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Is there progress in human geography? The problem of progress in the light of recent work in the philosophy and sociology of science

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Abstract: In this article I discuss the meaning of progress in geography and the social sciences in general in the light of recent debates in the philosophy and sociology of science. I review the way in which notions of progress derived from different philosophies of science have been used in geography in the past, before focusing on the kinds of constructivist challenges posed in recent work by authors such as Barnes. I criticize these approaches from a realist-constructivist perspective and argue for a deeper engagement with social epistemology.

Key words: constructivism and realism, metaphors, paradigms and incommensurability, scientific progress, social epistemology, sociology of scientific knowledge.

1 Introduction

On the face of it, it is very reassuring to have a journal called Progress in Human Geography. The very title seems to confirm that human geography is indeed making measurable progress, issue by issue, and year by year. The inclusion of a wide range of regular ‘progress reports’ suggests that this progress is occurring on almost all fronts. Yet when I read these reports I often find it difficult to detect what concepts and measures of progress the authors are actually using. The reports are usually summaries of what different people have published in different problem areas since the last report. As a result of the tyranny imposed by output indicators and research assessment exercises, publication rates have been forced up as a matter of departmental prestige and individual survival, so there is more and more to review in each subject area. At the same time one also gets the sense of accelerating change, with frameworks, concepts
and ideas experiencing a rapidly diminishing half-life. This may be all very exciting, but increased output and rapid conceptual shifts do not necessarily imply progress. Are we then really making progress and, if so, in what sense?

This issue has been addressed in this journal before (e.g., Lowe and Short, 1990) but it seems to me that the issue is important enough to keep under continuous review, particularly in the light of recent developments in the philosophy and sociology of science. Thus my interest in the issue has been stirred by the recent publication of books by Johnston (1997) and Barnes (1996), both of which present wide-ranging reviews of the development of human geography, one written from a realist position and the other decidedly anti-realist in tone. Whereas Johnston preserves a notion of progress (though a multidimensional one), Barnes draws upon a neopragmatist, postmodernist and social constructivist literature to try to undermine the very notion of progress itself.

Now if Barnes and others who think like him are right it would seem a heroic but increasingly empty gesture to go on calling a journal *Progress in Human Geography*. Perhaps it is time to rename the journal *Endless Reinterpretations in Human Geography* or, even more simply, *Summaries of What Academics Have Been Doing to Further Their Careers Since the Last Issue?* This is not a view I feel very comfortable with because I believe that some sense of making progress is central to what we do. Without some belief in, and conception of, progress I find it difficult to see what would motivate most academic work. Indeed, ‘without some overriding sense of intellectual direction how is anyone to justify any programme of inquiry?’ (Rule, 1997: p. 173). Hence my interest in focusing on the issue of progress here.

I start with some simple definitions before reviewing the way ideas of scientific progress have been incorporated in geography. I then summarize Johnston’s multiparadigmatic framework before exploring in more depth the kind of challenges posed by Barnes and the literatures he draws upon. I defend the notion of progress from a ‘realist-constructivist’ perspective that I hope will become clearer as I go along. I end with some comments drawn from social epistemology on the relationships between disciplinary progress and social and institutional contexts.

II Dimensions of progress

The first point to make is that progress is clearly multidimensional. For example, we might be making *institutional* progress in the sense that our subject is becoming more established and influential as a discipline within an academic world. We might be making *empirical* progress in the sense that our subject is able to predict more and more phenomena successfully within its field, or predict the same phenomena with increased accuracy. We might be making *explanatory* progress in the sense that our subject is generating improved concepts and theories which enable it to provide better explanations of phenomena in its field. We might be making *conceptual* progress in the sense that our theories are extending their scope and becoming internally more consistent. We might be making progress in *inter-subjective understanding* in the sense that our knowledge enables us to grasp more clearly how others see the world and each other. We might be making *pragmatic* progress in the sense that our subject is making a greater and greater contribution to public policy and the solution to social problems. In certain circumstances such progress may even be *emancipatory* in so far as it frees us from social
illusions or directly addresses the problems of the oppressed and marginalized (e.g., Lowe and Short, 1990).

Although progress is multidimensional, I take the view that institutional and pragmatic progress cannot long be sustained without some underlying empirical, conceptual or explanatory progress. I also take the view that progress in these dimensions is also necessary (though certainly not sufficient) for progress in intersubjective understanding. Thus I reject any simple dichotomy between explanation and understanding, a divide that has long plagued the social sciences in various forms. I am more sympathetic to those approaches that deny this dichotomy and seek for complementarity or compatibility between explanation and understanding (e.g., Fay, 1996). In what follows I therefore narrow the focus to concentrate on the explanatory/empirical/conceptual dimensions, topic areas traditionally covered by the philosophy of science. Without going as far as Popper (1963), who has claimed that only in science do we actually have firm criteria of what constitutes progress and how to measure it, it is historically the case that many of our most powerful concepts of progress have been derived from the practice and philosophy of science.

III  On the progress of ‘progress’ in geography: positivism to post-Kuhnianism

Most geographers have probably taken the progress of their subject for granted, but when forced to justify this have tended to shelter under the umbrella of some convenient philosophy of science. The positivist view of science, which geography began to absorb in the 1950s, defined progress in terms of the formulation of theories which incorporated one or more laws (Harvey, 1969). Successive theories were progressive in so far as they entailed their predecessors, or incorporated them as limiting cases in wider frameworks, permitting more accurate explanations and predictions (Laudan, 1990). As is well known, however, human geographers quickly retreated from what they soon realized were excessive and inappropriate demands on social theory.

As a partial alternative, Popper argued for a falsificationist methodology of bold conjecture and severe testing that would lead its practitioners towards theories with greater and greater truth content, or ‘verisimilitude’ (Newton-Smith, 1981; Hands, 1991). However, Popper argued, real theoretical progress required not just any old truth but ‘interesting truth’, and to display interesting truth a theory must 1) reflect some simple, new and powerful, unifying idea; 2) have new and testable consequences; and 3) pass new and tougher tests. In particular it should be successful in predicting ‘novel facts’.

Few geographers explicitly embraced this Popperian programme (Marshall, 1985), although for a while it proved very popular amongst economists (Blaug, 1980). However, most social scientists have found the Popperian criteria for progress too demanding, and the emphasis on the prediction of ‘novel facts’ too narrowly focused to cover much of what work is actually done in the social sciences. In any case, as Popper was finally forced to concede, his concept of verisimilitude was simply unworkable, and could not serve as a measure of progress (Maki, 1991).

Many geographers moved directly from positivism to Kuhn without passing through Popper. This was not surprising given Kuhn’s enormous impact on the way we think
about science and scientific progress. Kuhn presented a view of science developing unevenly over time, rather than through cumulative progress. During the phases of ‘normal science’ progress took place within the context of a particular, dominant paradigm which structured the questions to be asked, the methods of inquiry to be used, the criteria for explanation and thus the very definition of what constituted progress. Long periods of normal science were interspersed with crises, revolutions and the consolidation of a new paradigm likely to be incompatible with the old. This meant older questions were not necessarily solved, they were simply dissolved and different questions addressed with different standards of explanation. There was no overarching logic to demonstrate the superiority of the new paradigm over the old; they were simply ‘incommensurable’ (Kuhn, 1970). While Kuhn only applied this model to the natural sciences this did not stop geographers making extensive use of his vocabulary, although in the process they often misread Kuhn and diluted some of the radical import of his work (Mair, 1986; Johnston, 1997).

Post-Kuhnian philosophy of science has gone in several directions. One approach has been to try to reconcile Kuhnian insights with pre-Kuhnian concerns. Lakatos, for example, attempted to reconcile Kuhnian historiography with Popperian rationalism, focusing his attention on ‘research programmes’, with their ‘hard cores’ and their ‘protective belts’, which bore the brunt of testing (Lakatos, 1970). A research programme was ‘theoretically progressive’ if each new theory in a series explained previous successes and had additional (novel) refutable consequences. It was ‘empirically progressive’ if some of those predictions were confirmed. A degenerating research programme, on the other hand, failed to produce novel predictions, and was subject to a variety of ad hoc adjustments in response to anomalies (Hands, 1993).

This framework for evaluating progress attracted the attention of many economists, but fewer geographers (e.g., Wheeler, 1982). However, after numerous attempts to fit different bits of economics, such as microeconomics or Keynesian economics, into a research programme framework, more and more economists have recently become sceptical of the whole approach (e.g. Hausman, 1992; de Marchi, 1993; Backhouse, 1994). Not only is it difficult to structure theories in this way, Lakatos’ notion of progress is seen as still too dependent on the kind of ‘novel fact fetishism’ found in Popper (Hands, 1991).

Like Lakatos, Laudan (1977) has also tried to reconcile Kuhnian and pre-Kuhnian themes in a reticulated model of scientific change. Although his ideas have been used in some recent debates on progress in political theory (e.g., Simowitz and Price, 1990; Levy, 1996), his ideas seem to have had even less impact on geographers.

A different post-Kuhnian direction has been taken by realists of varied persuasion. Realism comes in many forms, but the version that has attracted the attention of most geographers has been the critical realism of Bhaskar (1978; 1991) and Sayer (1992). Broadly stated, the object of science is to gain explanatory knowledge of underlying structures, objects and powers through processes of rational abstraction that seek to distinguish between necessary and contingent properties and relations. Such an approach implies a view of progress as the uncovering of deeper and deeper levels of a stratified reality through progressively more rational abstractions. Although there are major problems in demonstrating the existence of underlying structures or mechanisms that may not be directly observable (Yeung, 1997), this is a framework that has become well established in many areas of geography.
It is evident then that geographers have absorbed different concepts of science, and thus different concepts of scientific progress, over the past four decades. Johnston’s *Geography and geographers* (1997) attempts to bring many of these strands together in a single framework.

**IV Progress in a multiparadigmatic geography**

Johnston’s *Geography and Geographers* has gone through many editions and has become the standard reference in its field. In the latest edition, Johnston rejects the simple Kuhnian idea of geography evolving through a sequential series of dominant paradigms separated by revolutionary breaks, but he still tries to retain certain elements of Kuhn’s thinking by disaggregating the notion of paradigm into three levels: ‘world views’, ‘disciplinary matrices’, and ‘exemplars’. He argues that since the 1950s geography has become more resolutely multiparadigmatic, in the sense that different world views (he refers to ‘positivism/empiricism’, ‘humanism’ and ‘social theory/radicalism’), and different disciplinary matrices (‘Marxism’, ‘realism’, etc.) continue to coexist in parallel with each other, each structured around its own distinctive set of exemplars (or canonical texts). These world views are ‘entirely incommensurable’ (Johnston, 1997: p. 386), so one cannot say in what sense one is an advance on another. However, Johnston argues we can still discern progress taking place within particular world views or disciplinary matrices. More and more sophisticated modelling, for example, evidences progress within the positivist/empiricist world view, although it would not be recognized as such by humanists who focus more on the development of intersubjective understanding. Progress within the social theory world view might be evidenced in realist terms through the uncovering of deeper structures and mechanisms through progressively more rational abstractions. In summary, Johnston presents us with a multiparadigmatic view of geography, embodying different philosophies and different concepts of progress.

Although this seems to allow a degree of diversity and pluralism within geography, Johnston’s underlying position is resolutely realist, a preference more clearly laid out in Johnston (1986). Here he criticizes the limitations of the positivist/spatial science and hermeneutic/humanist perspectives, making realism the key to a deeper explanatory form of knowledge which can lead to emancipatory ends.

Although I share Johnston’s realist inclinations, I have several points of criticism. First, I think his paradigmatic framework, even in its revised form, is beginning to come apart at the seams. For example, he discusses feminism and the cultural turn under his existing headings, but they are themselves so multiparadigmatic they threaten to burst through his carefully constructed framework. Secondly, there is also room for disagreement as to whether the world views which he identifies are really as incommensurable as he claims. Incommensurability comes in various forms – semantic, perceptual, ontological, etc. – and these pose problems of varying degrees of difficulty (e.g., Malone, 1993; Hoyningen-Huene et al., 1996). Kuhn himself was ambiguous on the issue of incommensurability, and it is possible to extract both conservative and radical interpretations from his writings. Indeed, in response to accusations of relativism and irrationalism, he suggested various criteria for rational choice between paradigms, such as accuracy, consistency, scope, simplicity and fruitfulness. In his later writings he also
wished to make clear that ‘incommensurable’ did not mean ‘incomparable’. Even some of his most famous illustrations of incommensurability have been reworked by others to show that it is less of a problem than he claims (see, for example, Kitcher’s and Couvalis’ reworkings of some of Kuhn’s classic cases in terms of ‘the refinement of reference potentials’ and ‘the causal theory of reference’ – Kitcher, 1993; Couvalis, 1997).

Thirdly, recent work in the sociology of scientific knowledge (which I come on to later) suggests science is too heterogeneous a practice to be encompassed in terms of paradigms (Galison, 1995). The language of paradigms suggests the existence of great continents of relatively stable ideas and practices. Instead, science is more like an archipelago of islands, each of which represents a distinctive subculture engaged on a different research programme centred on particular clusters of constraints. Rather than incommensurability between programmes, the evidence suggests widespread ‘trading zones’ between islands in which communication goes on using a great variety of ‘pidgin’ or ‘creole’ languages (Galison, 1995).

My own preference is to abandon the notion of paradigms and focus analysis on different research programmes in geography, but research programmes shorn of their Lakatosian restrictions. A model of how to proceed is offered by Bohman (1991) who focuses on more middle-range reconstructions of social theory than paradigms. He identifies a series of research programmes in the social sciences (rational choice, ethnomet hodology, interpretative theory, the theory of communicative action, etc.), each of which is distinguished by distinctive explanatory patterns or explanatory exemplars. Nevertheless, such research programmes share certain minimal assumptions, and Bohman shows how both intra- and intertheoretic comparisons might be made.

However, rather than developing these points of detail further here, I want to switch attention to a set of much deeper challenges to all such realist-based approaches to progress. Such approaches have been criticized from a third post-Kuhnian direction which encompasses a broad current of work with a relativist, anti-essentialist and anti-realist emphasis. It is this stream of work, broadly labelled constructivist (e.g., Boyd, 1992), that Barnes taps into to power his deconstructive critique of science and progress in geography. In the next section I look at these approaches in more detail, the use that Barnes makes of them and their implications for concepts of progress.

V Constructivist challenges: philosophical and sociological

Barnes’ central argument is that our concepts of scientific progress are derived from a wider Enlightenment project whose foundations have now been effectively demolished by postmodernist and neopragmatist critiques. As a result, our notions of progress have lost their meaning and legitimacy. Any view of our own intellectual history as an epic of progress must be replaced by a view of history as composed of ‘ruptures, disjunctions and gaping non-sequiturs’ (Barnes, 1996: 9). Thus in opposition to any ‘progressive’ account of economic geography Barnes prefers to stress discontinuities and dislocations, denying the subject a progressive view of itself.

Barnes justifies this viewpoint by drawing upon constructivist theories, both philosophical and social. From his standpoint, the work of Popper, Kuhn, Feyerband and others represented but a series of temporary bridges leading to much more radical critiques of science which have followed, represented for Barnes by the neopragmatism
of Richard Rorty and the ‘strong programme’ in the sociology of science. I will discuss these perspectives in turn.

1 Philosophical constructivism

a Rorty’s deflation of science and epistemology: A central theme which attracts Barnes to Rorty’s philosophy is his rejection of foundationalism, essentialism and the related notion that truth is in some sense defined by correspondence to the real. For Rorty (1991a: 79), the notion that truth is correspondence to reality is simply ‘an uncashable and outworn metaphor’. Although Rorty does not deny the reality of what he calls ‘brute physical resistences’, he sees ‘no way of transferring this non-linguistic brutality to . . . facts, to the truth of sentences’ (Rorty, 1991a: 81). There is, in short, no language, scientific or otherwise, that ‘cuts nature at its joints’, or that is in some way ‘Nature’s own’. Attempting to assert such a relation reflects an ‘ambition of transcendence’ that involves us in the impossible task of trying to ‘climb out of our own minds’ to some Archimedian point from which we could judge the accuracy of our representations. To escape from this conceptual trap we must first throw away our dominant metaphor of language as somehow mirroring or representing reality, and accept that we have no means of knowing when we have reached truth, or of telling whether one theory is truer than another. We are never in a position to compare a description to an object; we can only compare it to another description, and there is no neutral criterion for saying one description is ‘better’ than another.

This perspective clearly has important implications for how we view the whole activity of science. For Rorty, there is no scientific method as such that guarantees access to reality, and he wants us to replace all talk of scientific objectivity with that of solidarity, in the sense of a search for unforced agreement. Indeed, once we have got rid of notions of objectivity and scientific method we can see more clearly that science is just another literary genre and the social sciences are continuous with literature. Science is nevertheless exemplary, not in the sense that it gets things right by accurately mirroring reality, but because it is a model for achieving human solidarity.

As for scientific progress, we must clearly give up the notion of science travelling towards an end, which is closer correspondence with nature. The different vocabularies we use to represent nature or society are not to Rorty more or less objective, or more or less scientific, they are simply more or less useful in the sense that they work better for a given purpose. We must therefore replace talk of theories ‘corresponding’ with talk of theories ‘coping’, ‘always remembering that modern science does not enable us to cope because it corresponds – it just enables us to cope’ (Rorty, 1982: xvii).

Scientific change takes the form of a series of radical discontinuities, or metaphoric redescriptions, which work holistically and pragmatically. Although metaphors have no cognitive meanings beyond their literal ones, they can have great force in causing us to change our beliefs and shock us into seeing the world in a different way. Metaphors range from the simple to the complex, so that entire scientific revolutions can be conceived as shifts in ‘founding’ or ‘constitutive’ metaphors that lead to complete metaphoric redescriptions. Such metaphoric redescriptions may be highly productive in enabling us to gain predictive knowledge. However, this does not mean that metaphors can therefore be compared in terms of their success in representing the
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world, because ‘the world does not provide us with any criteria of choice between
metaphors’ in this representational sense (Rorty, 1989: 20). Intellectual progress is thus
‘a history of increasingly useful metaphors rather than of increasing understanding of
how things really are’ (Rorty, 1989: 9). Rorty gives us instead the image of our minds
gradually growing larger and stronger and more interesting by the addition of new
candidates for belief and desire, phrased in new vocabularies and new metaphors. This
is why Rorty’s image of the great scientist is someone who ‘made it new’ rather than
‘got it right’ (Rorty, 1991a: 44).

A sense of what Rorty intends us to do can be seen in his advice to feminists (1991b).
Feminists are urged to drop their language of representation, and the notion of
achieving progress through a progressively less distorted perception and understand-
ing of reality. The metaphor of mirroring should be replaced by the metaphor of evolu-
tionary development, in which there is a competitive struggle between different vocab-
ularies and speech elements. Rather than attempting the impossible task of describing
things ‘as they really are’, feminists should instead use new vocabularies to ‘make
invidious comparisons between an actual present and a possible, if inchoate future’
(Rorty, 1991b: 7). In the process of creating a new vocabulary they will also be creating
new identities for themselves. When these new vocabularies become dominant they
will have transformed the reality of their position (for comments see Lovibond, 1992).

Many of these ideas are taken up by Barnes in his critique of economic geography.
Mirror metaphors, he argues, run through much of mainstream economic geography
and both Marxist and neoclassical theories of value are essentialist theories in the
Rortyian sense. Barnes is also particularly interested in the role of big metaphors that
have shaped whole research agendas in geography. He sees the history of economic
geography as ‘the history of armfuls of forgotten metaphors’ (geological, meteorologi-
cal, morphological, etc.) that were once alive but have now become stale and literal
(Barnes, 1996: 156). The value of such metaphoric shifts is not that they take us closer to
reality, but that they give us a jolt and redirect the conversation in new and interesting
ways. Like Rorty, he takes the view that these metaphors are incommensurable.
Referring to Perroux’s growth pole theory and Massey’s location theory, based respec-
tively on images from magnetism and geology, he argues there is no rational basis for
comparing them in terms of their progressiveness because metaphors act like Gestalt
switches, changing the whole basis for comparison. Like Rorty, he rejects the implicit
essentialism of the mirror metaphor in favour of contextual theories without any foun-
dational bedrocks, and he finds elements of the nonessentialist value theory that he is
looking for in the work of Sraffa, Laclau and Mouffe, and Resnick and Wolff.

In summary, Rorty’s philosophical writings, and Barnes’ more focused geographical
ones, would seem to make our traditional concerns with progress irrelevant.
Nevertheless, however compelling some of these arguments may appear I believe that
there are even more compelling counterarguments.

b Lines of resistance: linguistic idealism versus modest realism: First, both Rorty and
Barnes score many of their points by setting up as their target a kind of ‘vulgar realism’
that few if any realists would subscribe to. We are then forced into a choice between a
‘vulgar realism’ on the one hand and what might be termed a ‘vulgar pragmatism’ akin
to linguistic idealism on the other, as if no middle way was possible. Yet many have
argued that some form of modest realism is an eminently defensible (though less rhetorically exciting) position, which avoids the absurdities of both extremes.

A modest realism need not claim that truth must involve a one-to-one correspondence between sentences and the world; some critics of Rorty have argued that the kind of holistic relationship between world and total language systems described by Davidson can serve as an adequate basis (Prado, 1987; Nielsen, 1991; Farrell, 1994). A modest realism need not demand a final vocabulary which is assumed to be ‘nature’s own’; it is fully compatible with a pluralism of vocabularies each relevant to our different purposes. But this kind of conceptual pluralism does not imply the kind of linguistic idealism that Rorty slides into where languages and conceptual systems seem to create the world. As Farrell (1994: 128) puts it, ‘even when the world appears according to the character of our conceptual apparatus, it is the nonconceptual world which is appearing, not the conceptual apparatus itself’.

A modest realism also need not involve any appeal to crude essentialism. Sayer, (1995) for example, does not try to defend the kind of ‘epistemological essentialism’ that makes foundationalist claims about knowledge of essences. He does, however, defend an ‘ontological essentialism’ that claims that objects (markets are one of his examples) have certain essential features without which they would not be those kinds of objects, and particular powers and ways of acting as a consequence of their intrinsic structures. Such essences need not be universal and unchanging, but specific to different historical contexts.

Finally, a modest realism can also be promiscuous, in the sense described by Dupre (1996). There are many overlapping and intersecting ways of classifying objects into kinds; there is no presumption of one true theory, or even of any convergence in theories; science is radically fractured and disunified without being any the less scientific.

In summary, we can find good grounds for defending a modest form of realism in geography and denying the polarized alternatives that Rorty and Barnes seem to offer us. We can accept arguments against any Archimedian point or mirror-imagery, but this does not mean there are no real-world constraints on theory choice. A modest realism need assert only a modest foundationalism (the truth of some of our beliefs is ultimately grounded in how things are) and a modest objectivism (it is the world that ultimately determines true beliefs).

c Underdescribing science: A crucial aspect of Barnes’ and Rorty’s approach is the denial that science has a privileged status as a discourse that in some sense puts us closer in touch with reality, or represents things better. In comparing Galileo and Aristotle Rorty says that ‘it just turned out’ that Galileo’s picture of the universe has worked better than the Aristotelian picture. By ‘worked better’ he means that it has proved more productive and has enabled us to ‘cope’ better. But there is no sense in which one can talk about cumulative progress in knowledge between the two pictures. Barnes makes a similar argument with respect to metaphors in economic geography.

There seem to be a number of problems and implausibilities with this perspective. First, Rorty accepts that science has proved remarkably productive as a discourse, but he seems to have no clear explanation why this should be so. Why, we want to ask, is science more productive than other discourses? Why does it enable us to cope better –
and cope with what? As Geras (1995) points out, Rorty can’t bring in the world at this point to provide any answers because he denies the world has any decisive role to play in the matter. We are left with the notions of ‘productivity’ and ‘coping’ as definitions internal to different world pictures or paradigms, but we still have no answer to the question as to why science works at all, and why it is so successful.

Secondly, there are problems with Rorty’s denial of science’s claims to realism and representation. The problem is that the language of realism and representation is clearly intrinsic to the very practice of science. Indeed it is difficult to conceive of science as we know it continuing without such a language. It is also the case that such a language, and the view of the world that underlies it, has also clearly proved very successful, at least so far in our history. In Rorty’s terms, such a language has given us ‘a lot of what we want’, and realist vocabularies of science ‘have survived well the Darwinian clash of vocabularies in history’ (Farrell, 1994: 125). It would seem that on pragmatic grounds alone Rorty ought to approve of such a vocabulary.

Thirdly, it could be argued that Rorty can only deny science any privileged status relative to other discourses because his view of science is a serious underdescription of what science is. Natural science, Rorty (1991a: 163) states at one point, is ‘simply an instrument of prediction and control’. Now this perspective, as Bhaskar argues, is a very instrumental one, and seems to be based on a narrowly positivist view of science, with its implicit ontology of atomistic events and its restricted Humean (or constant conjunction) view of causality (Bhaskar, 1991). Scientific realists would regard this as a basically flawed and impoverished view of science. A more sophisticated, realist ontology allows for the existence of entities, structures and mechanisms, with different powers and tendencies, and a more complex view of causality that denies any simple symmetry between explanation and prediction (e.g., Bhaskar, 1978; Hacking, 1983; Cartwright, 1983; 1989; Sayer, 1992). Such a view also helps to explain why science is different from other discourses in its reflexive ability to explain the grounds for its own success (Williams, 1983).

d Exaggerating incommensurability, overinflating metaphors: Rorty and Barnes tend towards a strong reading of Kuhn’s version of the incommensurability of scientific paradigms and world views, based upon their interpretation of the nature and role of metaphors in scientific discourse. Whilst I accept that it is important to study metaphors, particularly the kind of founding metaphors that shape major research programmes or whole disciplines, there are aspects of the Rorty–Barnes position that I find unconvincing.

First, there is the problem of overextension. Acceptance of the importance of metaphors should not lead us to the view that metaphor is all there is, or that talk in terms of metaphors and metaphoric redescription can displace all traditional issues and debates in philosophy of science. I am more sympathetic to the views of Haack (1987; 1994), who argues that although metaphors may be useful to scientific discovery, they are not essential. Metaphors are particularly useful ‘for expressing newly posited but so far imperfectly understood similarities, and as a spur to suggesting how they might be elaborated and developed’ (Haack, 1987: 285). For example, postulating that ‘the mind is a computer’ is a good way of prompting investigation and directing research in particular directions. Such metaphors are useful in the early stages of scientific research,
when they function as rough drafts of scientific theories. It is their very incongruity, their open-endedness and lack of specificity, that serves to prompt imaginative inquiry. But, Haack (1994) suggests, this does not mean that at a later stage they cannot be kicked away like ladders that have served their function. Of course, metaphors may lie deep within our discourses and be difficult to detect as they solidify into a kind of taken-for-granted literalness. However, the fact that we are able to excavate them (e.g., Mirowski’s work on the physics metaphors structuring neoclassical economics) shows that we can achieve objectivity and critical distance (Mirowski, 1990).

Secondly, even if we accept the importance of metaphor we are not tied to a theory of metaphor that denies cognitive content or requires a wholesale rejection of realism and the embrace of incommensurability. The Davidson/Rorty/Barnes view is an example of the ‘no-semantics’ approach to metaphor, but this is only one of at least five major approaches (Ankersmit and Mooij, 1993). Other approaches are also potentially fruitful, and some are quite compatible with certain versions of realism. Boyd (1979), for example, sees metaphors as one of the many devices used by scientists in the task of accommodating language to the causal structure of the world. Theory-constitutive metaphors play an important role in the early stages of theory development, fixing reference in a preliminary way to natural kinds whose essential properties are not yet known, perhaps using analogies to kinds whose properties are better known. Later research may show whether these referents do or do not exist (Boyd, 1979: 406).

Haack (1987), on the other hand, argues for a more moderate ‘fallibilistic realism’. In opposition to Rorty and Davidson, she argues that metaphors are telescoped similes and can have metaphorical and literal meanings. Metaphoric redescription is just part of the endless process of revising our categories and classifications in the hope of arriving at categories that better correspond to real natural kinds and serve our current interests. Hesse (1993) also presents a theory of metaphor that is compatible with a modest realism and a concept of scientific progress. Although language is metaphorical through and through, metaphors can have cognitive meaning and we can talk sensibly about standards of metaphorical correctness (for a similar view see Mooij, 1993). Discussion of the comparison and evaluation of metaphors in economics can also be found in Bicchieri (1988).

In summary, I am arguing that it is reasonable to claim that metaphors can be evaluated in terms of the meaningful representations of their objects (a point of view that Barnes himself slides towards at times). It follows that we need not exaggerate the incommensurability of metaphors in geography. For example, it is one thing to claim that neoclassical economics and Marxian economics are structured around different metaphors (Barnes, 1992), it is another to claim that they are incommensurable. At one level they might look like radically incommensurable discourses. Yet, from a higher vantage point, they seem more like complementary representations of market economies within a wider discourse of western political economy (Sayer, 1995). They are simply two different ‘optics’ or ‘modes of abstraction’ of the same totality, each revealing aspects of the economy that the other neglects.

Finally, I also think we have to ask ourselves what kind of practices would follow if we accepted Barnes’ and Rorty’s arguments. One line of critique is that such views are critically disabling (e.g., Bernstein,
1991). Rorty’s favoured ironists cannot present descriptions of society as empowering or emancipating by claiming they reveal real structures of oppression or injustice. Yet emancipation in real social systems may clearly require more than redecription; it may require the transformation of real and enduring social structures whose mechanisms of oppression need to be accurately identified.

Even worse, it has been suggested, Rorty’s voluntarism encourages fleeting paradigm shifts and exotic redescriptions which are largely judged on aesthetic grounds. To Bhaskar (1985: 134–35), Rorty’s project smacks of ‘an ideology for a leisured elite . . . neither racked by pain nor immersed in toil – whose lives may be devoted to the practice of aesthetic enhancement’. Even more harshly, Haack (1994: 139) warns, ‘there would be no honest intellectual work in Rorty’s post-epistemological utopia’.

The underlying argument then is that critique and emancipation would seem to require some element of philosophical realism as a basis. The identification of real structures, powers and tendencies is necessary to enable us to uncover ideological distortions and forms of domination, and to carry out thought experiments to explore the possibility of different, and better, social organizations. This is also the burden of Lovibond’s (1992) feminist reply to Rorty’s ‘advice to feminists’.

In summary, I believe there are many good reasons why we should be wary of the whole Rortyian project and its Barnsian echoes in geography. I believe we can resist attempts to deflate science, drop the language of progress and wrap ourselves in the rhetoric of metaphors, particularly when it can lead to a loss of critical power. But the above arguments represent only one dimension of constructivism. Barnes also draws upon another, complementary dimension, that can be labelled ‘social constructivism’.

2 Social constructivism

a Introduction: from the strong programme to the sociology of scientific knowledge: Although it was not his intention, Kuhn’s approach to science opened the doors to much deeper explorations of the sociology of science, with increasingly radical consequences. This soon became evident in the work of the so-called Edinburgh school, which revolutionized earlier and more conservative versions of the sociology of science in the 1960s by seeking to explain not only the practices but also the content of science in terms of social factors (e.g., Barnes, 1977).

The Edinburgh school argued that the closure of scientific debates is always under-determined by the evidence available and always primarily reflects a structure of social interests. Such an emphasis on the social causes of scientific beliefs left no space for epistemic values in theory choice, and denied there was a rational set of methodological rules that could ensure scientific success. The preferred mode of inquiry was to be guided by the tenets of the so-called ‘strong programme’, which emphasized causal explanations of scientists’ beliefs, impartiality and symmetry in the explanation of true and false beliefs, and reflexivity on the part of researchers with respect to their own findings. These arguments were illustrated in a series of studies which focused on the social determination of scientific beliefs in different, local contexts (Shapin, 1995).

In the late 1970s a new phase opened up, marked by a series of more microscale,
ethnographic accounts of laboratory practices which appeared to challenge traditional, realist accounts of science. Thus Latour and Woolgar’s (1979) *Laboratory life* was a participant observer’s account of the determination of the chemical structure of a new hormone in a California research laboratory. The authors argued that the substances identified could not be said to exist independently of the laboratory techniques, equipment and social processes of negotiation among scientists necessary to produce them. Although published scientific accounts were typically written as if the entire research process had been guided by the pre-existing reality of the substance, Latour and Woolgar (1979: 243) concluded that scientific activity was not ‘about nature’, but ‘a fierce fight to construct reality’.

A similar perspective underlay Pickering’s (1984) account of several decades’ work in high-energy particle physics, culminating in the quark-gauge view of modern physics. In Pickering’s account quarks also emerge as social constructs, a product of a hard-won scientific consensus which established an essentially self-referential and self-confirming package of theory, experimentation and data. The notion that quarks were there all along guiding research was dismissed by Pickering as ‘retrospective realism’, an after-the-event rationalization that only appeared in scientists’ accounts and concealed the underdetermined choices and judgements which scientists had to make at each stage through processes of social negotiation. Each choice was an example of ‘opportunism in context’ in which scientists pursued strategies that enabled them to recycle their skills and resources.

Studies such as these suggest that ‘the natural world has a small or non-existent role in the construction of scientific knowledge’ (Collins, 1981: 3). This perspective clearly undermines traditional ideas of scientific progress, such as the belief that our constructs can progressively come to map the world as it really is (Nanda, 1997). This is indeed the lesson Barnes draws for economic geography. Through a series of disjointed ‘vignettes’ rather than a progressive metanarrative he tries to show how theories in economic geography were never guided by any consistent scientific method, but reflected diverse sets of locally constituted social interests. Through their theories and practices geographers thus constructed rather than reflected spatial realities. As a result economic geography has not progressed in a scientific sense, but has jumped from one metaphoric redescription to another. Again, it seems, we have some powerful arguments against established notions of scientific progress. Again, however, I think there is a series of counterarguments that should give us pause for thought.

**b  Realist responses:** Many of the most powerful critiques of social constructivism have embodied realist ideas of one sort or another. One line of critique argues that much of the rhetorical effect of constructivist studies results from running together different meanings of the term ‘construction’. Whilst we might easily accept that scientific practice involves the construction of conceptual order, and of phenomena in laboratories, it is a big step from there to the more idealist, neo-Kantian form of constructivism in which representations are said to constitute material reality (Sismondo, 1993). This is simply an example of what has been termed ‘the epistemic fallacy’ which assumes that what we know determines what exists (Bhaskar, 1978).

Thus, with respect to studies such as those by Latour and Woolgar, it could be argued that constructing new physical phenomena in a laboratory in the way they describe is
not simply a social construction. It is a material process whose possibility depends on the potential for those substances’ existence in nature (Brown, 1989). In other words, the ability of scientists to arrive at a consensus in such situations is often more plausibly explained by the existence of the object they are analysing (Niiniluoto, 1991). Furthermore, the laboratory experimentation described, involving the manipulation of entities and processes whose causal powers are well established, in order to produce new effects and entities, is itself a good argument for at least a form of ‘entity realism’ (Hacking, 1983; 1992). Otherwise, the ability to produce new effects in predictable ways looks pretty much like a miracle.

If this is so then the ‘retrospective realism’ that Pickering and others dismiss as the false consciousness of naive scientists looks like a reasonable and plausible position for them to take. In all sorts of fields there are ‘good reasons’ for believing that entities really do exist (Miller, 1987). These good reasons might include criteria such as theoretical coherence, reproducibility, triangulation of methods, confirmation by different experimental techniques, successful prediction of new phenomena, etc. (Roth and Barrett, 1990).

These realist critiques provide a useful antidote to extreme versions of social constructivism. However, there is a danger here of becoming trapped in a series of unproductive confrontations between extreme versions of realism and constructivism, so that what are in fact the real gains from constructivist studies are lost. The approach I favour, ‘realist-constructivism’ (Hess, 1997), tries rather to build on the strengths of both perspectives.

c Realist-constructivism: Whilst rejecting extreme social constructivism, a realist-constructivism recognizes the profound importance of social factors. There is clearly no possibility of any socially unmediated access to external reality, natural or social, and scientific practice is a social activity shaped by a multiplicity of social and psychological factors, biases and interests. Social factors are involved in science at all levels. However, accepting this does not mean we have to conclude that social factors are all there is, in the sense that the consensus practices of scientists would have turned out the same regardless of any input from nature. Science may depend upon social institutions for its existence, but not for its truth (Nanda, 1997). Although we can only gain access to the world through our cultural and social categories, that does not mean that we cannot critically revise and transcend those categories through a kind of ‘conceptual bootstrapping’ in which we modify our conceptual categories in the light of evidence (Nanda, 1997). A realist-constructivism is thus based on a recognition of a constant interplay between social factors and pre-existing structures of reality.

A detailed exposition of such a viewpoint can be found in Kitcher (1993). Kitcher accepts that we can have no unbiased access to reality, but he argues that the history of science itself suggests that our biases are not so powerful that they prevent us working our way out of false beliefs. He thus rejects the view that nature has negligible impacts on our beliefs and that a consensus is entirely socially determined. Scientific debates, he argues, are indeed often closed through ‘decisive arguments’. Social factors may retard a decision, but not reverse it (a point he illustrates at length with examples from Darwinism, geology and astronomy). Such examples from the history of science illustrate the way in which ‘society, nature and sound individual
reasoning combined to drive the social learning machine to a new success’ (Kitcher, 1993: 218).

Science thus develops through successive ‘consensus practices’ which are progressive in so far as they involve the acquisition of significant truths, based upon a better conceptualization and categorization of experience, and improved ideas about natural groupings. ‘Conceptual’ progress involves shifts that improve the reference potential of key terms, and ‘explanatory’ progress involves improvements in our view of dependencies between phenomena. For Kitcher, the development of Darwinism since 1859 provides an extended illustration of such a model of progress.

In summary, Kitcher’s work provides an illustration of how one might recognize the profound influence of social forces whilst not abandoning traditional epistemological concerns and questions of progress. Nevertheless, I have some sympathy with those who feel that Kitcher’s position remains too close to traditional philosophy of science and is still not sensitive enough to sociological and constructivist arguments (Solomon, 1995). In particular, I think that realist ideas need some refinement in the light of work within the sociology of scientific knowledge which has revealed many of the hidden realities of scientific practice. My preferred version of realism is close to that defined as ‘pragmatic realism’.

This is a term used by Pickering (1995) in the context of a discussion of scientific practice as a dialectic of resistance and accommodation. What we encounter as resistences, he argues, are always situated with respect to particular scientific projects, theories, models, etc. Resistences emerge only in the real time of scientific practice, which is why he rejects the strong realist language of them being ‘already there’. Accommodation as a response to resistance involves tinkering with material procedures, developing interpretative models, etc., but it is always also situated with respect to goals, cultural resources, etc. A dialectic of resistance and accommodation unfolds, although this may lead to temporary, interactive stabilizations.

The notion of a dialectic of situated resistences and accommodations seems to me to be a useful one. But Pickering goes too far in resisting the notion of resistences as real, pre-existing structures which impose constraints on the possible outcomes of scientific work (Galison, 1995). Although the difference between Pickering and Galison may be interpreted as one of emphasis (Gingras, 1995), it is nevertheless an important difference in the context of discussions about realism and scientific progress.

3 Beyond constructivism and realism? The dubious promise of actor-network theory

My commitment to some form of realist-constructivism may appear futile to those who believe that rather than attempting to occupy some middle ground between two extremes we should think of ways of transcending the dualism entirely. This is a possibility held out by actor-network theorists such as Latour, Callon and Law who argue that actor-network theory provides a way of breaking out of a sterile confrontation between realism and constructivism. Thus Latour (1988a; 1992; 1993) rejects realism and the notion of science as the representation of the real. At the same time, however, he also rejects the extremes of social constructivism. The one approach makes nature do all the work of explanation, whilst the other makes society do all the work. Yet nature and society, he argues, are not stable, pre-existing entities; they only emerge after the
stabilization of a network. It is this focus on networks that provides the basis for a distinctive view of science and scientific progress.

Science, according to this view, involves the construction of networks which bring together heterogeneous elements, such as human and nonhuman actants, inscription devices, buildings, money, etc. (Callon, 1994). Networks are translation chains, involving processes of problematization, intérressement, enrolment and mobilization. The resulting networks vary in length and complexity. Statements which form part of the network do not talk of an outside reality; reference is nothing more than a network effect of a translation chain. As networks become stronger and more durable, knowledge claims become more accepted. The stronger the network, the harder the truth claim. Society and nature are thus network outcomes, not pre-existing essences. Only when debate has stopped and the network temporarily stabilized is Nature or Reality made the explanation. We only hear nature’s voice when all scientists agree; once disputes are settled we can all be realists. Then scientists have gathered sufficient allies to make dissent impossible.

In such a framework progress could be defined in terms of the extension and consolidation of the network, by establishing centres of calculation (laboratories, for example), making dissent costly or virtually impossible, and blackboxing more and more subnetworks so they become unchallenged and remain unopened. Knowledge accumulation becomes possible only when networks become stabilized and translation chains consolidated. Indeed, paradigms might be conceived as stabilized networks, with well-established centres of calculation and durable network links. Progress is thus a network outcome, a judgement that scientists pass on a network that has become stabilized.

The actor-network approach has already produced some interesting and provocative work (Latour, 1988b), and has attracted the attentions of some geographers (e.g., Demeritt, 1996; Murdoch, 1997). I am for now less convinced. For example, there seem to be major problems with the concept of ‘nonhuman agency’ and the claimed symmetry between humans and nonhumans (Jones, 1996). Can nonhuman entities be actors if they lack goals and intentions? Also, the actor-network model may seem philosophically radical, but its use is essentially conservative (Collins and Yearley, 1992). In recommending that we follow scientists as they build networks it provides a descriptive language but one without explanatory power. It is not very good at explaining why some actors are excluded from networks, and studies so far have been weak on factors such as class, race, gender, etc. It also fails to distinguish the categories of the actors and those of the social analyst. But analysis cannot remain at the level of repeating the actors point of view as expressed in their discourse (Gingras, 1995). It also appears that reality, nature, truth and progress get defined in a struggle for power in which scientists tend to win because they have more rhetorical and material resources. To some critics this appears to lead towards an extreme form of idealism and a coherence theory of truth with attendant problems as acute as simple correspondence theories (Nanda, 1997). To others, it has to be said, the very ambiguities in actor-network theory seem to leave room for realism to be smuggled back in (Gingras, 1995).

In summary, I am not yet convinced that actor-network theory delivers on its promised transcendence of the realism versus constructivism debate. I prefer to remain in the complex, messy, but fertile middle-ground explored by realist-constructivism.
VI Back to progress: arguments for social epistemology

I want to end in this final section on a more normative note. If, as constructivist studies suggest, social and institutional factors are so important to scientific practices, it is worth asking what social and institutional configurations we should put in place to maximize the prospects for scientific progress. It is these kinds of questions that are being addressed within the developing field of social epistemology (e.g., Fuller, 1988; Radder, 1992; Resnick, 1996).

Kitcher poses the problem in this way. Progress is the outcome of a well organized scientific community, functioning as a kind of social learning machine. What then are the properties that would make such a community an epistemically well designed social system, in the sense that it is capable of producing progressive sequences of consensus practices? One approach is to model the dynamics of different kinds of scientific community, trying to identify the optimal organization of cognitive labour. To illustrate this Kitcher builds a series of simple models of scientific communities in order to establish the equilibrium distributions of epistemic effort under various types of social conditions. His models, ‘inspired by Bayesian decision theory, microeconomics, and population biology’ (Kitcher, 1993: 305), incorporate different kinds of individual strategies which are subject to cost and time constraints. Thus some scientists may want to maximize their chances of being right (the epistemically pure); others may want to maximize their chances of being first to be right (the epistemically sullied). In pursuing their different strategies individuals have to weigh up the utility of strategies such as relying on authority and borrowing results from others, as opposed to checking, replicating or finding their own solutions. Such choices involve calibrating the authority of others in various direct or indirect ways. These choices can be modelled through decision trees and payoff strategies.

These simple models can then be used to explore various aspects of community dynamics, such as the optimum number of replicators, and optimum levels of cognitive diversity. As Kitcher shows, such models can lead to some surprising results, such as the evidence that ‘sullied communities’ may achieve a cognitive distribution with a higher probability of success than epistemically pure communities. This suggests that the influence of social motives in science need not therefore always be a problem; ‘particular kinds of social arrangements make good epistemic use of the grubbliest motives’ (Kitcher, 1993: 305).

Such models are interesting, as far as they go, but they are still very simple and in many ways unrealistic. As Hands (1995) points out, they are also rooted in ‘invisible hand’ type arguments. Like neoclassical economic models they seem to show that ruthless egotism and the pursuit of individual self-interest can lead to socially desirable outcomes. As with neoclassical models, however, these desirable outcomes depend on highly restricted assumptions that are unlikely to operate in practice. In particular, it seems to me that scientific communities also require higher levels of self-conscious reflexivity than are allowed for in these simple models.

Arguments for greater reflexivity have become common in the social sciences, but much of the debate centres around the reflexivity of the individual inquirer, reflecting on his or her own assumptions and biases (e.g., Pinch and Pinch, 1988; Woolgar, 1988; Ashmore, 1989). But reflexivity needs to be seen in more collective terms, as an achievement of a scientific community. Such deeper forms of reflexive awareness may
causally depend upon certain social relations, such as ‘an ethos of co-operation tinged with competition’ (Nanda, 1997: 342).

Such arguments for communal reflexivity have been made by some feminist critics of science who nevertheless wish to strengthen science rather than deconstruct or displace it. Longino (1990) and Harding (1991; 1993a; 1993b) both recognize the importance of social factors, interests and biases in scientific research but also stress the importance of going on to uncover background assumptions and biases through a transformative criticism of the contextual values within which science always takes place. This continual exposure of unthought biases and assumptions is a way of producing a ‘strong objectivity’ which strengthens science as a progressive project. Such reflexivity requires commitment to certain principles of organization and debate. Here Longino’s criteria for ensuring the possibility of unforced agreement echo Habermas’s criteria for an ideal speech community.

However, as I have argued elsewhere, this notion of reflexivity needs further deepening, in the way suggested by Bourdieu (Bassett, 1994; 1995; 1996). Bourdieu’s institutionalized reflexivity is a process aimed at uncovering the collective unconscious of a discipline, leading not to the deconstruction of science but to a more emancipatory science. In this sense, reflexivity is ‘a tool to produce more science not less’ (Bourdieu and Waquant, 1992: 194). It is through such deep reflexivity in a structured scientific field that the conditions for scientific progress actually become institutionalized.

VII Conclusions

I have tried to explore critically a number of areas of philosophical and sociological inquiry which have a direct bearing on debates about the nature and possibility of social scientific progress. I have at least convinced myself that such debates about progress can and must be kept alive in the face of extreme forms of philosophical and sociological constructivism. The kind of framework with respect to geography which I favour I have briefly outlined in my discussion and critique of Johnston’s approach, but my suggestions clearly need much more development than I have given them here. Finally, I hope my references to social epistemology will raise normative questions on possible links between progress and organization of social scientific communities.

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