), the direction of the development of a style of thinking has to be analysed. This phase is the first dynamic one. On a *fourth step*, the "sociological imputation" (soziologische Zurechnung) takes place, identifying carrier groups (Trägergruppen) for certain strata of knowledge.[62] Insofar as this analysis also is concerned with the variability of the relation between knowledge and carrier groups, this phase leads to a second dynamic one.

In summing up, we are confronted with some kind of shuttle service between the qualification and the assignment of knowledge. Thus according to the first level of Mannheim's conceptual framework, giving the grounding for the following, he basically is concerned with the structural aspects of cognitive styles. As he points out in his 1930 lecture: "Sociological analysis always has to reach the structural and try to grasp it"[63] (Die soziologische Analyse muß immer auf das Strukturelle zugehen und es zu erfassen versuchen). This first level of analysis is concerned with anthropological structural phenomena, while the second one analyses the "situational boundedness" or "situational determination" of knowledge in detail. It analyses the concrete, the specific relational character of all knowledge. And this relationality is due to certain aspect-structures.

60 Cf. Mannheim 1936, pp. 307 f. (Mannheim 1931, p. 677/234).
61 Mannheim 1936, p. 283 (Mannheim 1931, p. 666/223, col. 2).
62 Cf. Mannheim 1936, pp. 308 f. (Mannheim 1931, p. 677/234, col. 2).
63 Mannheim 1930, p. [14].

The central object of research in the sociology of knowledge is for Mannheim "conceptually grasping the respective systematization centers, i.e., those last categorical structural units in which all thinking individuals of necessity participate in a cultural circle".[64] We can summarize the core of his research interest in the formula: The sociology of knowledge should uncover the reasons why specific knowledge appears to be self-evident. Thereby we are dealing, in the case of Mannheim's sociology of knowledge, with a specific form of analysis of structural conditions. For Mannheim, the analysis of structural conditions aims at revealing the aspect-structures of thinking within an ongoing two-level analysis, because insofar as "every epoch has its fundamentally new approach and […] sees […] from a new perspective [aspect-structure (Aspektstruktur)], […] the historico-social process is of *essential* (i.e., *constitutive*) significance".[65] Thereby the phenomenon of "situational determination" [existential boundness (Seinsverbundenheit)] is "an inherent factor [Konstituens] in […] the theory of relationism".[66] Thus Mannheim's main concern is an analysis of the structural conditions of all types of knowledge, as I said before.

It is Mannheim's interest to identify structurally necessary, indispensable impregnations of knowledge (strukturell-notwendige Imprägnierungen des Wissens). This analytical interest refers to his concept of total ideology (totaler Ideologiebegriff), i.e., his point of reference is the "mental structure in its totality" (Denkstruktur in ihrer Totalität). His analyses try to criticize thinking, as he puts it, "on the structural or noological level"[67] (auf der Konstitutionsebene, auf der noologischen Ebene).

Even though, we might add, Mannheim also asks about the conditions for the constitution of the validity of aspect-structures, whereas "validity" in this context stands for the meaning structure as taken-for-granted. And his answer here is that it is due to carrier groups – even though this conception is not really elaborated in his writings: neither in *Ideology and Utopia* (1929) nor in his article on the sociology of knowledge (1931) and in his 1936 added part "Preliminary approach to the problem".

64 Lenk 1984, p. 42.
65 Mannheim 1936, p. 271 (Mannheim 1931, p. 672/219, col. 1).
66 Mannheim 1936, p. 305 (Mannheim 1931, p. 676/233, col. 1).
67 Cf. Mannheim 1936, p. 266 (Mannheim 1931, p. 660/217, col. 1).

Concluding Remarks

The position of a methodological relationism elaborated in Mannheim's case acquires central significance not only for his self-understanding and his sociology, but can also be regarded as a constitutive principle for sociology in general.

Mannheim faces the problem of relativism in the more general light of the question of socio-cultural and philosophical pluralism. Concerning this level of reflexivity it becomes clear that consequently we have to distinguish two different forms of consequences of pluralism: *first: relativism* in contrast to absolutism, totalitarianism, and fanatism; and *second: relationism* as opposing to monism and universalism. And it is the latter which leads us to a culture of compromise, a culture of tolerance, and a culture of reciprocity – even in epistemic cultures.

The leading background thesis even of these problems is what I use to call the 'structural ambivalence of modernity' oscillating between heterogeneity (i.e. secularization and pluralization, see, e.g., M. Weber, P. L. Berger), on the one hand, and homogeneity (i.e. nationalization and globalization, see, e.g., E. Gellner, A. Giddens), on the other hand. While reflecting processes of standardization of culture, of language, and of modes of education, these questions demonstrate the intimate relationship of problems of the sociology of knowledge with those of the sociology of domination.

Thus Mannheim legitimately cannot be called a radical or absolut relativist. To put it in classical philosophical terms: while Protagoras' homomensura-doctrine "Man is the measure of all things" (Der Mensch ist das Maß aller Dinge) argues for the individual human as being the only relevant criterion for truth, Mannheim argues differently by providing a two-step-answer: it is first of all the individual within its socio-cultural milieu and, secondly, we have to reflect this relation in the wider context of history. We therefore have to be aware of a *double socio-historical apriori*. A statement is apriori only in relation, i.e. something is apriori only in reference to something else. Therefore, it cannot be absolute as such, and the reproach of relativism does not make any sense. The problem of relativism is due to its confusion with the logic of relationism. Mannheim's analysis helps us to clarify this point.

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Tab. 1: Content and structure of Mannheim's contribution "Wissenssoziologie" – "Sociology of Knowledge" (1931)

Index Original Article 1931	Page no. 1931	Page no. reprint 1982	Page no. *Ideologie und Utopie* 1952	Index English translation *Ideology and Utopia* 1936	Page no. 1936
	659	216	227	1. The Nature and scope of the sociology of knowledge	264
I. A. Definition und Gliederung der Wissenssoziologie	659	216	227	1. (a) Definition and subdivisions of the sociology of knowledge	264
I. B. Wissenssoziologie und Ideologielehre	659	216	227	1. (b) The sociology of knowledge and the theory of ideology	265
II. Die beiden Teile der Wissenssoziologie	660	217	229	2. The two divisions of the sociology of knowledge	266
II. A. Die Wissenssoziologie als Theorie von der Seinsverbundenheit des Wissens	660	217	229	2. (a) The theory of the social determination of knowledge	266
II. A. 1. Die Lehre von der Faktizität der Seinsverbundenheit	660	217			
II. A. 1 a. Sozialprozesse, die den Erkenntnisprozess lenken	661	218			
II. A. 1 b. Das konstitutive Hineinragen des Sozialprozesses in die Aspektstruktur	662	219			
II. A. 2. Die Struktur und der Leistungscharakter der Wissenssoziologie	665	222			
II. A. 2 a. Die spezielle Bewegungsart, die in der Wissenssoziologie enthalten ist	665	222			

[Continued on next page]

Index Original Article 1931	Page no. 1931	Page no. reprint 1982	Page no. *Ideologie und Utopie* 1952	Index English translation *Ideology and Utopia* 1936	Page no. 1936
II. A. 2 b. Distanzierungsprozesse als Voraussetzung für die Wissenssoziologie	666	223			
II. A. 2 c. Das Phänomen des Relationierens	666	223			
II. A. 2 d. Das Phänomen des Partikularisierens	666	223			
II. B. Die erkenntnistheoretischen Konsequenzen der Wissenssoziologie	667	224	245	2. (b) The epistemological consequences of the sociology of knowledge	286
II. B. 1. Kritischer Teil	668	225			
II. B. 1 a. Erkenntnistheorie und Einzelwissenschaft	668	225			
II. B. 1 b. Aufweis der Partikularität des dominierenden erkenntnistheoretischen Ansatzes	670	227	249	3. The demonstration of the partial nature of traditional epistemology	290
II. B. 1 b. α Die Orientiertheit am exakt-naturwissenschaftlichen Denkparadigma	670	227	249	3. (a) The orientation towards Natural Science as a model of thought	290
II. B. 1 b. β Zusammenhang zwischen Wahrheitsbegriff und sozialhistorischer „Seinslage“	670	227	250	3. (b) The relationship between criteria of truth and the social-historical situation	291
II. B. 2. Positiver Teil	670	227	251	4. The positive role of the sociology of knowledge	292
II. B. 2 a. Revision der These, dass die Genesis unter allen Umständen geltungsirrelevant sei	670	227			
II. B. 2 b. Weitere Konsequenzen des wissenssoziologischen Pro-blemansatzes für die Erkenntnistheorie	671	228			

[Continued on next page]

Index Original Article 1931	Page no. 1931	Page no. reprint 1982	Page no. *Ideologie und Utopie* 1952	Index English translation *Ideology and Utopia* 1936	Page no. 1936
II.B. 2b. α Entdeckung des aktiven Elementes, das im Erkennen steckt	671	228			
II.B. 2b. β Das Konstitutiv-Perspektivische in bestimmten Erkenntnissen	672	229			
II.B. 2b. γ Das Problematische an der Konstruktion einer Sphäre des „Geltens an sich“	672	229			
II.B. 2c. Die beiden Wege der Erkenntnistheorie	674	231			
III. Arbeitstechnische Probleme des historisch-soziologischen Forschens im Gebiete der Wissenssoziologie	676	233	263	5. Problems of technique in historical sociological research in the field of the sociology of knowledge	306
IV. Kurzer Überblick über die Geschichte der Wissenssoziologie	678	235	266	6. Brief survey of the history of the sociology of knowledge	309

Tab. 2: Basic structure of Mannheim's conception of the sociology of knowledge as method of research

Objective system	Explanation	Mannheim's alternative denomination	Objective meaning
Distanzieren	Voraussetzung wissenssoziologischer Forschungen		
Relationieren (1982: 223: col. 1 bottom)	Faktizitätsfestellung; d.h. Freilegung standort-gebundener Aspektstrukturen; Naturwissenschaften hier nicht ausgenommen		Geltungseinschränkung, -relativierung 1
Partikularisieren (1982: 223: col. 2)	Bestimmung der Grundstruktur eines Denkstils (statisch): partielle Geltungszurechnung; d.h. vs. Wissenssoziologie als α) Destruktion jeden Geltungsanspruches und β) Irrelevanz der Genesis einer Aussage; statt dessen: γ) pro strukturelle Einkreisung von Geltungsansprüchen	Horizontalanalyse (1982: 231: col.1 top): d.h. Genesis und Geltung (1982: 227: col.2 bottom) d.h. konstitutive Perspektivität (1982: 229: col.2 middle) d.h. Relationismus (1982: 231: col.1)	Geltungszuweisung, -erklärung 1 (Resumee: 1982: 224: col.2 top, 225: col.1 middle, 226: col.2 middle, 227: col.2 top)

[Continued on next page]

Objective system	Explanation	Mannheim's alternative denomination	Objective meaning
Neutralisieren (1982: 231: col. 2 bottom)	Bestimmung der Entwicklungsrichtung eines Denkstils (dynamisch): Selbsterweiterung: Spiralmodell (1982: 232: col. 2 bottom)	Vertikalanalyse (1982: 231: col. 2 middle)	Geltungseinschränkung, -relativierung 2
Zurechnen	Bestimmung der Trägergruppen eines Denkstils	Soziologische Zurechnung (1982: 234: col. 1 top) (1982: 234: col. 2 middle)	Geltungszuweisung, -erklärung 2

Geltungszuweisung, -erklärung, i. e. Erklärung des Geltungswertes einer Aussage

Karl Mannheim, Relativism and Knowledge in the Natural Sciences – A Deviant Interpretation

MARKUS SEIDEL

If the title of a paper advertises that the author aims to provide a *deviant interpretation* of one aspect of the position of an important figure in a discipline – here Karl Mannheim – the first question of the reader is, or should be: "What, then, is the standard interpretation?". In preparing this paper, I noticed that it is far from easy to give a comprehensive answer to this first question without discussing some of the historical background of the discussion of relativism in the sociology of knowledge. It is only from this background that it is possible to notice why Mannheim's contribution was unique and led to the first vigorous attacks on the *epistemological* and especially *relativistic* implications of the sociology of knowledge. Furthermore, it is only with a rough understanding of Mannheim's position in the history of sociology of knowledge that it is possible to see why the idea of my paper – to understand Mannheim's attitude towards the sociological investigation of the natural sciences – is interesting in any case. Therefore, I will start with a very short historical sketch of the discussion at the beginning of the last century and of the expansion of it since that time. Setting Mannheim's contribution in context, we can then have a look on the interpretation of his stance on relativism. I will not concentrate on Mannheim's attempt to reject the reproach to hold a form of self-defeating relativism and to establish an acceptable form of – what he calls – relationism. My specific interest will be in Mannheim's position towards knowledge in the natural sciences

and mathematics – as Mannheim himself calls them 'the exact sciences'.[1] I will try to get some order in the interpretations such that it is easier to see which one to reject, which one to attenuate and which one to accept. Finally, I will show that *most* interpretations by friend and foe are, at best, misleading and *some* are astonishingly misguided. This will be done by trying to understand Mannheim's sociology of knowledge in the context of the intellectual background at his time with categories and concepts of current theoretical philosophy in the analytic tradition. This approach is unorthodox but, I hope, will prove fruitful.

Before I will start with my interpretation, however, I want to emphasize the restricted scope of my argument. It has to be noticed that this paper is *interpretative* and – as it should be – it is a *charitable* interpretation I am aiming at. If at points it seems as if I am *defending* Mannheim's position of relativism or relationism, be sure that this is a wrong impression. My own position concerning the relativism debate in the sociology of knowledge can be summarized in the following way: First of all, I think that *alethic* relativism – that is relativism about truth – is not really at issue in the relativism debate in the sociology of knowledge. What is at stake is *epistemic* relativism – that is relativism about justification. The latter does not necessarily imply the former. Secondly, I think that epistemic relativism in the sociology of knowledge – recall, relativism regarding *justification*, not necessarily also relativism about truth – can be traced back generally to two ideas. The first is a basically Kantian thought concerning the epistemic inaccessibility of the world-in-itself. You can find this idea, I believe, in such supposedly distinct approaches like Mannheim's, Thomas Kuhn's and also the Strong Programme's. The second is an argument that has been called 'argument from norm-circularity' by Paul Boghossian and which roughly consists of Agrippa's trilemma with an additional premise that includes an incommensurability thesis. Since I do not think that the world-in-itself is epistemically inaccessible and also do not think that an incommensurability thesis that would be required in the argument from norm-circularity can be sustained, it follows that I do not see how epistemic relativism can be defended. Furthermore, I do not

1 From now on I will restrict my analysis just to the natural sciences and will not discuss mathematics. It is an interesting, interpretative question whether Mannheim sees any difference between these areas of thought, but I will not dwell on this issue.

see how relativism concerning justification can escape *pragmatic* – not logical – self-refutation. However, simply forget about all that – just have in my mind that my *charitable interpretation* does not coincide with my own *position*.

Let us start then with setting Mannheim in historical context.

Mannheim in the Historical Context of the Sociology of Knowledge

Taking a historical stance towards the question of relativism in the sociology of knowledge demands discussion especially of *German* sociology of knowledge at the beginning of the last century – though, undeniably, a *comprehensive* historical introduction would require to mention such important predecessors as Francis Bacon, Karl Marx and Emile Durkheim to name just a few. In order to understand Mannheim's contribution and, especially, the strong opposition to his sociology of knowledge take the following quote from Ludwig Gumplowicz, one of the founding fathers of sociology, taken from his *Grundriß der Soziologie*:

> The major error of individualistic psychology is that *the person* thinks. From this error results the eternal quest after the *source* of thinking inside of the *individual* […]. This is a chain of errors. Since, first of all, it is not the person but its social community that is thinking. The source of thinking does not lie inside of the person but in the social environment, the person is living in, in the social atmosphere, in which it is breathing *and it cannot think differently* from the way it results with necessity from the influences of the social environment that concentrate in his brain.[2]

Notice, that Gumplowicz emphasizes that the *source* of individual thinking lies in the social environment of the individual. The individual cannot think differently, since it is located in and influenced by the community it is living in. Importantly, Gumplowicz does *not* say explicitly whether it is just *the way* the individual thinks that is influenced by the social environment or whether the source also of the *contents and validity* of knowledge of individual thinking is the society. The question of much epistemological ado about the sociology of knowledge is exactly this: How far is knowledge and thinking influenced or determined by social factors and, respectively, what exactly is the potential area of investigation of sociological investigations?

2 Gumplowicz 1905, p. 268 (my translation).

One of the founding fathers of the sociology of knowledge, Max Scheler, answers this question by distinguishing between so-called *ideal factors* (Idealfaktoren) and *real factors* (Realfaktoren). The real factors are, for example, political and economical circumstances; roughly what we can regard as 'social structure'. The ideal factors are values and ideas, for example as we find in religion or in science. Scheler believes that the main task of the sociology of knowledge is to investigate the relations between these two realms of factors. Though both realms of factors are, according to Scheler, causally independent of each other the ideal factors need to be realized by the real factors. The ideal factors, themselves, have no social force; the real factors determine which of the ideas and values is in fact realized in a historical situation. The sociology of knowledge is thus confined to investigate these processes and not the ideas and values themselves. This becomes clear in a much quoted statement by Scheler. He thinks quite in accord with Gumplowicz that the "sociological character of all knowledge, of all forms of thought, intuition and cognition is unquestionable".[3] However, he goes on to clarify that "although the content and even less the objective validity of all knowledge is not determined by the controlling perspectives of social interests, nevertheless this is the case with the selection of the objects of knowledge."[4] For Scheler, the content and validity of knowledge are out of reach of the investigation by the sociology of knowledge. How the contents are selected to become the objects of knowledge is the proper area of sociological research.

With this thought of Scheler in mind we can come to the position of Karl Mannheim. Also Mannheim clearly attacks the dominance of an individualistic bias in epistemology and approves the generally social character of knowledge in quite the same manner as Gumplowicz and Scheler. In the introduction to his *Ideology and Utopia* he claims:

> We will not succeed in attaining an adequate psychology and theory of knowledge as a whole as long as our epistemology fails, from the very beginning, to recognize the social character of knowing, and fails to regard individualized thinking only as an exceptional instance.[5]

3 Scheler 1975, p. 44 (translation by Merton 1973, p. 23).
4 Scheler 1975, pp. 44 f. (translation by Merton 1973, pp. 23).
5 Mannheim 1946, p. 29.

However, though Mannheim and Scheler share the conviction of the general social character of knowledge, Mannheim rejects Scheler's clear separation of ideal and real factors.[6] For Mannheim, the relation of values (ideal factors) and social structure (real factors) is dynamic:

> In one word, as soon as we abandon the platonizing conception, the phenomenological difference of the real and ideal factors will be subordinated to the genetic unity of the historic process, and we shall advance to the point of origin where a real factor is *converted* into a mental datum.[7]

Whereas Scheler could exempt the contents and the validity of knowledge from sociological investigations, since the sociological question just concerned how the ideal factors are *realized*, Mannheim thinks that the real *and* ideal factors are part of a historical and genetical process that can be investigated from the sociological point of view. Therefore, for Mannheim the area of investigation of the sociology of knowledge also comprises the *contents* and the *validity* of this very knowledge.

We see now, why the question of relativism in the sociology of knowledge becomes urgent with respect to Mannheim's programme: Whereas Scheler's ideal factors are not influenced by the real factors, Mannheim cannot separate strictly between the *genesis* of a belief and its *validity* – no wonder that he aims at a "revision of the thesis that the genesis of a proposition is under all circumstances irrelevant to its validity".[8] He does, however, try to circumvent a "relativism in the sense of one assertion being as good as another".[9] Mannheim's attempt to do so is complex and it would take another paper to explain it fully. Nevertheless, the basic idea is that the view Mannheim calls *relationism* just "states that every assertion can only be relationally formulated".[10] According to Mannheim, this view becomes relativism only when judged on the background of an "older static ideal of eternal, unperspectivistic truths independent of the subjective experience of the observer".[11] In fact, Mannheim's proposed solution

6 Cf. Remmling 1975, p. 40.
7 Mannheim 1952 a, p. 162.
8 Mannheim 1946, pp. 262 f. (improved translation).
9 Mannheim 1946, p. 270.
10 Mannheim 1946, p. 270.
11 Mannheim 1946, p. 270, cf. also Mannheim 1952 b, p. 194.

is – as he says more than once – the revision of traditional epistemology[12] and he believes that, once we judge his theoretical approach in the light of such a revised epistemology, there will be no problem of relativism anymore.[13] I will not comment extensively on this attempt to solve the problem of relativism. However, it has to be noted that Mannheim's attempt at points appears to have the following, rather trivial structure: There will be no problem with relativism once you accept relativistic epistemology. Such a truism, of course, provides no argument to accept a relativistic epistemology in the first place and can be maintained for virtually any position – including absolutism – on analogous lines. Let us, however, not dwell on Mannheim's proposed solution.

We have seen that Mannheim expands the area of investigation of the sociology of knowledge. It is requested to do research in all those areas in which we can find, as he calls it, 'existential determination of knowledge and thought' (*Seinsverbundenheit/Seinsgebundenheit des Wissens und Denkens*).[14] And Mannheim gives us criteria for these areas that clearly express his expansion of the potential realm of investigation of the sociology of knowledge:

> The existential determination of thought may be regarded as a demonstrated fact in those realms of thought in which we can show (a) that the process of knowing does not as a matter of fact develop historically in accordance with immanent laws, that it does not follow only from the 'nature of things' or from 'pure logical possibilities', and that it is not driven by an 'inner dialectic'. On the contrary, the emergence and the crystallization of actual thought is influenced in many decisive points by extra-theoretical factors of the most diverse sort that may be called 'existential factors'. This existential determination of thought will also have to be regarded as a fact (b) if the influence of these existential factors on the concrete content of knowledge is of more than mere peripheral importance, if they are relevant not only to the genesis of ideas, but penetrate into their subject matter and form, their content and formulation […].[15]

Mannheim's expansion of the potential area of sociological investigation to the *contents* and the *validity* of knowledge does, however, not

12 Cf. Mannheim 1946, pp. 45, 257, 269.

13 Cf. the paper of Martin Endreß in this volume and Endreß 2000 for an elaboration of this strategy.

14 'Existential determination' is, in one important respect, an unfortunate translation of the German 'Seinsverbundenheit'. The German term does not express a relationship of causal, lawlike necessity.

15 Mannheim 1946, p. 240 (improved translation).

cover all branches of knowledge and it is at this point, where we come to the topic of my paper. It is undeniable that Mannheim exempts the contents of the natural sciences and mathematics from his thesis of the existential determination of thought. He says:

> In assertions of this latter sort [i.e. in the humanities, M. S.], we may speak of an 'infiltration of the social position' of the investigator into the results of his study and of the 'existential-relativity', i.e. the relationship of these assertions to the underlying 'existence'. And we will contrast these assertions with those, which (like in the case of the assertion 2 times 2 = 4 just mentioned) do not contain such an infiltration of the social position of the investigator – at least not in a for us transparent way – into the assertion.[16]

Mannheim, therefore expands, the area of investigation of the sociology of knowledge to the *contents* and *validity* of knowledge, he flinches, however, from sociological investigations of the contents and validity of *all areas* of knowledge. Before I will go on to show how Mannheim's reservation concerning knowledge in the natural sciences has been interpreted, let me end with my short historical overview by considering one more recent position.

Having a look on the so-called 'Strong Programme', especially popular by the writings of Barry Barnes and David Bloor, we can see a further expansion of the area of sociological investigations. Let us start with the very first sentences of David Bloor's much discussed book *Knowledge and Social Imagery*:

> Can the sociology of knowledge investigate and explain the *very content* and nature of *scientific* knowledge? Many sociologists believe that it cannot. They say that knowledge as such, as distinct from the circumstances surrounding its production, is beyond their grasp. They voluntarily limit the scope of their own enquiries. I shall argue that this is a betrayal of their disciplinary standpoint.[17]

The italicized parts point to the problem Mannheim had, according to the Strong Programme: Though Mannheim investigates the *contents* of some knowledge, he hesitates to examine the contents of *scientific* knowl-

16 Mannheim 1946, p. 244 (improved translation), cf. also Mannheim 1952 b, p. 193.
17 Bloor 1991, p. 7 (italics added).

edge.[18] And the 'Strong Programme' is supposed to emancipate itself from Mannheim's and other traditional sociologists' restriction of scope by trying to show that the answer to the question posed by Bloor must be an unrestricted 'YES'. Thus, the impartiality and symmetry requirement of the programme can be read as an expression of demarcation from traditional sociology of knowledge, notably Mannheimian: Whilst Mannheim confined the explanatory task of the sociology of knowledge to a special class of beliefs, the Strong Programme seeks to explain true *and* false, scientific *and* unscientific, rationally *and* irrationally held beliefs by the same types of cause.[19] As will be shown in turn, the Strong Programme's demarcation from Mannheimian sociology of knowledge depends on a specific interpretation of his position concerning knowledge in the natural sciences.

Let us see, then, how Mannheim's position is usually interpreted.

The Standard Interpretation(s) of Mannheim with Respect to Knowledge in the Natural Sciences

It is undeniable and clear from the quote above that Mannheim makes *some* exemption concerning the natural sciences. However, the interpretations of Mannheim at this point differ with regard to two questions. The first question is: What exactly is exempted from sociological analysis with regard to the natural sciences? The second question is: Why does Mannheim exempt whatever is exempted from his sociological analyses?

With respect to the first question nobody denies that Mannheim forecloses the *contents* of the areas in question from sociological analyses. Some interprets, however, appear to go further. Take, for example, some of the claims by proponents of the Strong Programme. Barry Barnes in his monograph *Scientific Knowledge and Sociological Theory* indicates that "Mannheim, it will be remembered, accepted that mathematics and

18 It goes without saying that Mannheim is not the only one criticized by the proponents of the Strong Programme for limiting the scope of sociological investiagtions. Thus, Bloor claims: "Like Karl Mannheim before him, and many others since, Merton felt that sociological enquiry into the nature of knowledge was bound to be of a limited character." (Bloor 2004, p. 82). Cf. also Barnes/Dolby 1970.

19 Cf. Bloor 1991, p. 7.

the natural sciences lay beyond the scope of his theories"[20] and in a paper written with David Bloor declares that Mannheim "exempted the physical sciences and mathematics from his sociology of knowledge".[21] Note that these statements are ambiguous between the well-documented fact that Mannheim exempted the *contents* of the natural sciences from his thesis of existential determination and the more demanding thesis that Mannheim believes that the sociology of knowledge has nothing to say about the structure, methodology and the history of the natural sciences at all. The latter interpretation is suggested also by other authors. Thus Peter Farago believes that "according to Mannheim, there is no place for the sociology of knowledge in the history of exact sciences".[22] And Frederic Vandenberghe is convinced that "Mannheim explicitly exempted [the exact sciences] from the purview of his sociology".[23] The interpretation to the effect that Mannheim exempted the natural sciences completely from being within the scope of sociological analyses is not unique to authors who are sympathetic to relativist thought. Thus, in contrasting Mannheim and the Strong Programme, Robert Nola, a distinguished critique of both approaches in the sociology of knowledge, claims that "[all] of mathematics and science is to be included within the scope of [the Strong Programme] while they are excluded from the scope of Mannheim's [sociology of knowledge]."[24] We can therefore give two different interpretative answers to the first question as to what exactly is exempted in Mannheim's approach with regard to the natural sciences: The modest interpretation consists in accepting that Mannheim precludes the sociology of knowledge from analysing the *contents* of the natural sciences. According to a more ambitious interpretation Mannheim exempts *the* natural sciences from the purview of the sociology of knowledge. I will show that only the modest interpretation can be correct. In fact, as will turn out, Mannheim himself at points analyses what he calls 'the paradigm of thought of the exact-natural sciences'[25] by relating it to a specific historical and social situation.

20 Barnes 1974, p. 4.
21 Barnes/Bloor 1982, p. 26.
22 Farago 2002, p. 182.
23 Vandenberghe 1999, p. 56.
24 Nola 2003, pp. 198 f., cf. also Goldman 1986, pp. 303 f.
25 Cf. Mannheim 1946, p. 261.

What about the second question concerning the interpretation of Mannheim's exemption? Mannheim surely exempts the contents of the natural sciences but what are the reasons for this restriction of scope of sociological analyses? I think it is possible to find three different interpretations of Mannheim's restriction. As I hope to show in turn: In effect, all these do not get Mannheim right.

The first, and probably most prominent interpretation, is what I will call 'the failure-of-nerve-thesis'. This thesis plays an important role in the argumentation and foundation of the Strong Programme, especially in the texts of David Bloor. We find the thesis in the following passage from Bloor's *Knowledge and Social Imagery*: "Despite [Mannheim's] determination to set up causal and symmetrical canons of explanation, his nerve failed him when it came to such apparently autonomous subjects as mathematics and natural science."[26] According to Bloor, the failure of nerve is not just restricted to Mannheim – it is the explanation for the 'weak programmer's' reluctance to expand the disciplinary scope of the sociology of knowledge: "The cause of the hesitation to bring science within the scope of a thorough-going sociological scrutiny is lack of nerve and will. It is believed to be a foredoomed enterprise".[27] Bloor does not think that such a psychological explanation itself is sufficient to account for the traditional sociologist's hesitation. However, according to Bloor, the reluctance expresses itself in the sociologist starting an "a priori and philosophical argumentation".[28] The interpretation at issue thus consists in the claim that Mannheim exempted the contents of knowledge in the natural sciences from sociological investigation because his nerve failed him and that he stops to argue *sociologically* and *based on empirical fact* – the reproach is that Mannheim stops to argue scientifically and indulges oneself in philosophical speculations.[29] In Bloor's contrasting picture, the aim of the Strong Programme is to argue thoroughly sociologically – it is supposed to be a naturalistic and scientific programme. It is thus no exaggeration to claim that the failure-of-nerve-thesis stands in the centre of the argumentation of why the Strong Programme should be regarded

26 Bloor 1991, p. 11, cf. also Bloor 1973, p. 179.
27 Bloor 1991, p. 4.
28 Bloor 1991, p. 4.
29 Cf. also Barnes 1974, p. 147 f.

strong: Whereas the weak programmers at points lose nerves and adhere to philosophical argumentation, the Strong Programmers stand out with nerves of sociological steel.

The second interpretation can also be found in the writings of a Strong Programmer, namely Barry Barnes. At the beginning of his monograph *Interests and the Growth of Knowledge* Barnes distinguishes between a 'contemplative account' and – what can be called – a 'sociological account' of knowledge. Though the distinction of both accounts is much more complex, part of it consists in the same idea we already came across in the quotes from Gumplowicz, Scheler and Mannheim: Whilst the contemplative account "describes knowledge as the product of isolated individuals", on the sociological account "knowledge is treated as essentially social".[30] Barnes thinks that in the history of sociology of knowledge "individual writers rarely situat[e] themselves consistently and unambigiously on one side or the other" and Karl Mannheim stands up as a witness for Barnes' case:

> But, although these points [bespeaking for the sociological account, M.S.] are reasserted a number of times throughout the work, a great part of its argument and much of its concrete discussion is, in fact, predicated upon the contemplative model. Natural science and mathematics, Mannheim tells us, are forms of knowledge which bear no mark of the context of their production and which can properly be assessed entirely in terms of their correspondence with reality. Moreover, precisely *because* they are the products of disinterested contemplation, they are *preferable* to other kinds of knowledge, to sociology or history or political thought.[31]

This interpretation goes further than Bloor's: Like Bloor, Barnes assumes that behind Mannheim's exemption of the contents of knowledge in the natural sciences stands a non-sociological, philosophical model ('the contemplative account'). However, Barnes also thinks that Mannheim for this reason *evaluates* the knowledge in the different areas of knowledge differently. As is clear from the quote, Barnes believes that Mannheim thinks that knowledge in the natural sciences is *preferable* or *better* than knowledge in other branches of inquiry. The same thesis can also be found in the interpretation of the sociologist Bernd Schofer. He thinks that, for Mannheim, natural-scientific knowledge has "an episte-

30 Barnes 1977, p. 2.
31 Barnes 1977, p. 3.

mological privileged status".[32] Thus, the second interpretation consists in the claim that Mannheim believes in the *preferability* and *epistemological privilege* of knowledge in the natural sciences.

The third interpretation is closely connected to the former ones, it is better, however, to keep them separated. Whilst the first interpretation alludes to Mannheim's supposed failure of nerve and the second to a different evaluation of the knowledge in different areas of thought, the third interpretation maintains that Mannheim exempts the contents of knowledge in the natural sciences since the contents themselves *in principle* are such that they cannot be examined by sociological investigations. Recall the quote from Mannheim on the criteria for the areas in which to find 'existential determination': Those areas are exempted that "as a matter of fact develop historically in accordance with immanent laws, that […] follow only from the 'nature of things' or from 'pure logical possibilities', and that [are] driven by an 'inner dialectic'."[33] According to the third interpretation, Mannheim thinks that the very objects of knowledge in the areas of natural science and mathematics are such that the development of these areas is largely predictable. Thus, the distinction between knowledge in the 'exact sciences' and knowledge in areas of 'existentially determined thought' is not contingent – rather Mannheim believes that the very 'nature' of the objects of knowledge in these distinct areas is such that the results and contents of the natural sciences cannot be in focus of the sociology of knowledge in principle. We can find this interpretation again in the writings of Barnes and Bloor:

> Even the sociologist Karl Mannheim adopted this dualist and rationalist view when he contrasted the 'existential determination of thought' by 'extra-theoretical factors' with development according to 'immanent laws' derived from the 'nature of things' of 'pure logical possibilities'. This is why he exempted the physical sciences and mathematics from his sociology of knowledge.[34]

Furthermore, also Bernd Schofer thinks that Mannheim in restricting the scope of sociological investigations reacts on the different "objects of knowledge"[35] in the different areas: According to Schofer, Mannheim appeals to a "foundationalist interpretation of knowledge in the natu-

32 Schofer 1999, p. 45 (my translation).
33 Mannheim 1946, p. 240.
34 Barnes/Bloor 1982, p. 26, cf. also Longhurst 1989, p. 45.
35 Schofer 1999, p. 44 (my translation).

ral sciences"[36] and pleads for the absolute character of this knowledge whereas he treats the knowledge in the areas of existentially determined thought to be of a fundamentally different kind. The reason for this strict distinction of areas of inquiry is, according to Schofer, that Mannheim believes that it is simply adequate to the very objects of inquiry.[37] The distinction of the different areas of thought, thus, cannot be contingent. The idea of this interpretation is summarized in the following statement of Henk Woldring:

> Why does [Mannheim] keep the natural sciences outside of the social determiners of consciousness? Indeed, there are certain differences between the determining in the social and the natural sciences, but Mannheim makes that distinction absolute and comes to a division of human consciousness in two parts.[38]

The third interpretation, therefore, consists in the claim that Mannheim exempted the contents of knowledge in the natural sciences since he believes that knowledge in these areas is as a matter of principle of a fundamentally different kind than knowledge in the areas of existentially determined thought. The distinction between these areas is not contingent.

I think that these three interpretations are not exclusive: In several ways they are connected to each other and it is possible for an interpreter to think that all three interpretations are correct. Let us see whether the interpretations in fact are appropriate.

The Shortcomings of the Standard Interpretation(s)

As I admitted, it is futile to deny that Mannheim exempts the contents of the natural sciences and mathematics from his thesis of the existential determination of thought. Recall the following quote:

> In assertions of this latter sort [i.e. in the humanities, M. S.], we may speak of an 'infiltration of the social position' of the investigator into the results of his study and of the 'existential-relativity', i.e. the relationship of these assertions to the underlying 'existence'. And we will contrast these assertions with those, which (like in the case

36 Schofer 1999, p. 45 (my translation).
37 Cf. Schofer 1999, p. 44.
38 Woldring 1987, p. 165.

> of the assertion 2 times 2 = 4 just mentioned) do not contain such an infiltration of the social position of the investigator – at least not in a for us transparent way – into the assertion.[39]

Clear as this statement of the exemption of the contents of mathematics (and the natural sciences)[40] from the thesis of the existential determination of knowledge might be, it is often remarked by interpreters that Mannheim appears to be ambivalent on this issue.[41] For example, Mannheim also claims

> that in the quantum theory, for instance, where we are dealing with the measurement of electrons, it is impossible to speak of a result of measurement which can be formulated independently of the measuring instrument used [because] the measuring instrument […] itself relevantly influences the position and the velocity of the electrons to be measured.[42]

And he concludes that

> [if] we followed this trend of thought, which in its unformulated relationism is surprisingly similar to our own, then the setting-up of the logical postulate that a sphere of 'truth in itself' exists and has validity seems as difficult to justify as all of the other empty existential dualisms just mentioned.[43]

Let us not quarrel with whether Mannheim's assessment of the 'un-

39 Mannheim 1946, p. 244 (improved translation), cf. also Mannheim 1952 c, pp. 35 f., 44, Mannheim 1952 d, p. 130.

40 Cf. for explicit statements of the exemption of the contents of the natural sciences: Mannheim 1952 a, p. 135, Mannheim 1952 b, pp. 193 f.

41 Cf. Brown 1984, p. 4, Longhurst 1989, p. 45, Remmling 1975, p. 23, Schofer 1999, p. 44 Fn. 42, Scott 1998, p. 111.

42 Mannheim 1946, pp. 274 f.

43 Mannheim 1946, p. 275. With respect to this quote, Martin Endreß pointed out to me that there has been a development in Mannheim's position concerning the exemption of the contents of the natural sciences from sociological analyses (see his contribution in this volume and personal conversation). On the one hand, I totally agree with Endreß and, as will turn out, a development in Mannheim's attitude towards the natural sciences is no surprise from the point of view of my interpretation: Mannheim's outlook includes that it is adequate also to investigate the contents of the natural sciences by sociological means *in case they demand it*, and his 1931 reference to quantum theory might lay testimony to the fact that he thought *then* that the natural sciences demand it. On the other hand, I do not think that there has been a change in Mannheim's general philosophical outlook concerning this question. Since I will present testimony for my interpretation from his early as well as late work I think that the reason for the development in Mannheim's attitude is due to a change in his view of the natural sciences from *the same* general *philosophical* and *sociological* point of view as in his early writing.

formulated relationism' in quantum theory is correct.[44] What is decisive for the task of interpreting Mannheim is that the statement points to an ambivalence in Mannheim's position: On the one hand, Mannheim clearly restricts his "thesis of the inherently relational structure of human cognition"[45] to the humanities and the social sciences, on the other hand he sees a trend of thought with an 'unformulated relationism' in theories in the natural sciences. At this point the question emerges immediately how to explain this ambivalence. The answer to this question will show that Mannheim's position in one sense is anti-naturalistic and in another sense is naturalistic.[46] And this fact will cast light on Mannheim's general epistemological outlook and his handling of the issue of relativism. Furthermore, it will clarify Mannheim's position on knowledge in the natural sciences and mathematics.[47]

44 Surely, it has to be pointed out that Mannheim's claim that in quantum theory we find an 'unformulated relationism' that is supposed to make it difficult to justify 'a postulate of the existence of a sphere of truth in itself' needs a lot of further explanation on Mannheim's part. Depending on what exactly is meant by 'relationism' in this context it risks to be either trivial or simply false (cf. for an early criticism: Hinshaw 1943, pp. 65 f.). For the present purpose of showing that Mannheim does not exempt the contents of the natural sciences *in principle* of sociological investigations, it is, however, not necessary to make an attempt at understanding Mannheim's elliptical claim.

45 Mannheim 1946, p. 269 (improved translation).

46 Distinguishing between anti-naturalistic and pro-naturalistic doctrines in this context demands a short note on the differences between the distinction here and the distinction as it is used in Popper's book "The Poverty of Historicism". Popper also distinguishes between anti- and pro-naturalistic doctrines in historicism and, notably, in the work of Mannheim (cf. Popper 2002). Part of Popper's description of the anti-naturalistic doctrines comes close to the one that I am going to use in the following: "It is the doctrine that the proper method of the social sciences, as opposed to the method of the natural sciences, is based upon an intimate understanding of social phenomena" (Popper 2002, p. 17). However, his description of the pro-naturalistic doctrine is different from the one that concerns me. As for the pro-naturalistic doctrines, Popper identifies in historicist methodology "a strangely […] sociological theory – the theory that society will necessarily change but along a predetermined path that cannot change, through stages predetermined by inexorable necessity" (Popper 2002, p. 46). Whether or not Mannheim believes in deterministic, historical laws of social development is of no concern for my description of his pro-naturalistic thesis.

47 In this way, my interpretation is following recent trends. Cf. the remark of Zammito 2007, pp. 802 f.: "Recent research suggests that Bloor never fully realized the particular contextual and conceptual concerns which animated Mannheim's original form of the sociology of knowledge, and consequently misunderstood Mannheim's attitude toward natural science and mathematics".

Historicism and Mannheim's Anti-Naturalism

Mannheim undeniably stands in a historicist tradition of distinguishing between the methods of the so-called *Naturwissenschaften* (natural sciences) and *Geisteswissenschaften* (humanities) by defending a distinctive method of understanding (*Verstehen*) against a method of explanation (*Erklären*):[48] Just recall Wilhelm Dilthey's famous dictum „Die Natur erklären wir, das Seelenleben verstehen wir".[49] In this sense, Mannheim's position is thoroughly anti-naturalistic: He denies that there is only one ultimate kind of method in the sciences, namely the explanatory method that manifests itself paradigmatically in the natural sciences.[50] He speaks of "abandoning the natural-scientific way" and "treading the path of interpretative psychology".[51] Therefore, the humanities must

> emancipate [themselves] completely from the hegemony of the methodological principles of natural science; for in the natural sciences, where problems of this kind are necessarily lacking, we encounter nothing even faintly analogous to the thought patterns with which we have to deal at every step in the cultural sciences.[52]

The natural sciences need one sort of thought pattern, the humanities need another one. The difference in thought patterns is made clear, so Mannheim believes, by a distinction of what he calls *static* and *dynamic* thinking: Whilst the thought pattern of the natural sciences is supposed to be static, in the humanities we need to establish a thought pattern of dynamic thinking.[53] This also has consequences for the kind of knowledge and progress in the different sciences: In the natural sciences, the development is supposed to be 'linear' and "it is possible to accumulate knowledge and discover truths […] without reference to the historical

48 Cf. Mannheim 1952 c, pp. 81 f., Diskussion 1982, pp. 400 f.
49 Dilthey 1924, p. 144.
50 Cf. Mannheim 1982, pp. 75 f., 185: "[One] of the unjustified assumptions of every natural-scientific conception of thinking consists of hypostasizing one form of knowledge as knowledge *per se*.", cf. also Mannheim 1946, pp. 150 f. It is this position that Mannheim himself calls 'naturalism' and critizes thoroughly. Mannheim's criticism of naturalism is influenced especially by Troeltsch. Cf. also Simonds 1978, pp. 36 ff.
51 Mannheim 1982, p. 76.
52 Mannheim 1952 c, p. 37 (improved translation), cf. also Mannheim 1952 c, pp. 70 f., 82.
53 Cf. Mannheim 1952 d, p. 92.

background of the knowing subject".[54] In the humanities, however, "the course and structure of development are altogether different."[55] It goes without saying that such a form of methodological dualism cannot be acceptable for the proponents of the Strong Programme – and not just for them.[56] Manley Thompson has characterized naturalism as "the view that the methods of natural science provide the only avenue to truth".[57] In this sense, to be sure, Mannheim's position is anti-naturalistic.

From this undeniable anti-naturalist, methodological dualism of Mannheim, it is a short way to take his exemption of the contents of the natural sciences from the thesis of existential determination as an argument against the relativistic implications of his sociology of knowledge – as, in fact, Bernd Schofer has proposed.[58] Thus Mannheim claims that

> the accusation of relativism derives from a philosophy which professes an inadequate conception of 'absolute' and 'relative'; a philosophy which confronts 'truth' and 'falsehood' in a way which makes sense in the sphere of so-called exact science.[59]

Notice, however, that – in accordance with Mannheim's thought – it is not simply the restriction of the sociology of knowledge to the contents of the humanities that is at work here. It is crucial to see that it is *not the restriction of scope* of the thesis of existential determination that is supposed to debilitate the reproach of relativism, but exhibiting the alleged inadequacy of a certain philosophical approach to the knowledge of the humanities and social sciences, an approach that is according to Mannheim adequate only for the natural sciences.[60] In fact, a simple restriction of scope cannot help to counter the reproach of relativism that is at issue here, since Mannheim tries to answer the reproach concerning the knowledge of the humanities, and answering this reproach by pointing to the knowledge of the natural sciences would – on Mannheim's assumption of the cru-

54 Mannheim 1982, p. 98, cf. also Mannheim 1952 d, p. 115, 1952 a, p. 135.
55 Mannheim 1982, p. 99.
56 Cf. e.g. Bloor 1991, p. 19.
57 Thompson 1964, p. 193, cf. also Keil/Schnädelbach 2000, p. 25.
58 Cf. Schofer 1999, p. 45. Especially Mannheim's claim about the linear development and the accumulation of truth and knowledge in the natural sciences cannot be accepted by those relativists in the sociology of scientific knowledge who see themselves in a Kuhnian heritage (Cf. especially Barnes 1982 and also Barnes' contribution to this volume).
59 Mannheim 1952 d, p. 93.
60 Cf. Kaiser 1998, p. 52.

cial differences of the humanities and the natural sciences – simply be to change the topic. To say it shortly: Mannheim does not react to relativism by proposing a merely *local* relativism, but by attacking the philosophical and epistemological presuppositions of the attack. And he does so, as will be shown in turn, by *naturalizing* epistemology in a sense.

Mannheim's Naturalism and Anti-Foundationalism

Let us investigate Mannheim's attack on the philosophical background of the critic's reproach in detail. Despite Mannheim's clear anti-naturalist, methodological dualism of understanding and explanation, there is a sense in which Mannheim's position can be called *naturalistic*.

In the present context, it is methodological naturalism that is at issue[61] and I will distinguish between two different theses in naturalistic positions.[62] In the modern classic of naturalized epistemology, namely Quine, these theses are combined: Quine argues against "the goal of a first philosophy prior to natural science"[63] and "see[s] philosophy not as an *a priori* propaedeutic or groundwork for science, but as continuous with science".[64] The first thesis, to be detectable in Quine, is a thesis about the methodology of disciplines and claims that there is only one kind of scientific methods for all genuine scientific work. The second thesis that can be found in Quine is a thesis about the relationship between epistemology and empirical investigations in general and denies that epistemology should be the a priori foundation of the empirical sciences.

The first thesis can be found in many descriptions of naturalism. Thus, e.g., Arthur Danto claims:

> Naturalism […] is a species of philosophical monism according to which whatever exists or happens is natural in the sense of being susceptible to explanation through methods which, although paradigmatically exemplified in the natural sciences, are continuous from domain to domain of objects and events.[65]

61 It is an interesting question how some forms of methodological naturalism are related and possibly dependent on some forms of ontological naturalism. For the present purpose, however, it is not necessary to look at these relations in detail.

62 There are many forms of naturalism, but for the present purpose a more fine-grained distinction is not necessary. Cf. for such a project: Haack 1993a, Koppelberg 1996.

63 Quine 1981a, p. 67, cf. also Quine 1981a, p. 72, Quine 1981b, p. 20.

64 Quine 1969, p. 126.

65 Danto 1967, p. 448, cf. also Sellars 1967, p. 173.

In the following, therefore, I will refer to the first thesis as the thesis of *monistic methodological naturalism*. However, the second thesis can also be found in many writings on naturalism. For example, Dirk Koppelberg describes the 'traditional-analytic epistemology' against which naturalistic epistemology objects to as consisting of two theses:

> 1. In its status as discipline epistemology is independent of our empirical beliefs and of the sciences, because it previously aims at providing the foundations for these. This is the thesis of disciplinary autonomy. 2. In its methodological procedure epistemology is committed to pure conceptual analysis, whose results can be recognized a priori and are valid necessarily. This is the thesis of concept-analytic autonomy.[66]

In the following, therefore, I will refer to the second thesis of naturalism that aims at denying this picture of traditional-analytic epistemology as the thesis of *anti-foundationalist methodological naturalism*.

We have already seen that Mannheim is attacking *monistic methodological naturalism*. However, in order to understand Mannheim's position concerning the natural sciences more clearly, it is necessary to note that he *explicitly* argues against the picture of traditional-analytic epistemology just described and therefore *defends* an *anti-foundationalist methodological naturalism*.[67] He "adduce[s] those arguments which undermine or at least call into question the absolute autonomy and primacy of epistemology as over against the special sciences"[68] and claims that "notwithstanding its claim to be the fundamental science and the critique of all experience as such, epistemology in fact always exists only as a justification of a mode of thought already existing or just emerging".[69] Mannheim investigates the relationship between the special sciences and epistemology. His picture of this relationship is that of a mutual foundation. On the one hand, epistemology is supposed to be foundational in that "it supplies the basic justifications for the types of knowledge"[70] of the special sciences. Mannheim's position implies "no denial […] of the importance of episte-

66 Koppelberg 1996, p. 74 (my translation).
67 Cf. also Frisby 1993, p. 170, Raven et al. 1992, pp. xiii f., Remmling 1973, p. 23. Frisby sees a connection to Kuhn: "Mannheim here anticipates some elements of Kuhn's argument concerning the development of scientific knowledge in that he argues that revolutions in epistemology succeed revolutions in science and not vice versa and, in that, he sees epistemology as a mode of legitimation of the existing state of science" (Frisby 1993, p. 170).
68 Mannheim 1946, p. 259.
69 Mannheim 1952 b, p. 227.
70 Mannheim 1946, p. 259.

mology or philosophy as such."[71] On the other hand, however, he is also convinced that epistemology is not independent of science, but influenced by the form science takes at a given moment. Thus, "[in] principle, no doubt, [epistemology] claims to be the basis of all science but in fact it is determined by the condition of science at any given time."[72] He clearly debilitates against the thesis of disciplinary autonomy as described above:

> [The] belief is no longer tenable that epistemology and noology, because of their justifiable claim to foundational functions, must develop autonomously and independently of the progress of the special sciences, and are not subject to basic modifications by these.[73]

We see Mannheim's denial of what we have called 'traditional-analytic epistemology' in the following remarkable passage:

71 Mannheim 1946, p. 260. Whether or not Quine aims at conducting epistemology wholly within the natural sciences or not, and whether Quine proposes what has been called a "replacement naturalism" (Cf. Feldman 2001) is the issue of debate. Quinean naturalism famously sees 'epistemology, or something like it, as a chapter of psychology' (Cf. Quine 1969, p. 82). See on the two faces of Quine's naturalism: Haack 1993 b.

In addition, Mannheim's position is not as clear as it seems: In his essay 'Historicism' he claims that from his new 'dynamic point of view' "the place of epistemology as a fundamental science will be taken by the philosophy of history as a dynamic metaphysic; all problems as to how the various realms of thought and life are 'grounded' in one another become re-oriented anew around this point of departure" (Mannheim 1952 d, p. 97, improved translation, cf. also Mannheim 1952 d, p. 127). Mannheim's later turn away from history to the sociology of knowledge might also be interpreted as implying that "[the sociology of knowledge] will in fact be the master science dealing with the validity of knowledge, taking the place of epistemology" (Kecskemeti 1952, p. 18) and "the decisive thesis that sociology is the fundamental science providing the criteria for the validity of socio-existentially determined knowledge" (Remmling 1975, p. 23). And concerning the investigation of values, Mannheim claims that "[w]hat will really happen will be that the theological and philosophical obligation will be replaced by a sociological one" (Mannheim 1957, p. 132). Thus, it is possible to discern reductionist tendencies also in Mannheim's thought. Mannheim himself, however, explicitly denies such a kind of reductionism: "First, what must be said quite clearly is that I do not wish to replace philosophy by sociology" (Mannheim 1993, p. 445).

72 Mannheim 1946, p. 259, cf. also Mannheim 1952 c, p. 37: "Methodology seeks but to make explicit in logical terms what is *de facto* going on in living research."

73 Mannheim 1946, p. 259, cf. also Mannheim 1982, pp. 151 f. "To see clearly in these matters, one must not forget that philosophy, life, and scientific knowledge never go along side by side in isolation. Philosophy, in its various tendencies, always rises, rather, out of a current of life (and usually one that is social conditioned), and serves as pioneer for it, first formulating, in premonitory anticipation and programmatically, the new 'will to the world' (Weltwollen), only to return in the sequel and penetrate life and science itself."

> New forms of knowledge, in the last analysis, grow out of the conditions of collective life and do not depend for their emergence upon the prior demonstration by a theory of knowledge that they are possible; they do not therefore need to be first legitimized by an epistemology. The relationship is quite the reverse: the development of theories of scientific knowledge takes place in the preoccupation with empirical data and the fortunes of the former vary with those of the latter. The revolutions in methodology and epistemology are always sequels and repercussions of the revolutions in the immediate empirical procedures for getting knowledge. Only through constant recourse to the procedure of the special empirical sciences can the epistemological foundations be made sufficiently flexible and extended so that they will not only sanction the claims of the older forms of knowledge (their original purpose) but will also support newer forms. This peculiar situation is characteristic to all theoretical, philosophical disciplines. Its structure is most clearly perceivable in the philosophy of law which presumes to be the judge and critic of positive law, but which is actually, in most cases, no more than a *post facto* formulation and justification of the principles of positive law.[74]

Now, decisively, this sort of naturalizing epistemology in the sense of anti-foundationalist methodological naturalism, does not stop short of the natural sciences. Undeniably, Mannheim identifies a static epistemology and methodology as the foundation of the natural sciences. This, however, is not unchangeable but, in accordance with Mannheim's epistemological outlook, something to be investigated by historical and sociological means. Mannheim's anti-foundationalist outlook also on the natural sciences can be seen quite clearly in the following quote:

> Let a new mode of cognition with a certain paradigmatical structure arise, such as, for instance, modern natural science, and epistemology will try to explain it. […] Since it finds the paradigm as already given, its view will be oriented by this partial paradigm – also its concept of truth will be the product of this ex-post-situation. […] The most important fact of the point of view of the sociology of thought […] is that it is not, as one would be tempted to assume at first sight, one epistemology that struggles with another, but the struggle always goes on between already existing modes of thought, paradigms, which the respective epistemologies only serve to justify. In the historical-social context epistemologies are only advance posts in the struggle between thought-styles.[75]

Mannheim's picture concerning the natural-sciences is thus the following: A certain thought-style develops historically. We can investigate the

74 Mannheim 1946, pp. 259 f., cf. also Mannheim 1952 b, pp. 227 f.
75 Mannheim 1952 b, pp. 227 f.

thinking of this thought-style sociologically and historically. This, is true, also for the thought-style of the natural-sciences as Mannheim makes clear:

> Also this thinking is not free-floating from the sociological point of view, since the basic impulses from which exact inquiry ascends are bound to a determinate stadium in social development […] and the needs of the social still influence the questions and direction of inquiry of natural-scientific cognition[76].

Once we investigate the thought-style of the natural-sciences, however, we find – and this might undeniably prove wrong – a cumulative growth of knowledge in the history of the natural sciences. Therefore, in accordance with anti-foundationalist methodological naturalism, Mannheim can claim that also the thinking in the natural sciences is socially conditioned[77] and investigate this social determination at length,[78] but come to the conclusion that the contents of the natural-sciences are exempted from the thesis of existential-determination: It is the socially dependent and historically investigable "paradigm of thought of the exact natural-sciences"[79] itself that includes the ideal of absolute truth.[80] For Mannheim, therefore, the "ultimate task in this respect is to re-interpret the phenomenon of static thought – as exemplified by natural science and by other manifestations of the civilizational sphere in general – from a dynamic point of view".[81] Thus, it is Mannheim's denial of a traditional-analytic epistemological outlook that explains his alleged ambivalence concerning the natural sciences: If epistemology and the sciences go on a par, it is by no means surprising that Mannheim sees a relational epistemology in the natural sciences in case the historical investigation reveals that they

76 Mannheim 1984, p. 66 (my translation).
77 Cf. Mannheim 1984, p. 66.
78 Cf. Mannheim 1982, pp. 151–6.
79 Mannheim 1946, p. 261 (improved translation).
80 Cf. Mannheim 1946, p. 262: "We see, therefore, not merely that the notion of knowledge in general is dependent upon the prevailing form of knowledge and the modes of knowing expressed therein and accepted as ideal, but also that the concept of truth itself is dependent upon the already existing types of knowledge." Cf. also Mannheim 1952 b, p. 227. Mannheim investigates the social and political components of the absolute conception of truth and the 'demand of universal validity': "With this, there was revealed a purely sociological component in the criterion of truth, namely, the democratic demand that these truths should be the same for everyone. This demand for universality had marked consequences for the accompanying theory of knowledge" (Mannheim 1946, p. 149).
81 Mannheim 1952 d, p. 132.

demand it. And if the historical investigation unveiled an hitherto unknown relationism in the natural sciences, also the *contents* of knowledge in these areas could be investigated by sociological means.

We can now assess the interpretations of Mannheim according to the picture just revealed from his writings. First of all, it should be clear by now that Mannheim does not exempt *the* natural sciences from being in purview of sociological investigations: Thinking in the natural sciences, according to Mannheim, develops in determinate historical and social situations and we can investigate this development – in fact, Mannheim does at points. Secondly, Mannheim's exemption of the contents of knowledge in the natural sciences is not an expression of lack of nerve and will as the failure-of-nerve-thesis suggests. The reason for the exemption is his own analysis of the social and historical background of thinking in the natural sciences. Mannheim might be wrong in thinking that the natural sciences historically developed cumulative. However, Mannheim's claim need to be understood to be a fallible, historical and sociological claim – not a claim of a sociologist suddenly starting dubious philosophical speculations once his nerve failed him. Thirdly, the claim that Mannheim believes in the *preferability* and *epistemic privilege* of knowledge in the natural sciences cannot be sustained once we take serious Mannheim's denial of *monistic methodological naturalism*. If Mannheim thought of knowledge in the natural sciences as preferable and epistemically privileged, we should have expected that he pleads for an attempt to purify knowledge in the humanities from the vitiating elements. However, Mannheim does quite the contrary and espouses – as we have seen in the quote above – for an *emancipation* of the humanities from the methods of the natural sciences. In fact, Mannheim explicitly thinks – quite in accord with his philosophical tradition – that "there are also elements in this knowledge which assure deeper penetration into its object than there is ever possible in the exact sciences" such that "[there] is a moment within qualitative knowledge by virtue of which it is unquestionably superior to natural-scientific knowledge".[82] I do not see how

82 Mannheim 1982, p. 252, cf. also Mannheim's diagnosis that "We shall see in this a defect of existentially-determined thinking only if we adopt a methodology based upon the exact natural sciences as a model" (Mannheim 1952 b, p. 194).

to reconcile these statements with the claim that Mannheim thinks of knowledge in the natural sciences as *preferable* or *epistemically privileged* to existentially determined knowledge.

What, however, about the interpretation that Mannheim exempted the contents of knowledge in the natural sciences since he believes that the knowledge in these areas is as a matter of principle of a fundamentally different kind than knowledge in the areas of existentially determined thought and that the distinction between these areas is not contingent. My interpretation resting on the distinction between naturalistic and anti-naturalistic trains of thought in Mannheim's position appears to sustain this interpretation: Mannheim's denial of monistic methodological naturalism suggests that the objects in the areas of the humanities and the natural sciences are of fundamentally different kinds such that this Mannheimian distinction explains his exemption of the content of knowledge in the natural sciences from sociological analysis.

Again, we need to understand that Mannheim proposes an anti-foundationalist methodological naturalism in order to see why this interpretation cannot be correct. Recall that Mannheim thinks that "notwithstanding its claim to be the fundamental science and the critique of all experience as such, epistemology in fact always exists only as a justification of a mode of thought already existing or just emerging".[83] Now, as we have seen, for Mannheim this diagnosis does not stop short of thinking in the natural sciences. And, decisively, it also does not stop short of Mannheim's *own* thinking. Therefore, as we can see in many places of his work,[84] Mannheim clearly proposes a principle of reflexivity – a principle of the applicability of his theories and views to his own position. If he does so consistently, then Mannheim also needs to view his own division of methods in the humanities and the natural sciences as the product of a certain episode in history and a determinate social constellation. And, this is exactly Mannheim's consequence:

> How far our own account is positionally determined and how far we are aware of this, we wish to clarify by a remark which is essential to the thesis propounded here. At the beginning of this paper, we postulated a rigid methodological dualism between the exact sciences and the 'historical-cultural sciences'. This dualism cannot

83 Mannheim 1952 b, p. 227.

84 Cf. Mannheim 1946, pp. 68 f., 103 f., 168 f., Mannheim 1952 a, p. 137, Mannheim 1952 d, p. 130 Fn. 1, Mannheim 1993, p. 443, Mannheim 1982, pp. 178, 180.

> be the final form in which the problem of scientific method presents itself. […] We find ourselves, however, at a stage in the history of thought which is so preoccupied with special disciplines and thus with partial systems that philosophical construction unavoidably slips back into one of these 'partial' systems and therefore into methodology – even where this was not intended. We just 'see' thought still either from the point of view of the natural sciences or of late more and more from that of the historical sciences[85].

From this quote, it is clear that Mannheim does not think of a divide *in principle* of the methods of the natural sciences and the humanities. He explicitly claims "that the justification for a duality (or plurarity) of methods of thought does not lie in the area of inquiry".[86] His own monistic methodological naturalism is – quite in accord with his own anti-foundationalist methodological naturalism – the ex post expression of a specific historical and social situation. Mannheim, therefore, – to quote Robert Brandom from another context – "treat[s] the *distinction* between things that have natures and things that have histories, between things studied by the *Naturwissenschaften* and things studied by the *Geisteswissenschaften*, as itself a cultural formation".[87] Recall Mannheim's statement that "the revolutions in methodology and epistemology are always sequels and repercussions of the revolutions in the immediate empirical procedures for getting knowledge."[88] There is no reason to think that Mannheim exempts his own methodology from this rule and believes in an *absolute* distinction between the methodologies adequate for different realms of thought.

One obvious rejoinder comes to mind: Surely, it might be argued, I have shown by quoting in detail from Mannheim's work that Mannheim's attitude towards the sociological treatment of knowledge in the natural sciences is much more complicated than usually has been thought. However, as revealing as this might be, it does not speak against the traditional interpretation – it speaks against clarity in Mannheim's own thought. Thus, the critic might suggest, I have constantly omitted those quotes from Mannheim's work that clearly speak in favour of the traditional interpretation.

My final task, thus, will be to show that even the quotes that allegedly

85 Mannheim 1952 d, p. 130 Fn. 1.
86 Diskussion 1982, p. 398 (my translation), cf. also Mannheim 2003, p. 151.
87 Brandom 2000, p. 27.
88 Mannheim 1946, p. 260.

bespeak the traditional interpretation cannot be used to demur my interpretational thesis. Here is the quote that is constantly referred to in order to sustain the traditional interpretation:

> The existential relatedness of thought may be regarded as a demonstrated fact in those realms of thought in which we can show […] that the process of knowing does not as a matter of fact develop historically in accordance with *immanent laws*, that it does not follow only from the '*nature of things*' or from '*pure logical possibilities*', and that it is not driven by an '*inner dialectic*'. On the contrary, the emergence and the crystallization of actual thought is influenced in many decisive points by *extra-theoretical factors* of the most diverse sort. These may be called, in contradistinction to purely theoretical factors, existential factors.[89]

It is exactly this quote that is used by Bloor whenever he is going to sustain his 'failure-of-nerve-thesis'.[90] The italics in this quote are mine – they are set in order to emphasize the key notions that are important for the interpretation along the traditional lines. Thus, so the argument goes, as can be seen clearly in the quote Mannheim distinguishes between different realms of thought. Those where there are existential factors to be investigated by the sociology of knowledge. And those in which thinking develops in accordance with 'immanent laws' and by inner, purely theoretical factors. Here, the sociology of knowledge has nothing to investigate, since there are supposed to be no social causes, unfortunately identified with 'extra-theoretical factors' by Mannheim, in play. Such knowledge, pure and unconstrained by social factors is taken by Mannheim to be "apparently autonomous"[91] and "*preferable* to other kinds of knowledge".[92]

As I said in the very beginning, surely Mannheim exempts the contents of knowledge in the natural sciences and mathematics from sociological analyses and it would be absurd to claim the contrary if we take the quote above seriously. However, once we have a look on the quote again we see that the quote actually sustains my interpretation of Mannheim. Thus, take the quote with a different emphasize marked again by italics of mine:

> The existential relatedness of thought may be regarded as a *demonstrated fact* in those realms of thought in which *we can show* […] that the process of knowing does not *as a matter of fact develop historically* in accordance with immanent laws, that

89 Mannheim 1946, pp. 239 f. (improved translation).
90 Cf. Bloor 1973, p. 179, Bloor 1991, p. 11. The quote is also used by critics of the programme: Cf. Nola 2003, p. 194.
91 Bloor 1991, p. 11.
92 Barnes 1977, p. 3.

> it does not follow only from the 'nature of things' or from 'pure logical possibilities', and that it is not driven by an 'inner dialectic'. On the contrary, the emergence and the crystallization of actual thought is influenced in many decisive points by extra-theoretical factors of the most diverse sort. These may be called, in contradistinction to purely theoretical factors, existential factors.

There is no failure of nerve or general retreat from sociological investigation once it comes to the realms of knowledge in the natural sciences and mathematics. And there is no exemption of the contents of knowledge in these areas *in principle*. On the contrary, Mannheim sees the urgency to *demonstrate* the special character of knowledge here and to *show* that *as a matter of fact* these areas developed historically as he believes they did.

Concluding Remarks

My proposal has been to have a closer look at Mannheim's famous thesis of the exemption of knowledge in the natural sciences from sociological analysis. First of all, it has been shown that Mannheim does not treat *the* natural sciences from being out of reach of sociology completely: He discusses the social and historical background of the thought-style of the natural sciences at length. Furthermore, it has been shown that many of the interpretations of Mannheim's exemption of the contents of knowledge in the natural sciences fall short of being adequate. Mannheim's exemption is not understandable as a failure of nerve expressing itself in philosophical speculation. On the contrary, Mannheim thinks that the *historical* and *social* analysis of thinking in the natural sciences reveals that as a matter of fact the contents of knowledge in these areas "[give] no clue as to when, where, and by whom [they] were formulated".[93] Mannheim also does not believe in the preferability or epistemic privilege of knowledge in the natural sciences: On the contrary, he thinks that knowledge in the humanities assures a deeper penetration into its object and, therefore, is superior especially in an epistemological manner. Also Mannheim does not believe in an *absolute* divide of methodologies demanded by the fundamentally different 'nature' of the objects of inquiry

93 Mannheim 1946, p. 244.

in the different areas. His own denial of monistic methodological naturalism should be seen as reflexively in view of his anti-foundationalist methodological naturalism.

What is the upshot of all this for the discussion of relativism in the sociology of knowledge with regard to Mannheim's position? I think that a fair discussion of Mannheim's contribution should treat him as a *respectable* predecessor of relativistic thought in the sociology of knowledge. That is: I hope that my close inspection of his treatment of knowledge in the natural sciences reveals the prevalence of a kind of caricature of Mannheim's position in recent discussion. Though I am surely no defender of Mannheim's relationist position, a serious treatment of his thought might prove fruitful not only for historical reasons.

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Knowledge, Practice, and the Problem of Relativism – Reconsidering Michael Polanyi's 'Personal Knowledge'

EVA-MARIA JUNG

Introduction

The concept of *tacit* or *implicit knowledge* is widely used in many fields of sciences, such as psychology, philosophy, cognitive and neurosciences, or knowledge management.[1] Particularly during the last decades, the interest in the implicit dimension of cognition has grown steadily. By emphasizing that a variety of tacit processes shape our knowledge, the ideal of a wholly explicit knowledge expressible and transformable by language has been challenged. In the light of these recent developments, it is remarkable that the British-Hungarian philosopher Michael Polanyi (1891–1976) who introduced the concept of tacit knowledge in the middle of the twentieth century is relatively unknown, especially within European philosophy of science and epistemology.[2] Neither the backgrounds of Polanyi's approach to knowledge nor his intentions for referring to the *tacit dimension* have come into focus.

In this paper, I will focus on the question of how Polanyi's theory faces the problem of relativism. I will argue that although Polanyi explicitly rejects relativism his discussion of scientific controversies yields certain relativist conclusions. Moreover, his theory threatens to become inconsistent due to a tension between these relativist tendencies and his epistemic and scientific realism. The paper is divided into three parts. First,

1 See, for example, Baumart 1999, Collins 2010, Reber 1996 and Sternberg & Horvath 1999, just to name a few recent books.

2 For an overview of the reception of Polanyi's work see Mitchell 2006, pp. 137 ff.

I will give a general overview of Polanyi's approach to knowledge and its background. Second, I will discuss Polanyi's theory in relation to the problem of relativism. Third, I will highlight some major similarities and differences between Polanyi's view and recent accounts of tacit and explicit cognition.

Polanyi's "Personal Knowledge"

Polanyi: Life and Times

Polanyi had been working as a scientist for more than three decades when he decided to devote his academic life to philosophy and the social sciences.[3] Born in Hungary in 1891, he studied medicine and received a doctorate in chemistry from the University of Budapest in 1917. Three years later, Polanyi moved to Berlin where he took a position at the Kaiser-Wilhelm-Institute for Fiber Chemistry until he was invited by Fritz Haber to head a department at the Kaiser-Wilhelm-Institute for Physical Chemistry in 1923. During his research time in Germany, Polanyi got in contact with a circle of highly prominent scientists including Albert Einstein, Max Planck and Erwin Schrödinger. He was widely considered as an extraordinary, headstrong talent and as a leading candidate to win a Nobel Prize. In the face of the political changes in Germany during the early 1930s, Polanyi decided to move to Britain where he accepted the Chair of Physical Chemistry at the University of Manchester. Since his interests had shifted from natural science towards economy, sociology, philosophy and theology, the University of Manchester created for him a Chair of Social Sciences in 1948. Although some of his scientific peers deeply regretted his change of interest, Polanyi himself claimed to have found his true vocation as a philosopher; he regarded his turn to philosophy as an "afterthought"[4] to his career as a scientist.

Polanyi's contributions to the humanities and the social sciences cover a wide range of issues, addressing various questions about science and society. In particular, many authors from different fields have adopted his concept of tacit knowledge which lies at the core of his work. According to Polanyi, the question of how to appropriately analyze knowledge is

3 For Polanyi's biographical details see Wigner & Hodgkin 1977 and Mitchell 2006.
4 Polanyi 1966, p. 3.

not only important for epistemology and philosophy of science but also has far-reaching implications for ethics and political philosophy.[5] Since his arguments are usually based upon a variety of sources from different scientific and non-scientific disciplines and even include personal experiences, Polanyi's work is rather complex and idiosyncratic. In the next sections, I will summarize the main ideas of his approach to knowledge including the theoretical background.

Establishing a Post-Critical Philosophy

Within Polanyi's philosophy, tacit knowledge is inextricably linked to the concept of *personal knowledge* which lies at the core of his plea for a new ideal of knowledge and science. He explicitly attacks what he calls "objectivism" – the tendency to think of knowledge as wholly explicit, impersonal and objective.[6] Thus, Polanyi's theory of knowledge has been considered as strongly opposed to Karl R. Popper's[7] who expressly defends an ideal of knowledge as "usually independent of anybody's claim to know",[8] as "*knowledge without a knower*".[9] In contrast, Polanyi argues that the concept of knowledge should be strongly related to the knowing subject. The idea that science is guided by an ideal of wholly objective findings and scientific methods is, according to him, totally misleading and yields a problematic consequence, namely the split of fact and value. He claims that objectivism which is said to be tied to a "critical philosophy"[10] has gone too far: the attempt to avoid unjustified dogmas and prejudices has led to an ideal of knowledge totally context-independent and impersonal, thereby ignoring the real nature of knowledge and its roots in society and tradition.[11] Therefore, Polanyi's declared aim is to establish a "post-critical philosophy" by focusing on a principally new concept of knowledge. As he puts it in the foreword of his magnum opus "Personal Knowledge" (1958):

5 Cf. Polanyi 1946, pp. 7–19 and Polanyi 1958, pp. vii–viii.
6 Cf. Polanyi 1958, pp. 15 ff.
7 For the dispute between Polanyi and Popper see, for example, Hall 1982.
8 Popper 1972, p. 109.
9 Ibid., italics in original.
10 Polanyi 1958, p. 169.
11 Cf. ibid., p. 264 ff.

> I want to establish an alternative ideal of knowledge, quite generally. Hence the wide scope of this book and hence also the coining of the new term I have used for my title: Personal Knowledge. The two words may seem to contradict each other: for true knowledge is deemed impersonal, universally established, objective. But the seeming contradiction is resolved by modifying the conception of knowing.[12]

The turn to a post-critical philosophy shall be based upon our relating the concepts of belief and knowledge to a new framework that Polanyi traces back to Saint Augustine. In this framework, belief cannot be explained in terms of a wholly rational attitude towards the world, but is rather considered as manifesting a "fiduciary act"[13] which lies beyond empirical observation and reason. It shall express a personal, non-rational attitude without which any manifestation of intelligence is impossible.[14] According to Polanyi, post-critical philosophy aims to overcome both medieval dogmatism and modern positivism by acknowledging the fiduciary programme, i.e. by reconsidering all cognitive phenomena against the background of a new theory which accounts for the personal, historical and socio-cultural preconditions of cognition:

> This then is our liberation from objectivism: to realize that we can voice our ultimate convictions only from within our convictions – from within the whole system of acceptances that are logically prior to any particular assertion of our own, prior to the holding of any particular piece of knowledge.[15]

Thus, Polanyi's argument against objectivism is mainly based upon the claim that knowledge cannot be detached from its roots and that an ideal of a wholly explicit and impersonal knowledge is therefore untenable. Rather, knowledge is said to be analyzable only within a "fiduciary framework"[16] which includes our adherence to a particular culture and tradition.

Knowledge Beyond Language

Polanyi's starting point for the construction of this new conception of knowledge is the claim that knowledge goes beyond language – "we can know more than we can tell".[17] Referring to perceptual knowledge as a

12 Polanyi 1958, p. vii.
13 Ibid., p. 294.
14 Cf. ibid., p. 264 f.
15 Ibid., p. 267.
16 Ibid., p. 266.
17 Polanyi 1966, p. 4.

paradigm case, he emphasizes the everyday presence and importance of this insight: if we try to explain how we are able to recognize faces, we might formulate criteria concerning the nose, the mouth, the eyes, etc. But our capacity to recognize the face concerned at a glance cannot be explained by what we have to tell about the face, how long and how detailed the list of criteria might be. In this case, and in many other kinds of capacities and skills, our knowledge can neither be formalized nor communicated by language or even by pictures.[18] According to Polanyi, findings of Gestalt psychology which mainly focus on perceptual knowledge serve as a main source for understanding our inexpressible, tacit knowledge. In particular, the distinction between a *focal awareness*, on the one hand, and a *subsidiary awareness*, on the other hand, is said to provide an explanatory basis. By referring to the skill of using tools, Polanyi shows how the two different modes of awareness are related to each other:

> When we use a hammer to drive in a nail, we attend to both nail and hammer, *but in a different way*. We *watch* the effect of our strokes on the nail and try to wield the hammer so as to hit the nail most effectively. When we bring down the hammer we do not feel that its handle has struck our palm but that its head has struck the nail. Yet in a sense we are certainly alert to the feelings in our palm and the fingers that hold the hammer. They guide us in handling it effectively, and the degree of attention that we give to the nail is given to the same extend but in a different way to these feelings. […] I have a *subsidiary awareness* of the feeling in the palm of my hand which is merged into my *focal awareness* of my driving in the nail.[19]

Thus, our tacit knowledge shall comprise both subsidiary and focal awareness; it is characterized by what Polanyi calls a "from-to relation":[20] in the case of face recognition, we attend *from* the features *to* the face and might be unable to specify the features – our knowledge of the situational particulars that guide our skill remains implicit.

The Triadic Structure of Knowledge

For Polanyi, the idea of a "from-to knowledge"[21] is inextricably bound to an act of "tacit integration"[22] by the knowing subject, i.e. the act of unifying the subsidiary elements into a whole. He claims that tacit knowledge is best be described by a triadic relation involving (1) the knower, (2) the

18 Cf. ibid., p. 5.
19 Polanyi 1958, p. 55; italics in original.
20 Polanyi and Prosch 1975, p. 34.
21 Polanyi 1966, p. 140.
22 Polanyi and Prosch 1975, pp. 62.

subsidiary particulars and (3) the focus of attention on which the subsidiaries bear on.[23] This triadic structure is said to reveal the fact that every act of knowledge is strongly dependent on the knower: her personal participation is simply required for transforming the particulars into a unified entity. Moreover, Polanyi emphasizes that tacit integrations cannot be reduced to any form of explicit integration like, for example, conscious inference.[24] The reason for this is that the main principle of Gestalt psychology – that the whole is more than the sum of its parts – becomes manifest in from-to knowledge. According to Polanyi, this process of emergence becomes apparent in our practical knowledge including scientific skills like using probes or measuring instruments, but also everyday abilities like riding a bicycle. However, it is important to note that for Polanyi tacit knowledge is not restricted to the realm of motor skills. He rather claims that even so-called "exact sciences" using highly abstract and formalized symbolic systems are based on tacit coefficients.[25] Since these sciences shall be regarded as mathematical formalisms with a bearing on experience, they also require the active, personal participation on the part of the scientist in establishing this bearing on experience. Polanyi claims, therefore, that even a mathematical or logical theory can only be understood by our tacit contributions to its formalism.[26] He insists that the idea of knowledge detached from the tacit dimension cannot be upheld, neither in science nor anywhere else. Although the use of language and symbols enables us to partly explicate and communicate our knowledge, a whole explication remains impossible: both the meaning of utterances and the establishment of other symbols or formalisms are dependent on skillful acts and practices and thus on personal coefficients which cannot be defined formally. Hence, Polanyi suggests replacing the idea of objective, impersonal epistemic processes with the idea of knowledge as a specific kind of art which is actively developed and applied.[27]

Moreover, for Polanyi the mentioned triadic structure of knowledge does not only comprise functional aspects, but also has profound im-

23 Cf. Polanyi 1969, pp. 181 ff.
24 Cf. Polanyi and Prosch 1975, p. 41.
25 Cf. Polanyi 1958, pp. 174 ff.
26 Cf. Polanyi 1958, p. 188.
27 Cf. ibid., pp. 3 ff.

plications for semantic, phenomenal and ontological dimensions.[28] First, from-to knowledge is said to be sense-giving, since the focal target brings out the *meaning* of the subsidiaries. Second, tacit knowing yields a phenomenological transformation because – once being integrated into a whole – things *feel and look different to us* from the way they did before. Finally, the tacit dimension of knowledge even embodies an ontological claim, namely, that the result of the act of knowing "is an aspect of reality which, as such, may yet reveal its truth in an inexhaustible range of unknown and perhaps still unthinkable ways".[29] According to Polanyi, the Gestalt principle that the whole is irreducible to its parts becomes manifest at all these four levels – the functional, the semantic, the phenomenal and the ontological.

In addition, Polanyi argues that the insight that all knowledge is grounded in tacit knowing and thus in personal conditions gives rise to reconsider the status of knowledge communication: in order to account for the tacit nature of knowing, learning via textbooks and lectures shall be replaced by more context-sensitive forms of learning which demand sympathy and mutual confidence between teachers and students.[30] Furthermore, he claims that centralizing scientific research undermines the importance of preserving traditional skills and knowledge. Although knowledge transmission should not be blind and uncritical, tradition and authority have to be regarded as important epistemic sources. As he puts it: "A society which wants to preserve a fund of personal knowledge must submit to tradition."[31]

To sum up: Polanyi introduces the concept of tacit knowledge against the background of rejecting the ideal of knowledge as wholly objective and impersonal which is represented by critical philosophy. By appealing to findings of Gestalt psychology, Polanyi emphasizes the emergent and tacit nature of knowledge and claims that knowledge cannot be detached from its roots in personal conditions. Moreover, for Polanyi the relevance of tacit knowledge is not restricted to epistemological questions. By re-

28 Cf. Polanyi and Prosch 1975, pp. 35 ff.
29 Polanyi 1969, p. 141.
30 Cf. Polanyi 1958, p. 53.
31 Ibid.

vealing semantical, phenomenal and ontological implications, it touches a wide range of descriptive and normative issues and leads to a considerable re-evaluation of science.

Polanyi and the Problem of Relativism

Throughout this section, I will discuss Polanyi's theory of knowledge in relation to the problem of relativism within epistemology and philosophy of science. In at least one passage of "Personal Knowledge", Polanyi rejects the interpretation of his theory as relativistic.[32] As we will see, this commitment is in accordance with his view that knowledge transcends mere subjective belief by being related to a mind-independent reality. However, regarding his theory as a full-fledged expression of anti-relativism seems to be unjustified for several reasons which I will outline below. In particular, I will argue that his treatment of scientific controversies and his appeal to "conceptual frameworks"[33] raises considerable difficulties: his epistemic and scientific realism threaten to be obscured by implicit tendencies towards relativism.

Transcending the Subjective-Objective Distinction

First of all, it is important to note that Polanyi claims that knowledge is, despite being personal, not merely *subjective*; it rather transcends the subjective-objective distinction. As he puts it:

> At all these points the act of knowing includes an appraisal; and this personal coefficient, which shapes all factual knowledge, bridges in doing so the disjunction between subjectivity and objectivity. It implies the claim that man can transcend his own subjectivity by striving passionately to fulfill his personal obligations to universal standards.[34]

Although being essentially personal, knowledge is said to be related to a "universal intent".[35] More particularly, Polanyi regards scientific discovery as a "contact with a hidden reality"[36] being based on the fact that the

32 Cf. Polanyi 1958, p. 316.
33 Ibid., p. 151.
34 Polanyi 1958, p. 17.
35 Ibid., p. 37.
36 Ibid., p. vii.

real manifests itself in "indefinite" and "unexpected" ways.[37] Thus, tacit knowledge is not merely a subjective experience nor can it be equated with *perspectival* knowledge. As we have seen before, the triad of knowledge entails an *ontological* shift, i.e. our knowledge does not simply represent the world how we feel it, but rather how it really is. As he puts it: "My definition of reality, as that which may yet inexhaustibly manifest itself, implies the presence of an *indeterminate* range of *anticipations* in any knowledge bearing on reality."[38]

Thus, Polanyi's theory involves *epistemic realism* in terms of the assumption of a reality existing independent of the knower and yet being accessible through the act of knowledge. Furthermore, he rejects the idea of a plurality of truths: "though every person may believe something different to be true, there is only one truth".[39] More specifically, he defines "true" as "expressing the asseveration of the sentence to which it refers",[40] interpreting his own theory of truth as closely akin to Tarski's correspondence theory of truth.[41] Thus, although our language shall be grounded in personal knowledge it nonetheless refers to reality and our statements can be classified as true or false.

However, at first glance Polanyi's epistemic realism, on the one hand, and his approach to knowledge, on the other hand, appear to be detached from each other. It is far from obvious how the relation between tacit knowledge and reality can be made intelligible. Polanyi addresses this problem by appealing to what he calls the "commitment situation".[42]

The Commitment Situation

Although Polanyi insists that there is only one truth, his approach to knowledge rules out any possibility to objectively verify or falsify epistemic statements in terms of rational resources. Since our knowledge is always tied to the tacit and thus to the personal dimension, we simply do not have appropriate means for a putative evaluation: "the establishment of truth becomes decisively dependent on a set of personal criteria of our

37 Cf. Polanyi 1969, p. 133.
38 Ibid., p. 141, italics in original.
39 Polanyi 1958, p. 314.
40 Ibid., p. 255.
41 Ibid.
42 Ibid., p. 302.

own which cannot be formally defined."[43] Thus, the inarticulate always "has the last word."[44] But though denying the possibility of objective criteria for verification or falsification, Polanyi holds that tacit knowledge can still be justified and therefore overcome the mere subjective dimension. The process of justification is strongly connected to what he calls the "commitment situation".[45] As he puts it: "I can speak of facts, knowledge, proof, reality, etc., within my commitment situation, for it is constituted by my search for facts, knowledge, proof, reality, etc. as binding on me."[46]

Thus, Polanyi regards commitment as "the only path for approaching the universally valid".[47] But how is such a commitment to be understood? Polanyi claims that a scientist, being engaged in the pursuit of truth, simply believes theories to be reliable and to manifest rationality due to the contact with reality.[48] He declares himself committed to believe in the universal validity of knowledge and thus transcends his own subjectivity "by striving passionately to fulfil his personal obligations to universal standards".[49] Intellectual passions charge scientific issues with emotions, making them attractive and affirming them as precious. Thus, the excitement of scientists making discoveries expresses intellectual passion; the belief that a theory is beautiful and precious for science is essentially connected to believe that theory to be true. According to Polanyi, the commitment situation in which our intellectual passions become manifest therefore reveals a *mutual correlation between the personal and the universal realm*. Like love to which it is thought to be akin the intellectual commitment saves personal knowledge form merely being subjective. As he puts it:

> […] I think we may distinguish between the personal in us, which actively enters into our commitments, and our subjective states, in which we merely endure our feelings. This distinction establishes the conception of the *personal*, which is neither subjective nor objective. In so far as the personal submits to requirements acknowledged by itself as independent of itself, it is not subjective; but in so far as it is an action guided by individual passions, it is not objective either.[50]

43 Polanyi 1958, p. 71.
44 Ibid.
45 Ibid., p. 302.
46 Ibid., p. 303.
47 Ibid.
48 Cf. ibid., p. 104.
49 Ibid., p. 17.
50 Ibid., p. 300, italics in orginal.

Thus, Polanyi's characterization of science can be interpreted as expressing *scientific realism*, more particularly, the claim that scientific theories aim to provide true descriptions of the world.[51] However, his suggestion to bridge the gap between the universal and the personal turns out to be problematic when confronted with the problem of scientific controversies.

Scientific Controversies

Polanyi directly addresses the problem of scientific controversies. Since he thinks of science as being driven by intellectual passions within the commitment situation, he considers such controversies to be unavoidable: besides its functional and heuristic function, for Polanyi an intellectual passion inherits a *persuasive* character. Once we aim at a positive response, we find ourselves in a tension if others ignore the view of reality we feel committed to. As he puts it:

> To the extent to which a discoverer has committed himself to a new vision of reality, he has separated himself from others who still think on the old lines. His persuasive passion spurs him now to cross this gap by converting everybody to his way of seeing things.[52]

By appealing to four paradigm cases – Freud's psychoanalysis, Eddington's a priori system of physics, Rhine's "Reach of the Mind", and Lysenko's environmental genetics – Polanyi argues that conflicting scientific systems are faced by what he calls a "logical gap".[53] Thus, a general problem of communication arises due to the strongly different conceptions of experience to which the conflicting systems are tied. Polanyi claims that any of the four mentioned authors has his own "conceptual framework"[54] which essentially influences the way to perceive and experience the world. As he puts it: "They think differently, speak a different language, live in a different world, and at least one of the two schools is excluded to this extent for the time being (whether rightly or wrongly) from the community of science."[55]

Thus, the persuasion of adherents of another system cannot be based upon rational grounds, but rather requires the winning of, what Polanyi

51 Such a definition of scientific realism can be found in van Fraassen 1980, pp. 8 f.
52 Polanyi 1958, p. 150.
53 Ibid.
54 Ibid., p. 151.
55 Ibid.

calls, an "intellectual sympathy"[56] on the side of our opponents. The acceptance of a new scientific framework is therefore akin to a "conversion"[57] and exceeds the power of rational arguments. Polanyi's appeal to logical gaps between conflicting systems of thought has often been interpreted as foreclosing the concept of *incommensurability* which was introduced simultaneously by Thomas S. Kuhn and Paul Feyerabend.[58] Given the similarities to Kuhn and Feyerabend, Polanyi's theory has often been regarded as relativist.[59] However, this interpretation would obviously undermine his expressed statement on relativism. In the following, I will focus on the question to which extent Polanyi's treatment of scientific controversies indeed reveals relativist tendencies.

Is Polanyi a Relativist?

"Relativism" does not refer to a single position, but rather to a whole family of different views. The general schema "*X is relative to Y*" can be the basis for various relativist accounts depending on what is referred to by *X* and *Y*. In the case of Polanyi, we have to analyze which *X* he thought to be related to his "conceptual frameworks". As we have seen before, Polanyi rejects *alethic* or *truth relativism*, i.e. the thesis that the truth-values of judgments or propositions are not absolute, but rather relative to certain parameters.[60] Moreover, by appealing to epistemic realism he denies the thesis that our knowledge is only relatively true or false. Thus, his claim that persons inhabiting different conceptual frameworks "live in different worlds" is obviously not meant literally, but rather expresses an analogy. It seems to involve, however, a certain *conceptual relativity*[61] since Polanyi endorses the claim that concepts we use to describe the world are relative to our own framework and that different frameworks cannot be translated into each other. It remains the question whether Polanyi's

56 Polanyi 1958, p. 151.
57 Ibid.
58 Cf. Kuhn 1962 and Feyerabend 1962. Kuhn admits that Polanyi had a strong impact on his theory. The mutual influences between Kuhn and Polanyi are rather complex. For a discussion see, for example, Moleski 2007 and Jacobs 2007.
59 Cf., e.g., Jacobs 2001.
60 For this definition of alethic or truth relativism see, e.g., Boghossian 2011, pp. 58 f.
61 Cf., e.g., Baghramian 2004, pp. 212 ff. and Taylor 2011.

position can be regarded as including *epistemic relativism*,[62] i.e. the thesis that facts about what belief is justified by a given piece of evidence may vary from community to community.[63] A closer look at his treatment of scientific controversies will help us to find an answer to this question.

Polanyi devotes a rather extensive part of "Personal Knowledge" to the discussion of Evans-Pritchard's findings of the behavior of the Azande, a tribe of North-Central Africa, which were published in 1937.[64] He claims that we can get a better understanding of systems of thoughts by examining how the Azande uphold their belief system against putative evidences. The behavior of the Azande is said to reveal three aspects of stability that characterize such systems. First, the Azande translate putative doubts brought up against their theory into their own language and thus transforms it to a support of the own structure of their belief system (*aspect of circularity*). Second, the Azande automatically expand the circle in which the belief system operates, i.e. they extend the means of interpreting various eventualities. Thus, an epicyclical structure of the belief system is revealed which is, according to Polanyi, characteristic of conceptual frameworks (*aspect of self-expansion*). Finally, the Azande deny the ground for any rival theory since they reject relevant objections. Whereas experiences which support the system could be brought forward one by one, a new conception, e.g. that of natural causation, could be established only by a vast number of relevant instances, and it is difficult for the Azande even to understand the relevance of these instances (*aspect of suppressed nucleation*). According to Polanyi, all these three aspects protect an existing system of belief against doubts arising from any putative piece of evidence. He admits that, although we are convinced that Azande beliefs are wrong, our own system of thought has significant similarities to their system:

> We do not share the beliefs of Azande in the power of poison-oracles, and we reject a great many of their other beliefs, discarding mystical conceptions and replacing them by naturalistic explanations. But we may yet deny that our rejection of Zande

62 The notion of "epistemic relativism" is sometimes used in a wider sense than here and involves also alethic relativism. See, for example, Baghramian 2004, pp. 180 ff.

63 Such a definition of epistemic relativism can be found in Boghossian 2006, pp. 58 f.

64 Cf. Polanyi 1958, pp. 287 ff.

> superstitions is the outcome of any general principle of doubt. For the stability of the naturalistic system which we currently accept instead rests on the same logical structure.[65]

However, admitting structural similarities between the two systems would not lead automatically to *epistemic relativism*. Polanyi's view could be saved from being relativistic by invoking a rational criterion which allows us to evaluate the two systems. But Polanyi does not offer any indication as to what these rational means could be. Choices between frameworks can, according to Polanyi, only be explained in terms of *non-rational* resources: it is the "fiduciary act"[66] arising from our commitment situation which ties us to a certain conceptual framework. Indeed he claims that we are able to expose certain statements as "unscientific"[67] insofar as those merely resemble unfounded guesses by not being embedded into a sufficiently coherent system of thought. But if different coherent belief systems are confronted with each other, we do not have any rational resources to decide which one to chose. To reiterate, Polanyi tries to avoid relativism by invoking a non-rational, fiduciary criterion: we trust in the rationality of ourselves and other human beings sharing our scientific community. We simply believe that we are in contact with the hidden reality that manifests itself in various ways. We might be *personally* unpersuaded by the Azande witchcraft, but we are not able to provide rational criteria on which a general doubt against their belief system can be based. Thus, Polanyi's theory indeed threatens to include epistemic relativism and to undermine his expressed commitment to anti-relativism. Since Polanyi regards any knowledge as tacit knowledge or as being grounded in the tacit dimension, epistemic justification seems to be mainly restricted to what Ludwig Wittgenstein called "forms of life" („Lebensformen")[68] and Nelson Goodman "ways of worldmaking".[69]

The fact that Polanyi's treatment of scientific controversies serves as a basis for supporting epistemic relativism becomes even clearer in the light of recent debates within the sociology of knowledge. Though Polanyi is first and foremost interested in the tacit and thus in the personal

65 Polanyi 1958, p. 292.
66 Ibid., p. 294.
67 Ibid., p. 155.
68 Cf. Philosophical Investigations §. 19 in Wittgenstein 1984, pp. 245 f.
69 Cf. Goodman 1978.

dimension of knowledge, the claim that knowledge is strongly dependent on social conditions should be regarded as an important side effect of his theory. In this respect, his theory can be interpreted as foreclosing certain arguments of the sociology of knowledge.[70] As we have seen, Polanyi's discussion of Azande belief system and conceptual frameworks could indeed be regarded as supporting the thesis that there are no universal norms of truth or rationality. Within the so-called "strong programme", Barry Barnes and David Bloor prominently invoke the thesis that "there are no context-free or super-cultural norms of rationality"[71] to endorse epistemic relativism.[72]

However, Polanyi would have certainly been dissatisfied with such an interpretation of his theory. It conflicts not only with his expressed commitment to anti-relativism, but also threatens to render his theory inconsistent. Against the background of an epistemic relativism the endorsement of epistemic and scientific realism seems to be implausible. The assumption of an objective reality knowable to us and of a unique truth as the goal of science threatens to become unintelligible if justification of what we assume to know cannot be regarded as objective.

I think that Polanyi could choose among three strategies for overcoming the difficulties shown above. First, he could directly address the position of relativism and argue that this position is inconsistent and, therefore, untenable. Second, he could abandon his idea of conceptual framework being separated by logical gaps. He would have to argue that although at first glance belief systems of different community may strongly vary a closer look at the structure of these systems could reveal that neither essentially different frameworks nor a logical gaps exist and that the thesis of conceptual or epistemic relativity can be refuted. Third, Polanyi could admit that his theory endorses both conceptual and epistemic relativism, but deny that this renders his theory inconsistent. In this case, he would have to provide some convincing argument for the thesis that his relativist tendencies can be brought in line with his epistemic and scientific realism.[73] However, all these strategies would require a lot more argumentative work than is already given in Polanyi's work.

70 Cf., e.g., Fuchs 1993 and Shapin 1995.
71 Barnes and Bloor 1982, p. 27.
72 Cf. Bloor 2011 and Siegel 2011, pp. 205 ff.
73 Cf. Faulkner 2004.

The Tacit-Explicit Distinction Nowadays

As we have seen, Polanyi's approach to knowledge faces severe difficulties when confronted with the problem of relativism. However, these difficulties do not arise for each concept of tacit knowledge. In this section, I will briefly discuss some of the major similarities and differences between Polanyi's account and recent theories.

The polar opposites of tacit or implicit, on the one hand, and explicit, on the other hand, are widely used in various sciences. Furthermore, numerous further distinctions like unconscious/conscious, procedural/declarative, or knowing how/knowing that are often treated (quasi-)synonymously. A detailed analysis of the meaning of these concepts would take too much space and is beyond the scope of this article. I will rather highlight four points concerning the relation between the recent application of these concepts and Polanyi's theory of knowledge in order to show that Polanyi's intentions are often not taken seriously.

First, during the last decades the so-called "practice turn" can be observed in various disciplines that cover the social and cultural sciences as well as the cognitive sciences.[74] According to this new research programme, practices are the key to understand different phenomena like cognition, perception, emotion and social behavior. Polanyi's theory can be interpreted as giving main impulses to the shift of attention from theoretical, factual knowledge to cognitive practices.[75]

Second, it is important to note that Polanyi, though treating tacit and explicit knowledge as being opposed to each other, does not claim for a *distinction* of both kinds of knowledge. As said before, he rather holds that *all* knowledge is grounded in tacit knowledge and that explicit knowledge as its opponent is only an unrealizable and misleading ideal:

> Now we see *tacit knowledge* opposed to *explicit knowledge*; but these two are not sharply divided. While tacit knowledge can be possessed by itself, explicit knowledge must rely on being tacitly understood and applied. Hence all knowledge is *either tacit* or *rooted in tacit knowledge*. A *wholly* explicit knowledge is unthinkable.[76]

Thus, by introducing the concept of tacit knowledge Polanyi aims at es-

74 Cf. Schatzki, Knorr Cetina, and von Savigny 2001.
75 Cf. Collins 2001.
76 Polanyi 1969, p. 144; italics in original.

tablishing a new point of view from which epistemological and scientific issues have to be regarded. As we have seen before, tacit knowledge is not restricted to practical skills and capacity, but rather includes all forms of knowledge.

Third, even though tacit or implicit forms of knowledge are usually regarded as actually not represented by language some authors claim that they are still *expressible* by language or *formalizable* through symbols. For example, Jerry Fodor defines tacit knowledge in the following way:

> On the present account, 'tacit knowledge' is, *inter alia*, a theoretical term in psychology. The term is introduced by reference to the computational operations of some optimal simulation of an organism, but the relation that the term designates presumably holds between the organism itself and some proposition, rule, maxim, or technique.[77]

However, by being bound to the irreducible emergent integration by the knower Polanyi's tacit knowledge is *in principle* neither explicable nor formalizable. Since the claim that the whole is more than the sum of its parts becomes manifest in any act of knowledge, Polanyi's approach to knowledge strongly differs from that in psychology described by Fodor. Within Polanyi's framework, tacit knowledge simply cannot be understood in analogy to explicitly represented knowledge. Moreover, though in recent debates it is often referred to his famous statement that we can know more than we can tell, the implications Polanyi derives from this statement have mostly been neglected. Thus, it is usually ignored that for Polanyi the tacit is inherent to the personal and, therefore, to the rejection of any wholly objective knowledge in general.

Fourth, despite the shown differences between Polanyi's account of knowledge and recent theories, he certainly would have been pleased by some developments within the sciences of cognition. Especially research programs like "embodied" and "embedded cognition"[78] share some of Polanyi's ideas concerning knowledge and skills: he consistently emphasizes the bodily and environmental conditions underlying epistemological processes. According to him, the tacit dimension of knowledge requires the knower's empathic "indwelling".[79] As he puts it:

77 Fodor 1968, p. 638; italics in original.
78 Cf., e.g., Varela, Thompson and Rosch 1992 and Clark 1997. For Polanyi's impact on the embodied cognition approach see also Yu 2008.
79 Cf. Polanyi and Prosch 1975, pp. 37 ff.

> Our subsidiary awareness of tools and probes can be regarded now as the act of making them form a part of our own body. […] We may test the tool for its effectiveness or the probe for its suitability, e.g. in discovering the hidden details of a cavity, but the tool and the probe can never lie in the field of these operations; they remain necessarily on our side of it, forming part of ourselves, the operating persons. We pour ourselves out into them and assimilate them as parts of our own existence. We accept them existentially by dwelling in them.[80]

Our body is said to be the "ultimate instrument of all our external knowledge, whether intellectual or practical."[81] Moreover, Polanyi's theory may be interpreted as foreclosing current approaches in psychology and cognitive science aiming at including phenomenological perspectives on knowledge.[82] As we have seen, phenomenology is considered as one of the four aspects on which tacit knowledge bears on. Furthermore, for Polanyi the phenomenological perspective serves as an explanatory tool for understanding the inherent structure of knowledge, especially in practical abilities and sensorimotor skills.

In sum, many current approaches to cognition refer to Polanyi's insight that cognition cannot be fully reduced to explicit knowledge. Given the shown differences between Polanyi's and recent theories of tacit knowledge, it becomes clear that the appeal to the tacit dimension does not automatically yield severe problems with regard to relativism. However, the discussion of Polanyi's view reveals that any theory of knowledge threatens to include relativist tendencies if it does not clarify the relations between tacit knowledge, on the one hand, and the possibility of objective epistemic justification, on the other hand.

Conclusion

By rejecting the idea that everything we know can be expressed in terms of language or symbols, Polanyi develops a new approach to knowledge that emphasizes the tacit and personal character of cognition. However, confronting Polanyi with the problem of relativism reveals a tension in his theory between his epistemic and alethic realism and his discussion of scientific controversies.

80 Polanyi 1958, p. 59.
81 Polanyi 1966, p. 15.
82 See, for example, Gallagher and Zahavi 2008. Relations between Polanyi's theory of knowledge and Merleau-Ponty's phenomenology are discussed in Grene 1966.

To conclude, the discussion of Polanyi's approach to knowledge in relation to the problem of relativism reveals some core problems we face if we aim to appropriately analyze human knowledge: if we admit that our knowledge is grounded in non-explicable, tacit epistemic practices, how could epistemic relativism be avoided? And how could the ideas of epistemic and alethic realism in principle be reconciled with such a tacit framework?

Polanyi's merit was to bring into focus the tacit conditions of knowledge and to account for the roots of our knowledge in personal experience, tradition and society. Even though Polanyi's theory is not fully adopted within recent research on cognition, his approach to knowledge reveals some important questions that need to be addressed.

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