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REALISM, RELATIVISM, AND CONSTRUCTIVISM*

ABSTRACT. This paper gives a critical evaluation of the philosophical presuppositions and implications of two current schools in the sociology of knowledge: the Strong Programme of Bloor and Barnes; and the Constructivism of Latour and Knorr-Cetina. Bloor's arguments for his externalist symmetry thesis (i.e., scientific beliefs must always be explained by social factors) are found to be incoherent or inconclusive. At best, they suggest a Weak Programme of the sociology of science: when theoretical preferences in a scientific community, *SC*, are first internally explained by appealing to the evidence, *e*, and the standards or values, *V*, accepted in *SC*, then a sociologist may sometimes step in to explain why *e* and *V* were accepted in *SC*. Latour's story about the 'social construction' of facts in scientific laboratories is found to be misleading or incredible. The idea that scientific reality is an artifact turns out to have some interesting affinities with classical pragmatism, instrumentalism, phenomenology, and internal realism. However, the constructivist account of theoretical entities in terms of negotiation and social consensus is less plausible than the alternative realist story which explains consensus by the pre-existence of mind-independent real entities. The author concludes that critical scientific realism, developed with the concept of truthlikeness, is compatible with the thesis that scientific beliefs or knowledge claims may be relative to various types of cognitive and practical interests. However, the realist denies, with good reasons, the stronger type of relativism which takes reality and truth to be relative to persons, groups, or social interests.

1. PHILOSOPHY MEETS SOCIOLOGY – OR DOES IT?

In January 1977, the Academy of Finland organised an international seminar on science studies in Espoo, near Helsinki. At that time I was a 'young angry realist'. (Today, I am still a realist.) In my doctoral dissertation, I had mobilised inductive logic – a tool developed originally within narrow empiricism – to give support to the realist interpretation of theoretical terms and thereby to refute instrumentalism.[1] Further, I had just written my first paper on the concept of truthlikeness which was intended to save fallibilist critical realism both from naive realism and from Feyerabendian scepticism, relativism, and anarchism.[2]

At the Espoo seminar, Michael Mulkay read a paper on "a comprehensive framework for the analysis of scientific development". Mulkay suggested that there is nothing epistemologically unique in science: a sociologist of science should not take for granted that "scientific knowl-

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edge is valid in a way which other forms of knowledge are not". Scientists are not committed to a distinctive set of non-political values, Mulkay argued; rather, they have a diverse set of loose norm formulations from which they can flexibly select in accordance with the particular social context and in support of their own interests.[3]

I remember how amazed I was to hear a distinguished sociologist of science speak with what sounded to me like the voice of Paul Feyerabend. Mulkay not only rejected the realist picture, where the critical attitude of scientists and their ever sharper methods of inquiry at least in the long run guarantee that the results of research constitute a progressive body of fallible knowledge, he also argued that this realist conception is merely an 'ideology' which should be replaced by a description of scientists as political opportunists.

Mulkay, thus, seemed to claim that scientists in fact *are* just like Feyerabend tells they *ought* to be.[4] In my comment, I suggested that Feyerabend's 'anything goes' is also an ideology which might be wrong even if Mulkay's description of the actual behaviour of scientists was right. Therefore, it could still be rational to defend (as I indeed did) the critical realist 'ethos' of science even if the practice of science sometimes violated it – in the same way that legal and moral norms may remain valid and serve important functions in society in spite of occasional violations against them.

This proposal presupposes that a legitimate distinction can be made between the description and explanation of scientific activities, on the one hand, and the normative study of the rational aims and methods of science, on the other. It indeed used to be quite a common assumption that the former task belonged to the empirical fields of science studies (history and sociology of science), while the latter task belonged to methodology and philosophy. Thus, a historian and a sociologist may *describe* the actual behaviour of the scientists and the 'ideological' norms and values that guide the members of the scientific community (see Merton, 1973), while a philosopher wishes to *prescribe* how science ought to be done or what rational behaviour in science might be.

Today we are less confident of such a neat division of labour. It is challenged by those philosophers of science who – allying their forces with historians, evolutionary biologists, anthropologists, psychologists, and cognitive scientists – have turned their attention to the 'naturalised' study of human cognitive faculties, the styles of discourse and the patterns of inference and argumentation in real-life scientific communi-

ties, and the behaviour of scientists in actual situations of theory choice.[5]

This programme of *philosophical naturalism* is characterised by the assumption that science is the paradigm of human rationality. Thereby it tries to reduce the normative *ought* to the historical *is*. Lakatos (1976) proposed that 'theories of rationality' could be tested by their ability to prove the rationality of as many historical episodes of science as possible. Larry Laudan with his collaborators have developed a series of historical case studies to put theories of scientific change into 'empirical tests'.[6] Giere (1988) claims that the "overwhelming empirical evidence that no Bayesian model fits the thoughts or actions of real scientists" puts an end to the debate whether scientists ought to be Bayesians.

Even though I am ready to acknowledge that the empirical study of science may give us extremely important information, it cannot tell the whole story about scientific rationality. Scientists learn to do their work from their teachers, colleagues, and textbooks, which in turn are – explicitly or implicitly – influenced by methodological and philosophical theories about science. This fact makes the 'tests' of methodological norms circular in a vicious way.[7] To avoid this circularity, we should find scientists who have never learned anything about methodology or heard anything about philosophy of science. But how could anyone seriously suggest that the scientists in a pure state of ignorance about scientific method (like the tribes of 'primitive cultures' studied by the anthropologists seeking true unspoiled humanity) would be the best source of information about scientific rationality?

Instead of philosophical naturalism, I concentrate in this paper on another brand of naturalism, the so-called *sociology of knowledge*. Even if these sociological programmes are also working carefully with historical case studies, their approach to rationality is diametrically opposed to the philosophical naturalists': the sociologists refuse to presuppose that scientific beliefs, if compared to beliefs within other human communities or 'tribes', have any special relation to reason, truth, or reality.

This brings me back to my brief encounter with Mulkay. It seems to me that the 'strong' programmes of the sociology of knowledge, in spite of often pretending to be *non*philosophical or even *anti*philosophical, are in fact heavily laden with philosophical assumptions – and also draw very strong philosophical conclusions. As such, I feel the situation where philosophers become historians, sociologists become philoso-

phers, etc., may be highly stimulating and should be welcomed by all parties. But it also means that the often concealed philosophical prejudices of the sociologists of knowledge should be made explicit and put into scrutiny. It is in this friendly but merciless spirit that I discuss here the recent work of some sociologists – as if they were any other fellow philosophers.

2. THE EDINBURGH PROGRAMME: STRONG OR WRONG?

The Edinburgh school (David Bloor, Barry Barnes, Steven Shapin) has been the most influential of the sociological approaches to scientific knowledge. The theoretical principles of the Strong Programme were formulated in Bloor's *Knowledge and Social Imagery* (1976).[8] Since then they have been extensively and effectively criticised, among others, by Martin Hollis, Steven Lukes, Larry Laudan, and J. R. Brown.[9]

The Strong Programme aims to give a scientific explanation of "the very content and nature of knowledge" (Bloor, 1976, p. 1). Here knowledge means, instead of "true belief", whatever the scientists collectively "take to be knowledge" (ibid., p. 2). The principle of *Causality* says that the explanation of scientific beliefs should use the "same causal idiom" as in any other science (ibid., p. 3). *Impartiality* requires that both true and false, or both rational and irrational, beliefs should be causally explained, and *Symmetry* demands that both kinds of beliefs should be explained by the same types of factors (ibid., pp. 4–5). Finally, *Reflexivity* indicates that the programme should apply to itself.

It is clear that the Strong Programme is consciously based upon very heavy philosophical assumptions. Bloor's book is indeed advertised in the back cover as "a forceful combination of materialism, relativism and scientism". He gives no concessions to the idea that there might be methodological differences between the natural and social sciences: "the search for laws and theories in the sociology of science is absolutely identical in its procedure with that of any other science" (ibid., p. 17). Moreover, "in the main science is causal, theoretical, value-neutral, often reductionist, to an extent empiricist, and ultimately materialistic like common sense" (ibid., p. 141).

However, in spite of Bloor's methodological monism and his principle of Reflexivity, there is a dramatic difference in his descriptions of science on two levels. As the *method* of the sociologist of science,

science for Bloor satisfies very strict – and many of us would say, old-fashioned – positivist, empiricist, inductivist,[10] and causalist principles. But as the *object* of sociological study, science for Bloor is 'a social phenomenon' whose methods, results, and objectivity are relative to social interests and causally influenced by social factors. If this is not a direct contradiction, at least it gives a highly perplexing picture of the Edinburgh school.

Bloor's critics have claimed that his principles are either empty, trivially true, or wrong. Indeed, most of his opponents would accept the idea that scientific beliefs – or, perhaps, rather theoretical preferences and choices in science[11] – can be explained. For example, the explanation of a belief may refer to its reasons, i.e., to other beliefs from which it has been derived:

(1) Scientific community C believes that p, because C has received the information that q and C thinks that q entails or supports p.

As a special case of (1), where q is identical with p, C's belief that p is explained by the fact that C has received the information that p from a source C accepts (e.g., observation or experiment). At least the fashionable causal theories of knowledge[12] are ready to construe the connective 'because' in (1) as expressing a causal relation. As false sentences can be correctly derived from false premises, or incorrectly from true premises, model (1) can impartially be applied both to true and false beliefs. A radical *internalist* might then claim that *all* scientific beliefs are causally explainable by their reasons, so that his or her meta-methodological position would satisfy all of Bloor's four principles of Causality, Impartiality, Symmetry, and Reflexivity.

This argument shows that Bloor's extreme *externalism* – the demand that the sociology of science explains beliefs always by social factors – does not follow from the basic principles of the Strong Programme but, rather, is an addition to its artillery. This externalist thesis cannot be proved by case studies, since they at best show that social factors sometimes play some role in explaining the success of some idea in a cultural or social climate. And the whole thesis in its strong form will be watered down, if one follows Bloor's defensive strategy of claiming that even explanatory reasons are in some vague sense 'social'.[13]

Bloor argues that "epistemic factors" are really "social factors", since "the link between premise and conclusion is socially constituted".

Here Bloor is not denying the possibility of internalist explanations of the form (1), but insists that we need social and historical factors to explain why the community C took reason q to support p or applied q in a particular way. This might be called the *Weak Programme* of the sociology of knowledge, since it suggests that a sociologist may step to explain scientific beliefs only after a philosopher has given a rational explanation of them.

I think this Weak Programme may legitimately be applied to partially explain the reasoning and the weighting of evidence by scientists in many special cases. As Papineau (1988) points out, this partial influence of social factors does not imply that scientific practice is not generally reliable (in Goldman's sense) for generating true or truthlike theories. Furthermore, the suggestion that the link between a reason q and a belief p *always* needs a social explanation (in terms of extra-scientific, political, religious, etc., factors) is not plausible, since deductive and mathematical reasoning as (per *definitionem*) necessarily truth-preserving is compelling (*pace* Bloor, 1976).

The situation becomes even worse in the *Empirical Programme of Relativism* of Harry Collins. This approach, which has also led to a number of interesting historical case studies, explicitly adopts a relativistic position where "the natural world has small or nonexistent role in the construction of scientific knowledge".[14] But if this is the case, *why* should we have any reason to believe that the empirical results of the sociologists of science, or inductions from their results, have any content which is a contribution of the social world of science that they study?

At this stage it seems fair to agree with Laudan (1982) that the Symmetry requirement is an instance of questionable 'premature dogmatizing'. Any comprehensive framework for science studies should acknowledge that the opinions of the scientific communities may depend on a variety of different types of factors – among them 'internal' reasons, arguments, prejudices, mistakes, persuasive communication, and 'external' social influences. Case studies should show what factors in fact were active and what their interplay really was.[15] The task for a theory of science would be to provide a plausible model which shows how and where external factors may play a role in scientific practice.

Bloor and Barnes have recognised the need to give independent philosophical support for their externalism. In *Knowledge and Social Imagery* (1976) Bloor argued that the necessity characteristic to logical and mathematical thought is due to socially relative practices and 'nego-

tiations'. Deriving inspiration from Kuhn and Hesse, Bloor and Barnes have both suggested that social factors influence science primarily through the 'conventional character' of language, conceptual classifications, and (Collins adds) inductions.[16] In *Wittgenstein: A Social Theory of Knowledge* (1983) Bloor further argues that our thinking or mental states are socially constructed.

While these views are philosophically interesting and would deserve further discussion, it will suffice here to note that this strategy of argumentation is not likely to achieve a victory for the Strong Programme. Human languages do have an important 'conventional' element: they are 'social constructions', the meanings of words are based upon conventions accepted and sustained in the linguistic community, and the choice of conceptual frameworks reflects human interests or social purposes. This is a fairly standard view of language among philosophers – from Peirce to Wittgenstein, Carnap, and Popper. Many philosophers of mind would also accept that man is a social being who in his practices is always conditioned by the culture that he also transforms. It does not follow that truth *about* languages (or about other social constructions in Popper's World 3), or truth expressible *in* these languages, is somehow relative to social interests.[17] And it does not follow that particular beliefs formulated in scientific languages have to be explained by social factors.

These radical conclusions about truth and beliefs would need stronger premises than the conventional character of human languages and the social nature of human minds. We shall now turn to the programme of 'constructivism' that has attempted to establish such premises.

3. SOCIAL CONSTRUCTIVISM

Among the sociologists of science, the *constructivist programme* appears to be currently the most popular approach.[18] Its classical works are Bruno Latour and Steve Woolgar's *Laboratory Life: The Social Construction of Scientific Facts* (1979) and Karin Knorr-Cetina's *The Manufacture of Knowledge* (1981).

The constructivists are interested in the actual production of scientific knowledge within research groups working in laboratories. As their method they typically use participant observation by an 'outsider' in the laboratory witnessing the strange behaviour of the 'tribe' of scientists. This "ethnographic study of scientific work" (Knorr-Cetina, 1983)

thus attempts to approach science in the same way that an anthropologist investigates foreign cultures. *Laboratory Life* in particular is an exciting and lively description of Latour's adventures in the wonderland of R. Guillemin's biochemical laboratory at La Jolla.

The anthropological study of the everyday laboratory practices of science may give very interesting new perspectives on the construction of scientific beliefs or knowledge in the scientific community. The constructivist programme wishes to interpret this process more radically as a construction of scientific facts, theoretical entities, and even reality.[19] They further think that this interpretation "makes unnecessary the use of ad hoc epistemological explanations" (Latour and Woolgar, 1986, p. 166). In his 1986 postscript, Latour proposes "a ten-year moratorium on cognitive explanations of science" (ibid., p. 280).

However, it is again clear that the constructivist programme is committed to very strong philosophical assumptions. Already the decision to employ observation by an outsider, who has freed his mind from all prejudices and preconceptions about science, is laden with the rhetoric of naive Baconian inductivism, positivism, and behaviourism.

Latour starts from an "agnostic position":

> There are, as far as we know, no a priori reasons for supposing that scientists' practice is any more rational than that of outsiders. (Ibid., p. 30)

But when the story goes on, Latour urges that the *falsity* of this supposition follows from his antiepistemological starting point:

> The notion that there is something special about science, something peculiar or mysterious which materialist and constructivist explanations can never grasp . . . will remain as long as the idea lingers that there is some peculiar thinking process in the scientist's mind. (Ibid., p. 168)

This idea, which would save the ad hoc epistemological concepts "we have tried to rid ourselves", is "inconsistent with our argument so far". Thus, Latour's story involves a fallacious slide from,

I don't assume that science is rational,

first to,

I assume that science is not rational,

and finally to,

I prove that science is not rational.

The most central idea of the constructivist programme is expressed by the claim that scientific reality is an artifact, created by selective, contextual, and socially situated scientific laboratory practices and negotiations.

> The constructivist interpretation is opposed to the conception of scientific investigation as descriptive, a conception which locates the problem of facticity in the relation between the products of science and an external nature. (Knorr-Cetina, 1983, pp. 118–19)

As "scientific objects are produced in the laboratory",

> it is the thrust of the constructivist conception to conceive of scientific reality as progressively emerging out of indeterminacy and (self-referential) constructive operations, without assuming it to match any pre-existing order of the real. (Ibid., p. 135)

In the Latour–Woolgar story, Guillemin's laboratory used two hundred tons of pig brains to synthesize one milligram of Thyrotropin Releasing Factor (TRH) – a substance in the hypothalamus that releases a hormone, thyrotropin, from the pituitary.[20] For this work, Guillemin received (with Schally) a Nobel Prize in 1977.

Latour interprets TRH as an artificial laboratory construction and the fact that TRH is Pyro-Glu-His-Pro-NH_2 as a social construction. Scientific facts are created by a consensus, or by the acceptance of a statement, which is preceded by experiments, measurements, inscriptions, debates, and negotiations.

> We do not wish to say that facts do not exist nor that there is no such thing as reality. In this simple sense our position is not relativist. Our point is that "out-there-ness" is the *consequence* of scientific work rather than its *cause*. (Latour and Woolgar, 1986, p. 180)

4. PRAGMATISM AND INTERNAL REALISM

Knorr-Cetina (1983) is aware that her position has affinities with Nelson Goodman's (1978) ideas about 'world-making'. The same observation about Latour is made by Hacking (1988).

Besides the neo-pragmatist Goodman, it would be easy to find similarities between the constructivist manifestos and older pragmatists – especially F. C. S. Schiller's 'humanism' (the world, objects, and truth are man-made relative to local interests) and John Dewey's 'instrumentalism' (language, concepts, and theories are social products with instrumental value). Further connections to phenomenology (the constitution

or 'social construction' of the everyday life-world[21]) and to some trends of Western Marxism, such as 'practical materialism' or 'praxis philosophy'[22] (the primacy of practice over theory, the attack against representational theories of knowledge), are also evident.

An interesting comparison can also be made to Hilary Putnam's (1981) *internal realism*, which can be viewed as a combination of Kantianism and pragmatism – or as 'transcendental nominalism'.[23] Putnam is even more radical than the constructivist sociologists in his global claim that the world is 'carved into pieces' only through human languages, so that in a sense all objects and properties are man-made. In denying the ontological Myth of the Given, or the existence of a 'ready-made world' (cf. Tuomela, 1985), Putnam takes the world to be a construction of the scientific community. But his main emphasis here is in the linguistic and epistemic practices, such as the creation of concepts and theories, rather than in those material practices of laboratory experiments and measurements that the constructivists have discussed.

Further, Putnam's internal realism tries to avoid the trap of relativism by characterising truth in terms of ideal acceptability, since such epistemically ideal conditions can be hoped to fix truth in a unique manner. Similarly, Tuomela's (1985) *scientia mensura* allows science to decide ontology only via the ultimately best explanations of the Peircean limit science.[24] In contrast, the constructivist programme is relativist in the sense that it is interested in the 'construction of scientific reality' in highly localised, contextually defined, finite laboratory communities.[25]

A general problem with internal realism is that, taken literally, it makes existence and truth dependent upon ideal conditions which have not been, and perhaps never will be, realised.[26] How could there be anything real or true *now* or at any time before the scientific community has reached its Peircean limit? Even the more down-to-earth versions of practical materialism face the problem, familiar from the work of young Georg Lukács (1971), that while human actions with material and social practices are assumed to exist, nature (Popper's World 1) as an independent ontological category disappears. This view leads either to philosophical idealism (there is no mind-independent world) or World 1 is regarded as a 'noumenal jam', an existing Nothingness, before man carves it with his concepts. Both alternatives contradict well-established scientific facts about the existence of the world and its long history before the evolutionary appearance of *Homo sapiens*.

In contrast, a critical realist admits that there is a sphere of man-made material artifacts, social institutions, and abstract cultural entities (Popper's World 3), but he or she regards the physical World 1 as ontologically and temporally primary to such products of historical evolution as human consciousness (in World 2) and culture (in World 3). In these terms, radical constructivism can be viewed as an implausible attempt to show that even physical objects and properties are cultural, i.e., that World 1 is reducible to World 3.[27]

As Latour has not formulated any general version of his ontology, it is not possible to locate him precisely in a co-ordinate system of contemporary ontology and epistemology. But my guess is that the problems outlined above would be serious burdens for him (and for Knorr-Cetina) if his somewhat implicit philosophical leaning would be explicitly developed. Evidence for this claim can be received from the consideration of a more restricted issue: Latour's account of TRH as compared to rival views about the status of theoretical terms.

5. THEORETICAL ENTITIES AND MAKER'S KNOWLEDGE

According to the *instrumentalist* view, theoretical terms are merely linguistic, uninterpreted tools for systematizing observations and making predictions. The so-called theoretical entities are not assumed to exist in reality: science investigates only the 'empirical world', or what is observable in nature and laboratories. A 'fictionalist' version of instrumentalism regards theoretical entities as 'useful fictions' – allowing us to say in quotes that theoretical terms 'refer' to such fictions. In practice close to instrumentalism, van Fraassen's 'constructive empiricism'[28] regards the possible reference and truth of the theoretical language as irrelevant to the aims of inquiry.

On the other hand, *scientific realism* regards theoretical terms as attempts to refer to pre-existing, previously unobserved, and perhaps in principle unobservable things and properties. A successful theory should be true and informative: its existential claims should match entities existing in reality, and its universal or probabilistic laws should give a correct description of the regularities in the behaviour of these entities. If a theory is well-confirmed in tests or agrees sufficiently closely with observations, a realist regards this as a good reason for tentatively claiming that these two aims have been achieved – at least to some extent. In such cases, it is reasonable to conjecturally appraise

the theory (with its ontology and laws) to be probably true, approximately true, or truthlike.[29]

A position, which is half way between instrumentalism and realism, accepts the existence of theoretical entities that enter into causal interactions, but denies a realistic interpretation of the fundamental laws of theories. This view, recently defended by Hacking (1983), Cartwright (1983), Harré (1986), and Giere (1988), is sometimes called *entity realism*.

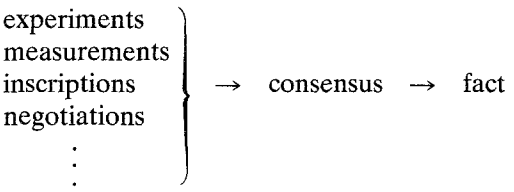
Another position between instrumentalism and realism is represented by those philosophers who think that theoretical entities are 'constructions'. For example, phenomenalists (like the young Carnap) claimed that theoretical terms can be explicitly defined by means of observational ones, so that theoretical entities are (to use Russell's phrase) logical constructions out of sense data. A mentalist interpretation of this stance, favoured by some 'constructivist' philosophers of mathematics, regards theoretical entities as constructs in human mind. Finally, theoretical entities may be viewed as results of 'material constructions', i.e., as experimentally produced artifacts. Examples of these artifacts include radioactive substances and synthetic materials produced in physical and chemical laboratories.

The incommensurability view, inspired by Kuhn's and Feyerabend's holistic theory of meaning, can be regarded as the doctrine that each theory defines those 'theoretical constructs' it speaks about. For example, each theory of the electron speaks about its own 'electrons' which satisfy its axioms. But when theories change, the postulated ontologies are also radically ruptured. To speak of electrons independently of any theory does not make any sense in this view, which also represents a relativist position about theoretical entities.

A realist instead interprets theories of the electron as speaking of the same unknown entities, identified indirectly through their causal role and influences, so that successive theories may give increasingly accurate descriptions of the nature of these things.[30]

Latour's account of TRH is not identical with any of the above views. The constructivist sociologists of science agree with instrumentalism in taking the laboratory phenomena as the *object* of investigation in the natural sciences – instead of treating it as *evidence* for theoretical claims about the independent world outside the laboratory, as realism does. On the other hand, Latour treats TRH as an artificial construction of Guillemin's laboratory team, but the process he describes is not an

instance of a logical, mental, or material construction. For example, the team at La Jolla did not simply bring about the substance TRH – in the same causal sense as, e.g., neutrinos can be 'created' in an accelerator by bombing heavy nucleii by α-particles and by letting the free neutrons decay into protons, positrons, and neutrinos. Such a causal creation takes place, even if we know nothing about neutrinos. Instead, Latour urges that facts about TRH were consequences of the eventually reached consensus in the laboratory community:



Latour's social construction, mediated by a consensus, is thus epistemic or doxastic in a peculiar sense.

As Brown (1989) convincingly observes, the construction of new physical phenomena in a laboratory is not a social construction, since the potential for their existence 'has always been there'. Moreover, he points out that Latour's argument ignores the difference between a *fact* and what is *believed to be a fact* (ibid., p. 83). All theoretical statements (e.g., about TRH) and beliefs about reality are always conjectural. If the talk about 'constructing facts' really means only the creation of conjectural theories about the world, Latour has used misleading terms to express his view.

There are indeed cultural entities and institutions (such as language, legal order, state), and cultural non-physical properties of material artifacts (such as their function, meaning, monetary value, etc.), which presuppose the existence of a social consensus in the relevant community. Further, in order to refer by a word to physical objects (such as 'table', 'horse', 'electron') a consensus about the meaning of these words is presupposed. But to claim that the reality of physical objects depends on a preceding consensus about their definition or agreement on their existence is again a confusion between World 1 and World 3.

We are able to agree, by our perceptual and linguistic abilities, that the thing in front of me is a table. The existence of this table cannot be explained by this consensus but, rather, our ability to reach a consen-

sus can be explained by the existence of the table. A critical realist extends this account of everyday objects (as ontologically prior to our perceptions and opinions about them) to scientific objects. The available fossil evidence warrants an abductive reasoning to the existence of dinosaurs more than 300 million years ago, and the previous existence of these animals (a long time before a concept identifying them was invented) and the traces they have left for the posterity explain our present consensus. Similarly, the agreement of Guillemin's team did not create TRH but, rather, the previous existence of a substance, controlling metabolism and maturation in animal and human bodies, explains the fact that the 'negotations' did reach an agreement that TRH exists and is Pyro-Glu-His-Pro-NH_2.

Only this order of explanation makes sense of the further fact that also other laboratories have been able to discover the *same* substance with the same structure as Guillemin. And if it were literally true to claim that scientists construct theoretical entities, then they would be also causally and morally *responsible* about them. But this would lead to absurdities: certainly the workers of R. Gallo's laboratory, which first identified the HI-virus, cannot be blamed for the later and earlier infections that this virus (i.e., other tokens of this virus-type) has caused.

It is not only the case that a critical realist can present a more plausible story about theoretical discoveries in science than a constructivist. The same fact – our ability to construct and manipulate theoretical entities – that Latour uses for defending his special brand of antirealism, has been employed by Hacking (1983, 1988) as 'an experimental argument' for entity realism. Essentially the same idea – derived from the Plato-Vico idea that Maker's Knowledge has a higher epistemic status than Spectator's Knowledge[31] – was a key element of Friedrich Engels's 1886 criticism of Kantian agnosticism:

> In addition there is yet a set of different philosophers – those who question the possibility of any cognition, or at least of an exhaustive cognition, of the world. To them, among the more modern ones, belong Hume and Kant . . . . The most telling refutation of this as of all other philosophical crothets is practice, namely, experiment and industry. If we are able to prove the correctness of our conception of a natural process by making it ourselves, bringing it into being out of its conditions and making it serve our own purposes into the bargain, then there is an end to the Kantian ungraspable 'thing-in-itself'. The chemical substances produced in the bodies of plants and animals remained just such 'things-in-themselves' until organic chemistry began to produce them one after another, whereupon the 'thing-in-itself' became a thing for us, as, for instance, alizarin, the

colouring matter of the madder, which we no longer trouble to grow in the madder roots in the field, but produce much more cheaply and simply from coal tar. (Engels, 1946, p. 24)

Here Engels (and Lenin who followed him) makes a stronger claim than the entity realist Hacking: our ability to produce chemical substances (similar to TRH) serves as a proof of our knowledge about their existence and properties.

6. TRUTHLIKENESS AND IDEALISATION

Nancy Cartwright (1983) defends entity realism in a form which claims that theories are "true only of what we make". In other words, theoretical laws 'lie' about the nature of the existing things, and the relation of truth holds only between laws and man-made constructs or models.[32]

Ron Giere (1988) presents a similar view by saying that a theory is trivially true in a model it defines, and the model is similar with the 'real system' in specified respects and to specified degrees (see Fig. 1).[33] In this way, Giere tries to avoid using the tricky or 'bastard' concepts of truthlikeness and approximate truth.

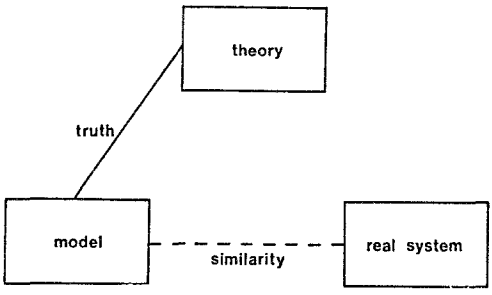


Fig. 1.

However, Giere fails to notice here that

$$\text{truth} + \text{similarity} = \text{verisimilitude}.$$

More precisely, a theory can be defined to be 'approximately true' if it is true in a model which is similar to the real system (i.e., to the fragment of the actual world we are interested in our inquiry). A theory

is 'truthlike', if it is similar to the most informative true statement (of our relevant conceptual system). If the theory contains counterfactual idealisational assumptions, then it has to be compared to factual statements through 'concretization', where idealisations are removed (see Fig. 2). Precise definitions for such similarity relations, in a variety of methodological cases, have been explored since 1974 by the supporters of the 'similarity approach' to truthlikeness.[34]

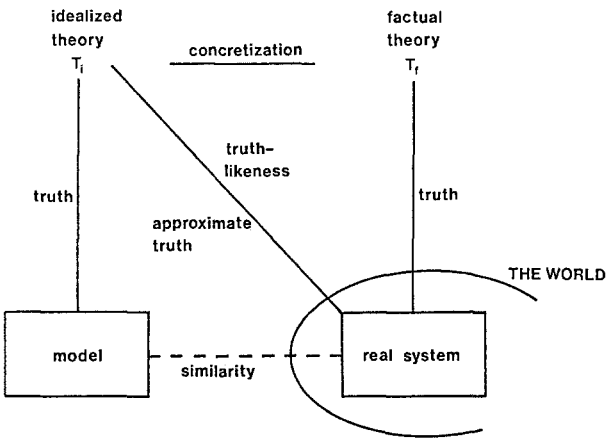


Fig. 2.

Hence, instead of avoiding the issues of verisimilitude, Giere's 'modest constructive realism' turns out to be representable within the framework of critical realism based upon the concept of truthlikeness.

7. RELATIVISM AND REALISM

Let us finally ask to what extent critical realism and relativism can be reconciled. This question is of course ambiguous, since relativism is in fact a family of different doctrines.[35]

Note first that relativity may concern ontological (objects, properties, facts, world), semantic (truth, meaning), epistemological (perception, belief, rationality), or axiological (morality) categories. Ontological relativity may justify semantic relativity, which in turn implies epistemological relativity. But the converse implications do not hold: for exam-

ple, truth may be absolute, even if our beliefs about truth were relative.[36]

Second, relativity may be to persons, groups, cultures, environments, languages, conceptual frameworks, theories, paradigms, points of view, forms of life, gender, social practices, values, interests, etc. Again different types relativisms may be independent of each other: relativity to individuals (what Margolis calls 'protagoreanism') need not imply relativity to conceptual frameworks ('incommensurabilism' for Margolis) or relativity to social interests, and vice versa.

The main argument against philosophical relativism is that its most usual formulations are either innocent (from a realist's viewpoint) or inconsistent.

Note first that some radical forms of relativism are inconsistent or imply a vicious infinite regress. Let us imitate Plato's argument (see Siegel, 1987) by applying it, e.g., to the claim that facts cannot exist unless constructed in a laboratory. Thus, a fact F exists if:

(2) there is a laboratory B where F has been constructed.

Now (2) expresses a fact, F' say, and it exists if:

(3) there is a laboratory B' where F' has been constructed,

etc. Continuing in this way, either we admit at some stage that some fact exists without construction or else we are involved in an infinite regress of an endless sequence of labs $B, B', B'', \ldots$.

Consider next the claim that all truths are relative to persons: my truth may differ from your truth. In a strong form this claim is inconsistent, since it is an example of an absolute truth. An innocent form of this thesis accepts something like:

(4) p is true for $a \equiv a$ believes that p,

where the right-hand side has truth conditions not relativised to persons any more. As two persons may have different beliefs, the statements 'p is true for a' and 'q is true for b' may quite well be both true at the same time, even if p and q contradict each other.

Similarly, the claim that all truths are relative to a point of view[37] may be reducible to:

(5) p is true-from-viewpoint-$v \equiv$ the world appears as p from the perspective v,

where the right-hand side expresses a non-relative fact about the world. Again the statements 'p is true-from-v' and 'q is true-from-w' may be compatible, when p and q contradict each other.

If all that relativism claims is that persons with different cultural background, education, religion, social class, gender, conceptual frameworks, theoretical assumptions, etc., tend to have different *beliefs*, relativism combines easily with realism. This view – which corresponds, via (4), to one way of reading the case studies of the Strong Programme sociologists – implies only that knowledge claims are in some way relative to the position from which a person argues or makes assertions. I myself find this implication quite acceptable.

Relativism becomes a threat to scientific realism if it makes (or succeeds in making consistently) the stronger claim that, in addition to knowledge claims, *truth* and *reality* are also relative to social interests.[38] But this claim is not plausible: the semantic concept of truth, explicated in Tarski's model theory, gives us an objective relation between a sentence (belonging to a language L) and a structure (representing the structural features of the actual world expressible in L). This relation is non-epistemic or 'recognition-transcendent' in the sense that it either obtains or not, independently of our knowledge or beliefs – but not in the sense that it would be impossible for us to obtain fallible evidence about it.[39]

It is quite acceptable to a realist that semantic truth, as a relation between a sentence p and world W, is relative to a language L where p is expressed. As soon as we have constructed the language L, by agreeing on the meaning of its vocabulary, the world W decides its (usually unknown) structure W_L relative to L.[40] Then truth-in-L means truth in W_L in Tarski's sense. This concept, where W_L corresponds to 'a way the world is' in Goodman's sense, is objective or independent of us, or of our idiosyncracies and personal interests, so that 'radical relativism' in Haack's (1987) sense is not valid.

This account does not make truth relative to 'accidental' features of a language. Suppose that languages L_1 and L_2 are intertranslatable, i.e., there is a mapping that correlates one-to-one expressions of L_1 and expressions of L_2 with the same meaning (e.g., 'bachelor' with 'unmarried man', etc.). Then a true sentence in L_1 is translated to a true sentence in L_2, and vice versa. In other words, if p is true-in-L_1, then p (or its counterpart) is true-in-L_2.

Relativity of knowledge claims would challenge scientific realism

also if it turned out that beliefs with different backgrounds cannot be compared at all or be ordered in any rational preference ranking, so that, in particular, no rational argument would show that our scientific beliefs are better than their predecessors or their non-scientific rivals. Sometimes such a relativity of perspectives is combined with absolutism: for example, Marxism–Leninism claimed that beliefs about history and society are relative to class status, but one of the class perspectives (that of the 'most progressive' class, i.e., the working class) is the 'right' one. But it is more common that a philosopher accepts realism *within* a conceptual framework (or viewpoint) and relativism *between* frameworks.[41] In other words, it is argued that statements 'p is true-in-framework-L' are objective, but comparisons of truths across different frameworks are not possible or meaningful. In this view, the choice of a framework is not a cognitive but a practical matter, relative to our variable interests and purposes.

I think this framework relativism contains an important insight. There is no privileged or absolutely ideal framework L such that all statements and theories could be expressed and compared within L. In fact every language gives at best a partial description of reality. But this does not preclude the possibility that there may be good epistemic reasons for preferring one language over another: the choice of a language depends on our cognitive problem and on the service its concept are able to give to theory formation (Hempel, 1952).

The truths in two disjoint or partly overlapping languages (e.g., physiological and psychological theories of human beings) complement each other – and need not be considered as rivals in any sense. More problematic are cases where two theories or belief systems from different periods or cultures are for some reason compared with each other. While it is true that in some cases the frameworks are incommensurable, i.e., not translatable to each other, it seems that the frequency and importance of such examples has been overestimated. For two given frameworks L_1 and L_2 there may exist a third wider framework L_3 such that L_1 and L_2 are expressible in L_3 or translatable to L_3. In cases where L_1 is translatable to L_2, L_2 itself serves as the common extension L_3. And meaning variance does not entail incomparability: there are ways of comparing the cognitive success of theories involving conflicting meaning postulates.[42] A scientific realist thus has many ways of giving a reply to framework relativism.

It is also interesting to note that my definition for the concept of

truthlikeness allows more relativity than truth simpliciter. The degree of truthlikeness $\mathrm{Tr}(g, h_*)$ of a statement g in language L depends on the 'distance' of g from a target sentence h_*, which is the complete true answer to our cognitive problem. This value depends on the language L, the chosen target h_*, the weights of importance of the relevant attributes of L, and the relative balance between our interest in hitting truth and getting rid of falsity. Thus, $\mathrm{Tr}(g, h_*)$ is not a purely semantical concept like truth, but it depends on our cognitive interests in a given situation of scientific research.

As the target h_* is normally unknown, I have proposed that the unknown value of $\mathrm{Tr}(g, h_*)$ is estimated by its expected value: if h_i, $i = 1, \ldots, n$, are the potential complete answers (mutually exclusive and jointly exhaustive), and if $\mathrm{P}(h_i/e)$ is the epistemic probability that h_i is true (i.e., $h_i = h_*$) given the available evidence e, then the estimated verisimilitude of g on evidence e is

$$\mathrm{ver}(g/e) = \sum_{i=1}^{n} \mathrm{P}(h_i/e)\,\mathrm{Tr}(g, h_i).$$

This value depends, besides the factors influencing Tr, on the evidence e and the probability measure P. A fallibilist knowledge claim on evidence e that one statement g is more truthlike than another statement g' (i.e., $\mathrm{ver}(g/e) > \mathrm{ver}(g'/e)$) is therefore relative to our rational degrees of belief (as expressed by P).

The account of scientific reasoning in terms of truthlikeness allows for two different situations. In the first of them, typical of curiosity-based fundamental research, our cognitive interest in some piece of information depends on our appraisal of its theoretical significance in the construction of knowledge. In the second, typical of applied research, cognitive interests are based on practical interests: the choice of a cognitive problem as the object of inquiry, and the relative weighting of the different aspects of the similarity relations, reflect our desire to know something for practical reasons.

Truthlikeness as the epistemic utility characteristic to science is also able to give an account of both consensus and dissensus within the scientific community. Variations in the pragmatic boundary conditions explain why in some situations the scientists do not and cannot reach a rational agreement. But this relativity does not exclude the possibility of consensus, since some comparative judgements about the cognitive

merits of rival hypotheses are 'robust' – independent of any possible pragmatic variations (cf. Niiniluoto, 1987a).

8. RATIONALITY AND PROGRESS

We are now ready to return to the problem of explaining scientific beliefs or preferences among theories.

Laudan (1977) formulated an *arationality assumption* which claims that "the sociology of knowledge may step in to explain beliefs if and only if those beliefs cannot be explained in terms of their rational merits". But, as Laudan (1984a) himself has convincingly shown, principles of rationality have changed in the course of the history of science. So whose theory of rationality should be used in the applications of the arationality principle?

Laudan's initial idea was to apply the best theory of rationality that *we* have. But, as Laudan has acknowleged later, it is more natural to explain a person's belief by referring to his or her own conception of rationality:

(6) a was in a situation S
a thought that it is rational to believe in p in situation S
Hence, a believed in p.

This model of rational explanation can be understood as a generalisation of schema (1).

To apply this idea to science, let SC be the scientific community, V_0 the accepted standards of rationality or 'scientific values', and e_0 the available evidence for SC at time t_0. Further, let $U_v(T, e)$ be the epistemic value of theory T on evidence e relative to standards V. Then a rational explanation of the preferences of SC at t_0 would look like the following:

(7) SC preferred T over T' at t_0 because $U_{v_0}(T, e_0) > U_{v_0}(T', e_0)$.

It may happen that our standards of rationality V would yield a different evaluation:

$$U_v(T, e_0) < U_v(T', e_0).$$

Therefore, (7) allows what Doppelt (1983) has called "moderate relativism of rationality".

Schema (7) explains scientific preferences in terms of their 'rational merits'. Laudan's (1984a) 'reticulational model' suggests that the acceptance of values V in SC could be explained by the theories and methods adopted in SC. But it seems to me that – in addition to this possibility – the choice of values may be justified by many other ways as well. Among them we may have reliance on metaphysical, epistemological, aesthetic, ethical, and social principles.[43] A sociologist of knowledge may at least, in some cases, give a good explanation why values V_0 were accepted in a community at a given time (e.g., why catholic astronomers tended to support instrumentalism) or why only evidence e_0 was available at t_0 (e.g., religious or ethical limitations of experimenting with human beings).

In spite of moderate relativism with respect to rationality, it may be suggested that the concept of cognitive progress in science should be defined in a non-relative way by referring to our standards.[44] My proposal here would be to use the concept of truthlikeness Tr to define an absolute concept of progress – and estimated verisimilitude ver to define an evidence-relative notion of apparent progress.[45] The latter concept allows for the possibility that the step from theory T to theory T' that appears progressive on evidence e_0 turns out to be degenerative relative to extended evidence e.

These remarks are sufficient to indicate how the concept of truthlikeness can be used for descriptive, explanatory, and prescriptive purposes in the theory of science. Such a theory gives a 'realistic' formulation of critical scientific realism – and at the same time it locates several places where scientific knowledge may be influenced by cognitive interests and social practices.

NOTES

* This paper was presented at the 8th Inter-Nordic Philosophical Symposium, Oslo, 18–20 May 1989. Some ideas from this paper were first expressed in a lecture in Professor Aant Elzinga's seminar in Gothenburg, 22 April 1988.

[1] See Niiniluoto and Tuomela (1973).

[2] For details about the concept of truthlikeness and its history, see Niiniluoto (1984; 1985; 1987a) and Oddie (1986). For scientific realism, see Popper (1972), Putnam (1983), Hacking (1983), Leplin (1984), Tuomela (1985), Niiniluoto (1987b), and Nola (1988).

[3] See Mulkay (1977).

[4] "Thus anarchism is not only *possible*, it is *necessary* both for the internal progress of science and for the development of our culture as a whole" (Feyerabend, 1975, p. 180). Similarities between Feyerabend and the recent sociology of science are discussed also in Russell (1983).

[5] Kornblith (1985) is an excellent collection of essays on 'naturalized epistemology'. Early forms of this programme established a link between philosophy and history of science, with a sharp separation from 'mob psychology' (cf. Lakatos, 1976) and sociology (cf. Laudan, 1977). More recently Giere (1988) has tried to combine 'naturalized' philosophy of science with sociology and cognitive science.

[6] See Donovan et al. (1988). Laudan (1977) originally required that such tests should be relative to certain intuitively clear cases, but later he has given up such an 'intuitionistic meta-methodology' (Laudan, 1986) – admitting 'virtually all' episodes of post-sixteenth century science as test cases. A different view, which I find much more promising (Niiniluoto, 1991), is the suggestion that at least many methodological norms are 'hypothetical imperatives' which express connections between means and ends (Laudan, 1987). Such connections can be sometimes established inductively by historical data, as Laudan rightly emphasises, but they may also admit purely formal demonstrations, as traditional analytical philosophy à la Carnap urged.

[7] For example, if a group of scientists favours 'cautious inductions' against 'hypotheses', or 'bold conjectures' against 'uninformative tautologies', it is easy to guess which philosophers they (or their teachers) have read. Galileo's failure to demand that a good theory should entail novel predictions certainly does not refute Lakatos (cf. Donovan et al., 1988) but, rather, indicates that Galileo did not subscribe to the later hypothetico-deductive methodology.

[8] See also Hollis and Lukes (1982), Barnes (1981), Bloor (1983), and the sympathetic evaluation by Mary Hesse (1980).

[9] See Hollis and Lukes (1982), the Laudan–Bloor controversy collected in Brown (1984; 1989). See also Freudenthal (1984), Sayers (1987), and Nola (1988).

[10] Bloor labels himself as an 'inductivist' in Brown (1984, p. 83).

[11] See the last section of this paper. It is perhaps a little curious that Laudan, in his controversy with Bloor, is ready to adopt the talk about 'belief' in scientific theories, since his own antirealist methodology is interested only in the problem-solving capacity of theories and denies the relevance of truth (and thereby, it seems, of belief as holding-to-be-true) among the aims of science (Laudan, 1977; 1984a).

[12] See, for example, Goldman (1967) and Papineau (1988). Goldman would add to (1) that C's belief in p is justified if q is obtained by a reliable process and if the inference from q to p is reliable, where 'reliable' means 'generally truth-producing' (see Kornblith, 1985, p. 603).

[13] See Bloor's replies to Ernan McMullin in Brown (1984).

[14] See Collins (1981) and also Knorr-Cetina and Mulkay (1983). Tibbetts (1986) gives an uncompromising expression to the thesis that the advocates of the empirical relativist and constructivist programme "simply refuse to play according to the rules and guidelines established by traditional philosophy": whenever terms like 'truth', 'reality', 'facts', and 'knowledge' appear, they are "reconstituted in sociological terms". Tibbetts fails to tell why these programmes nevertheless conduct empirical case studies and inductions from them in the style of traditional philosophy.

[15] A good example of such work is Roll-Hansen's (1989) study of the Lysenko affair. He shows that, in order to understand the success of Lysenko's pseudoscience in the Stalin's era, it is necessary to refer to illusory beliefs in the practical usefulness of vernalization and to the underlying principles of science policy based on the practice criterion of truth.

[16] See Brown (1984, p. 89), Barnes (1981), Bloor (1983).

[17] See Niiniluoto (1981). A consensus theory of meaning does not entail a consensus theory of truth (cf. Niiniluoto, 1987a). See also Section 7 below.
[18] This was evident from the enthusiastic reactions of the participants of the 4S and EASST conference in Amsterdam on November 1988. Especially Bruno Latour appears to be today the hero of the sociological community.
[19] Note how easily, e.g., Knorr-Cetina and Mulkay (1983, p. 12), slide from speaking of knowledge to talking about the construction of facts and reality – as if no arguments were needed to justify these steps. Cf. Tibbetts (1986)
[20] See also Hacking (1988) and Fox (1988).
[21] The classic exposition of this approach, which has lent its title to Latour and Woolgar, is Berger and Luckmann's *The Social Construction of Reality* (1971).
[22] Chalmers (1976), influenced by Louis Althusser, combines practical materialism with instrumentalism.
[23] This term is used by Hacking (1983).
[24] Michael Dummett's antirealism, which identifies 'true' with 'proved', is also an example of a non-relativist constructivist position.
[25] Latour's 'Give me a Laboratory and I shall Raise the World' (1983) gives an interesting analysis of the extension of Pasteur's laboratory to the whole world, but the scope of this laboratory world is still very far from Peirce's ideal community of investigators 'without definite limits'.
[26] Internal realism also employs concepts (like acceptability in epistemically ideal circumstances) which are more difficult to understand and make precise than the common-sense realist concept of truth as correspondence. Cf. Newton-Smith (1989) for a good criticism of some recent versions of internal realism.
[27] Cf. Popper (1972), Niiniluoto (1984, ch. 9; 1987b).
[28] For critical discussions on van Fraassen, see Churchland and Hooker (1985).
[29] For definitions of these concepts, see Niiniluoto (1985; 1987a; 1989).
[30] This suggestion relies on Putnam's 'Principle of Charity'. For a clear statement, see Musgrave (1979).
[31] See Hintikka (1974), Niiniluoto (1984, ch. 9).
[32] See also Cartwright's paper, 'Can Wholism Reconcile the Inaccuracy of Theory with the Accuracy of Prediction?', in this issue.
[33] If the real system is here only the empirical world and similarity obtains between the real system and an observational submodel, then Fig. 1 defines the concept of 'empirical adequacy' of van Fraassen's 'constructive empiricism'.
[34] See Niiniluoto (1985; 1986; 1987a), Oddie (1986).
[35] For good accounts of relativism, see Meiland and Krausz (1982), Hollis and Lukes (1982), Laudan (1984b), Margolis (1986), Haack (1987), and Siegel (1988).
[36] Of course these distinctions can be blurred by defining 'fact', 'truth', and 'belief' so that these notions become conceptually interrelated. For example, if truth is defined in terms of belief, then epistemological relativism implies relativism about truth (cf. below).
[37] See Hautamäki (1986).
[38] See, for example, Olivé (1987).
[39] For a more detailed argument, see Niiniluoto (1987a, ch. 4.3).
[40] For example, as soon as we have defined the meaning of words like 'cow' or 'electron', it is up to Nature to decide whether she contains entities satisfying the defining descriptions. What definition we choose may be a matter of social controversy, but when a definition is fixed, the rest is up to the world.

[41] See, for example, Elkana's (1978) 'two-tier-thinking'. Cf. also Oddie (1988).
[42] Pearce (1987) gives an elegant treatment of translation. See also Niiniluoto (1987a, ch. 13.3).
[43] Cf. Niiniluoto (1991).
[44] See also Brown (1984) and Laudan (1987).
[45] Cf. Niiniluoto (1984).

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