

The Royal Road: Marxism and the Philosophy of Science

What is a consequent Marxist view of the history and philosophy of science? Reference to the work of Marx and Engels (or even of Lenin) will not yield a satisfactory answer, although certain signposts are evident. For example, there is the famous observation on method in the Introduction to the *Grundrisse*, which argues that, contrary to the procedures adopted in classical economy, where the starting point for investigation is apparently concrete phenomena from which abstract theoretical descriptions are then derived, 'the method of rising from the abstract to the concrete is the only way in which thought appropriates the concrete, reproduces it as the concrete in the mind.'¹ Or there are Engels's late works, pre-eminently *Anti-Dübring* and *Dialectics of Nature*, in which the so-called laws of the dialectic are laid out schematically, and of which it is asserted that they constitute 'the science of the general laws of motion and development of nature, human society and thought'.² Or there is Lenin's critique of positivism in *Materialism and Empirio-Criticism*, on which the later works of Althusser would depend so

heavily for their justification of philosophy's role in relation to science.

Post-classical Marxism has been remarkably fecund in its treatment of epistemological themes and in elaborating competing versions of the Marxist theory of knowledge, emphasizing different passages or moments in Marx's (less often Engels's) corpus to buttress its claims for the authentically Marxist character of the theory. Western Marxism in particular, from Lukács, Korsch and Gramsci to Adorno, Della Volpe, Sartre and Althusser, has productively developed Marxist epistemology to the point that, if serious disagreements remain, it is nevertheless possible to assess Marxist philosophy of science and—to appropriate a famous metaphor—discover the rational kernel inside the mystical shell.

Such I take to have been the project of Roy Bhaskar over the past decade and a half, although the specifically Marxist pedigree of his work has only gradually become evident. (Marxism finds no place in *A Realist Theory of Science*, for example, his first, and still fundamental, book.)³ It is at any event fully evident in his most recent collection, *Reclaiming Reality*, which contains, among other riches, perhaps the finest brief historical and methodological assessment in English of the major issues in Marxist philosophy.⁴

Philosophical Underlabouring

What is the task of philosophy of science in Bhaskar's view? It lies, to cite the Lockean metaphor on which he has come increasingly to repose, in 'underlabouring' on behalf of the sciences. Underlabouring entails clarifying and explicating what it is the sciences do and how they do it, as well as, on occasion, criticizing existing scientific practices for failing to meet the standards of scientificity they set for themselves. Philosophical underlabouring (the proposed title for a planned further collection of essays; see RR, p. 208 n. 32) thus proposes a philosophy *of* science (what Bhaskar terms 'transcendental realism', the strong research programme first announced and elaborated in RTS) that is at the same time a philosophy *for* science (what Bhaskar is now willing to call 'critical realism'; RR, pp. vii, 190). But why should the sciences need a philosophy at all? What is to be gained, in the first instance for science but in the end for humankind generally, from a coherent account of what Rom Harré has called 'the principles of scientific thinking'?

Bhaskar's justification of his own enterprise is as follows:

¹ Karl Marx, *Grundrisse*, trans. Martin Nicolaus, New York 1973, p. 101.

² Friedrich Engels, *Anti-Dühring*, part 1, ch. 13.

³ Bhaskar has maintained, however, that this text was influenced by Marxism, notably by Althusser; see Gregory Elliott, *Althusser: The Detour of Theory*, London 1987, p. 331.

⁴ Roy Bhaskar, *Reclaiming Reality: A Critical Introduction to Contemporary Philosophy*, Verso, London 1989 (£11.95 pbk, \$34.95 hbk), ch. 7; hereafter cited parenthetically in the text as RR. Other texts by Bhaskar cited parenthetically include: *A Realist Theory of Science*, 2nd edn., Sussex 1978—RTS; *Scientific Realism and Human Emancipation*, Verso, London 1986—SHRE.

The essays collected in this volume all seek to *underlabour*—at different levels and in different ways—for the sciences, and especially the human sciences, in so far as they might illuminate and empower the project of human self-emancipation. They attempt, that is to say, for the explanatory-empiricist sciences of today, the kind of ‘clearing’ of the ideological ground, which Locke set out to achieve for the prodigious infant of seventeenth-century mechanics. Such sciences, which only partially and incompletely exist, will not only interpret but help to change the world. But they will do so rationally only on the condition that they interpret the world aright. (RR, p. vii)

Or, as he opines some pages later in a gloss on the eleventh of the *Theses on Feuerbach*: ‘The world cannot be rationally changed unless it is adequately interpreted’ (RR, p. 5). Critical realism is therefore ‘a necessary but insufficient agency of human emancipation’ (RR, p. 191). This, as Bhaskar himself observes, is at one with Marx’s conception of the theory/practice relation, at once virulently anti-idealist and anti-voluntarist (RR, pp. 128, 137). Critical realism is therefore not just an optional attainment for socialists; it undergirds the production of knowledge that enables their political practice. Why should this be so?

Bhaskar’s technical justification for this view is given in chapters 5 and 6 of *Reclaiming Reality* (RR, pp. 66–114), which reprise the positions of his two earlier books, *The Possibility of Naturalism* and *Scientific Realism and Human Emancipation*, respectively. But since a more accessible account is presented in chapter 8, a long essay on Richard Rorty, I shall focus on this text.

Rorty: Disabling the Human Sciences

Bhaskar’s choice of Rorty as antagonist is doubly motivated. First, Rorty’s prestige has grown steadily, both in and out of the philosophical community, ever since the publication of *Philosophy and the Mirror of Nature*.⁵ He is now one of those philosophical figures whose views matter and whose writings, as a consequence, are ceaselessly criticized, debated, elaborated in and out of professional philosophy. Moreover, as Bhaskar observes at the outset, the position Rorty has staked out is in many ways representative of an emergent orthodoxy in philosophy—perhaps best characterized as ‘post-empiricist philosophy of science’—that Bhaskar has ceaselessly criticized over the years.

A second reason for sustained treatment of Rorty is the latter’s increasing preoccupation with the domain marked out by the human sciences. If *Philosophy and the Mirror of Nature* attempted to map the terrain of traditional epistemology differently, the later essays in *Consequences of Pragmatism*⁶ and virtually the whole of *Contingency, Irony, and Solidarity*⁷ evince Rorty’s conviction that what he once termed ‘edifying philosophy’ must cash out its claims in the domain of ethics and, pre-eminently, politics. As Bernard Williams judiciously put it in his

⁵ Princeton 1979.

⁶ Minneapolis 1982.

⁷ Cambridge 1989.

review of *Contingency, Irony, and Solidarity*, Rorty's aim is 'to give liberalism a better understanding of itself than it has been left by previous philosophy'.⁸ The real stakes in Rorty's project are political, although the limitations it exhibits in this area derive from (in the sense of hanging together philosophically with) his continuing entanglement in a certain epistemological problematic. Bhaskar summarizes the matter nicely:

[I]t is Rorty's ontology which is responsible for his failure to sustain an adequate account of agency and a fortiori of freedom as involving inter alia emancipation from real and scientifically knowable specific constraints rather than merely the poetic redescription of an already-determined world. (RR, p. 146)

In short, Rorty's project disables the human sciences; hence, on Bhaskar's view, it deprives human beings of a necessary (if insufficient) instrument by which they might become free.

Bhaskar's critique can be divided into two parts. The first, sections 1–3 in the essay, goes over ground familiar to readers of his previous work, especially RTS. It rehearses themes Bhaskar has emphasized and theses he has urged in the philosophy of science, particularly in regard to the epistemology and ontology of the natural sciences. These themes (the ontic fallacy, the epistemic fallacy, the failure to distinguish between intransitive and transitive objects of knowledge) are replayed in a close explication of Rorty's work (primarily *Philosophy and the Mirror of Nature*). It would be impossible to summarize Bhaskar's argument, so I shall simply cite his conclusion here:

[J]ustifications within science are a social matter—but they require and are given ontological grounds. In failing to recognize this, Rorty has furnished us with a post-epistemological theory of knowledge without justification which matches his account of science without being. The result is just the opposite of what he intended: the epistemologization of being and the incorrigibility (uncriticizability) of what passes for truth. (RR, p. 160)

Rorty would certainly be surprised by this last charge, since his entire effort for nearly two decades has been to show that truth claims are always subject to criticism, or, as he now puts it, to redescription. A passage near the beginning of *Contingency, Irony, and Solidarity* captures the essence of this view:

To say that truth is not out there is simply to say that where there are no sentences there is no truth, that sentences are elements of human languages, and that human languages are human creations.

Truth cannot be out there—cannot exist independently of the human mind—because sentences cannot so exist, or be out there. The world is out there, but descriptions of the world are not. Only descriptions of the world can be true or false. The world on its own—unaided by the describing activities of human beings—cannot.⁹

On the face of it, this seems a pithy statement of the distinction

⁸ 'Getting it Right', *London Review of Books*, 23 November 1989, p. 5.

⁹ *Contingency, Irony, and Solidarity*, p. 5.

between transitive and intransitive objects of knowledge—roughly, ‘sentences’ and ‘the world’. In fact it is not, for reasons Bhaskar makes plain.

Rorty’s apparent commitment to a realist ontology is characteristically hidden by his systematic exploitation of an ambiguity in his terms. Bhaskar cites the claim made in *Philosophy and the Mirror of Nature* that ‘physics gives us a good background against which to tell our stories of historical change’ and comments thus:

If physics means ‘the physical world’ as described by [the science of] physics (hereafter physics_{id}—or the physical world), then it is true and unparadoxical. If, however, physics means ‘the set of descriptions’ of the physical world in the science of physics (hereafter physics_{sd}—or the science of physics), then as a rapidly changing social product it is part of the process of historical change and so cannot form a background to it. (RR, p. 151)

A similar ambiguity in Rorty’s use of the term ‘cause’ in *Contingency, Irony, and Solidarity* vitiates the axial notion of creative redescription on which that book turns (RR, pp. 151–2).

Bhaskar’s critique shows how such ambiguities are an absolute requirement of Rorty’s programme in philosophy, for they underwrite his fundamental conviction that an irreducible cleft divides the *Naturwissenschaften* and the *Geisteswissenschaften* (RR, p. 165). This in turn is a consequence of his thoroughgoing empirical actualism coupled with his attachment to the possibility of human freedom. Bhaskar puts the matter well:

The autonomy of the social and other less physicalistic sciences is rendered consistent with a comprehensive empirical actualism by allowing that physics (or the physical sciences) can describe every bit of the phenomenal world but that some bits of it, for instance the human, can also be truly redescribed in a non-physicalist way. (RR, p. 164)

This unresolved antinomy—for such it is, and none other than Kant’s famous Third to boot, as Bhaskar notices (RR, p. 164)—will come to underwrite the celebration of contingency in Rorty’s most recent book, which does no more than elaborate on the Sartrean point already made in a footnote to *Philosophy and the Mirror of Nature*: ‘man is always free to choose new descriptions (for, among other things, himself).’¹⁰ Are we?

Yes and no. Patently, there is much about human beings as moral and political—not just physical—beings that they have been bequeathed and that they are not in any obvious position to change or, save in fantasy, redescribe. Workers are exploited under capitalism (and other modes of production); Blacks in South Africa, and Palestinians in the West Bank and Gaza, are systematically deprived of civil and political liberties; women everywhere continue to be subjected to various forms of social discrimination. None of these groups of people ameliorates the actuality of their situation just by ‘creatively

¹⁰ *Philosophy and the Mirror of Nature*, p. 362.

redescribing' it in better terms. In fact, it can be shown that such redescrptions characteristically sustain or even worsen the lot of those structurally prevented from exercising personal or collective power. For example, when workers accept the basic conditions of the capital–wage–labour relation (along with its attendant juridical legitimation) by labelling it free (if unequal) exchange, they deprive themselves of the capacity to resist wage reductions when profits decline beyond a point capitalists consider acceptable. Or, to take a related instance, while certain short-term material gains may be obtained for workers by 'creatively redescribing' their relation to capital in purely economic (that is, contractual) terms, the long-term tendencies towards instability in the capitalist system make this a mug's game in which workers ultimately remain at the mercy of their employers, having ceded the power to control their fate to those whose interests are objectively opposed to theirs (a lesson painfully learned by the working classes in Western Europe and the USA since the 1970s). As Marx once pithily observed, the freedom to sell or withhold one's labour power is precisely the freedom to starve in the streets.

Freedom as a Regulative Ideal

Rorty, of course, thinks that such freedom to engage in fantasy is productive and must be protected. But there is the rub: how do we get from the plausible idea that human beings are by nature free (if conditionally so) to the state of actual freedom? This is a problem that Rorty's liberal recommendations cannot solve, and not just for narrowly political reasons. Bhaskar shows how Rorty's liberalism is entailed by his ontology, how a flawed politics is underwritten by a wrong-headed philosophical programme. Where does Rorty (along with his hero Mill) go wrong?

First, Rorty's exemption of human beings from the absolute constraints characteristic of nature and described in the physical sciences does not sufficiently recognize the '*sui generis* reality and causal efficacy of social forms' (RR, p. 174). He cannot do so because he systematically undervalues (or misdescribes) the nature of 'objective social structures (from languages to family or kinship systems, to economic or state forms), dependent on the reproductive and transformative agency of human beings'.¹¹ As Bhaskar goes on to observe:

These social structures are concept-dependent, but not merely conceptual. Thus a person could not be said to be 'unemployed' or 'out of work' unless she and the other relevant agents possessed some (not necessarily correct or fully adequate) concept of that condition and were able to give some sort of account of it, namely, to describe (or redescribe) it. But it *also* involves, for instance, her being physically excluded from certain sites, definite locations in space and time. That is to say, social life always has a material dimension (and leaves some physical trace)... (RR, p. 174)

This is again the point about inherent limitations on redescription. The problem with Rorty's conception of freedom is that, like Kant's,

¹¹ A similar criticism has been lodged against Rorty by Alexander Nehamas; see the latter's 'A Touch of the Poet: On Richard Rorty', *Raritan*, vol. 10, no. 1, Summer 1992, pp. 104–26.

it is merely regulative; such an ideal tells us little if anything about the objective constraints that operate on humankind in society and in nature to make certain courses of action at best unlikely or at worst impossible.

In opposition to the Rorty–Kant regulative ideal of freedom, Bhaskar proposes the concept of human emancipation, which, he observes, entails:

(1) a stronger sense of being ‘free’, namely as knowing, possessing the power and the disposition to act in or towards one’s real interests . . . ; and

(2) a stronger sense of ‘liberation’, namely as consisting in the transformation of unneeded, unwanted and oppressive to needed, wanted and empowering *sources* of determination.

Emancipation, that is to say, depends upon the transformation of structures rather than just the amelioration of states of affairs. And it will, at least in the case of self-emancipation, depend in particular upon a conscious transformation in the transformative activity or praxis of the social agents concerned. As such, emancipation is *necessarily* informed by explanatory social theory. (RR, p. 178).

There is, to be sure, a role for creative redescription in changing the conditions that render human beings unfree, but it is strictly dependent upon prior explanations of the social structures that are the underlying causes of unfreedom. To provide such explanations is the function of the (emancipatory) human or social sciences, which on this construal are neither methodologically distinct from, nor conceptually opposed to, but take their place alongside (and contribute to the emancipatory project of—in part by criticizing activity in) the physical sciences. The potential for there to be more Rortean liberal ironists who hold that their descriptions of the world are always contingent—or, better, revisable—thus depends for its realization on a, possibly unironic, commitment to the truth-value—that is, the real descriptive power—of social theory. Or, to adapt a famous sentence from Kant, we shall have to embrace reason in order to preserve poetry.

Althusser and the Production of Knowledge

Near the end of *Reclaiming Reality*, Bhaskar lays at Althusser’s feet responsibility for the sins of some of the latter’s more prominent British offspring, charging that the French philosopher’s ‘failure to give any apodeictic status to the real object rendered it as theoretically dispensable as a Kantian thing-in-itself and helped to lay the ground for the worst idealist excesses of post-structuralism’ (RR, p. 188).¹² The passage refers the reader to a fuller elaboration of this claim in *Scientific Realism and Human Emancipation*, which I shall consider in a moment. At stake here is not merely the scholastic question of whether or not Bhaskar has got Althusser right—though this is far from a

¹² Cf. the similar judgment made in Bhaskar’s encyclopedia article on the Marxist theory of knowledge: ‘In Althusser one finds . . . a form of scientific rationalism influenced by the philosopher of science G. Bachelard and the meta-psychologist J. Lacan, in which the intransitive dimension is effectively neutralized, resulting in a latent idealism’ (RR, p. 142).

trivial matter, given the importance (conceded by Bhaskar; see RR, p. 187) of Althusser's corpus to contemporary Marxism. The substantive issue concerns what are licit views in the philosophy of science from the standpoint of historical materialism. To anticipate, I shall be arguing that if Bhaskar is perhaps correct to chastise Althusser for undertheorizing the intransitive dimension in knowledge production, it may be said that Bhaskar's own comparative slighting of the ways in which ideology permeates the transitive dimension risks the charge of metaphysical dogmatism, which, in the current ideological *Kampfplatz* of philosophy, is perhaps the greater danger to the success of the realist programme.¹³ Althusser's 'scientific rationalism' (RR, p. 142) is more than matched by Bhaskar's rationalist faith that in philosophy the better argument and in science the superior hypothesis will necessarily carry the day.

Bhaskar's critique of 'the British post-Althusserians' (SHRE, p. 237) comes in the midst of a detailed examination of positivism and of its continuing legacy in the philosophy of science. He locates twin, symmetrical dangers in rationalism of the Popper–Lakatos sort and in empiricism. A corollary of the latter position, he suggests, is the Feyerabend–Bachelard line 'that philosophy should have no effect on science', on which view positivism has always depended. The odd thing, however, is that this latter has mutated, in the hyperempiricism of Hindess and Hirst, into a classical idealism, and this, Bhaskar avers, is the more or less inevitable result of Althusser's underemphasis on 'the real object':

An account that cannot think the necessity for both, and the irreducibility of, the concepts of thought and being . . . must lapse into idealism where concepts are part of being. The origin of these errors is clear. It lies in Althusser's initial inadequate theorisation of the concepts of the 'real object' and the 'thought object'. His failure to provide an apodeictic status for, or indeed give any real function to, the former rendered it as disposable as a Kantian *ding-an-sich*—a service duly performed, against the continuing materialist letter of Althusser's texts . . . (SHRE, pp. 237–8)

Althusser's own commitment to materialist (or, in Bhaskar's terms, transcendental realist) ontology is thus not in doubt; rather, his declension of knowledge production (the infamous Generalities I, II and III) gives insufficient weight to what he calls 'the real-concrete' (*concret-réal*), as opposed to the 'concrete-in-thought' (*concret-de-pensée*). Of these latter Bhaskar writes:

This does not correspond to the realist distinction between the intransitive and transitive objects of knowledge. For while, for the realist viewing knowledge in the transitive dimension as a process of production, the transitive object of knowledge may be said to correspond to Althusser's

¹³ I am not saying that such a change is warranted, but it is plain that Bhaskar's insistence on the irreducibility of the intransitive dimension tends to be construed thus; see, for example, Bruno Latour and Steve Woolgar, *Laboratory Life: The Construction of Scientific Facts* (1979), Princeton 1986, p. 178. In the current euphoria over constructivist accounts of scientific discovery, something more than a firm distinction between transitive and intransitive dimensions is required to make the case against a totally socialized view of the nature of theories.

Generalities I, the intransitive object of knowledge—what is known in and via this production process—is precisely the real object. It does not follow from the fact that we can only know in knowledge that we can only know knowledge! (or even knowledge of knowledge would be impossible). (RR, p. 188)

I shall make two comments. First, the transitive dimension is not confined only to Generalities I, but is fully constituted by Generalities I, II and III. These are, respectively: the raw materials (observational data, previous hypotheses, ideologies, and so forth) on which science works (Generalities I); the existing body of scientific theory that works on the raw materials (Generalities II); and the knowledge (new hypotheses) that is the outcome of this process of knowledge production (Generalities III). The latter then become part of Generalities I and II in the ongoing process of scientific inquiry described so aptly by Bhaskar (see RR, pp. 19–20). Second, Althusser is perfectly explicit about knowledge itself (it is always knowledge *of* things, including the theoretical things that give knowledge of the real), nowhere more than in *Reading Capital*:

No doubt there is a relation between *thought-about-the-real* and this *real*, but it is a relation of *knowledge*, a relation of adequacy or inadequacy of knowledge, not a real relation, meaning by this a relation inscribed in *that real* of which the thought is the (adequate or inadequate) knowledge. This knowledge relation between knowledge of the real and the real is not a relation *of the real* that is known in this relationship. The distinction between a relation of knowledge and a relation of the real is a fundamental one: if we did not respect it we should fall irreversibly into either speculative or empiricist idealism.¹⁴

Nothing in the Althusserian account of knowledge production is at odds either with Bhaskar's general conception of the transitive dimension, or with his commitment to the ontological priority of the relatively enduring real structures or mechanisms described in scientific laws. If Althusser has comparatively little to say about the 'real-concrete', this is just because he (perhaps wrongly) conceives that to be the exclusive prerogative of the sciences. The real is the object of scientific discourse; philosophy's task, as we shall see, lies elsewhere.

However, Bhaskar has another bone to pick with Althusser, one that follows naturally enough from what is termed Althusser's 'scientific rationalism':

Although opposed to any reduction of philosophy to science or vice-versa,

¹⁴ Louis Althusser, 'The Object of *Capital*', in Althusser and Balibar, *Reading Capital*, trans. Ben Brewster, London 1970, p. 87; hereafter cited parenthetically as RC. Cf. the following passage from a text of 1966, 'On Theoretical Work: Difficulties and Resources': 'Naturally, the knowledge of formal-abstract objects [the objects posited by theory—MS] has nothing to do with a speculative and contemplative knowledge concerning 'pure' ideas. On the contrary, it is solely concerned with *real* objects; it is meaningful *solely* because it allows the forging of theoretical instruments, formal and abstract theoretical concepts, which permit production of the knowledge of real-concrete objects'. Louis Althusser, *Philosophy and the Spontaneous Philosophy of the Scientists and Other Essays*, trans. Ben Brewster et al., London, p. 51; hereafter cited parenthetically in the text as PSPS.

in maintaining that criteria of scientificity are completely intrinsic to the science in question, Althusser leaves philosophy (including his own) without any clear role; in particular, the possibilities of any demarcation criterion between science and ideology, or critique of the practice of an alleged science, seem ruled out. (RR, p. 143)

Bhaskar's reference here, one presumes, is to the self-confessed theoreticism of the early (circa 1965) Althusser. It is somewhat surprising, then, that he will subsequently assert his preference for the texts of this period over the transitional *Philosophy and the Spontaneous Philosophy of the Scientists* and the later texts of *auto-critique*. More surprising still is that Bhaskar had recognized quite early on, in his essay on Feyerabend and Bachelard,¹⁵ the originality and validity of Althusser's new position in and on philosophy that was announced programmatically in *Lenin and Philosophy* (PSPS, pp. 167–202), but is more thoroughly elaborated in his lectures on philosophy and science. We now turn to these latter to consider what it means to be a Marxist philosopher of science. That this is no simple task should go without saying.

A Philosophy For Science

It will be recalled that the Althusser of *For Marx* and *Reading Capital* characterized philosophy, more specifically Marxist philosophy (dialectical materialism), as 'the Theory of Theoretical practice', a definition, he shortly recognized, that proposed 'a unilateral and, in consequence, *false* conception of dialectical materialism' (RC, p. 321). This 'false conception' was more than anything else the warrant for the charge of 'scientific rationalism', particularly in the relationship it asserted between theoretical work and ideological struggle. This latter is made explicit in a text of 1965, 'Theory, Theoretical Practice, and Theoretical Formation: Ideology and Ideological Struggle', where Althusser writes:

It is *theoretical formation* that governs ideological struggle, that is the theoretical and practical foundation of ideological struggle. In everyday practice, theoretical formation and ideological struggle constantly and necessarily intertwine. One may therefore be tempted to confuse them and misjudge their difference in principle, as well as their hierarchy. This is why it is necessary, from the theoretical perspective, to insist at once on the *distinction in principle* between *theoretical formation* and *ideological struggle*, and on the *priority in principle of theoretical formation* over ideological struggle. (PSPS, p. 38)

Now in one sense this is unobjectionable and in fact follows from Lenin's slogan about the relationship between revolutionary theory and revolutionary practice. But in another sense it is entirely wrong, for it seals off theory (here historical materialism, but at this stage in Althusser's career, philosophy as well) from the domain of empirical confirmation (or refutation), which is what ideological struggle can in principle provide. Althusser is indeed guilty at this period of the 'theoreticism' (more properly, speculative idealism) with which he has been charged.

¹⁵ See RR, p. 48; the essay was originally published in 1975.

The shift in Althusser's thinking, whatever its punctual determinations,¹⁶ would be clear by the time he prepared his lectures on philosophy and science. It is there from the outset in: (1) a sharp distinction between the procedures of the sciences and those of philosophy (PSPS, pp. 77, 81); (2) a clear stipulation of the relationship between philosophy and ideology (p. 83); (3) the attribution of a 'spontaneous ideology of scientific practice' to scientists (p. 88); and, finally, (4) a severe delimitation of the task of philosophy, which intervenes in scientific (and other) practice, not to guarantee its scientificity (as had been proposed in the earlier definition of philosophy), but to 'remove obstacles' in the path of scientific progress (p. 100). As becomes increasingly evident from the second lecture onwards, while Althusser's first definition of philosophy had indeed proposed a philosophy of science (and, correlatively, of the scientificity of philosophy itself), the new definition of philosophy would be effectively a philosophy for science: 'Scientists should above all count on their own forces: but *their* forces are not a matter for them alone; a good proportion of these forces exists *elsewhere*—in the world of men, in their labour, their struggles and their ideas. I will add: philosophy—not just any philosophy, not that which exploits the sciences, but that which serves them—plays, or can play, a role here' (PSPS, p. 112). In other terms, Althusser conceives the task of philosophy as underlabouring on behalf of the sciences, particularly the human sciences (PSPS, pp. 89–91).

While these lectures never use the phrase, the conception of philosophy as 'the class struggle carried on at the level of theory' operates throughout them. Indeed, the very idea of a 'Philosophy Course for Scientists', of which Althusser's lectures were one segment, is premised on a notion of philosophy's stake in the class struggle, its necessarily partisan position, and its irreducibility to sheer scientific problem-solving. The political-ideological function of philosophy that would come to feature so prominently in many of Althusser's subsequent texts, from *Lenin and Philosophy* (1968) and 'Philosophy as a Revolutionary Weapon' (1968), to 'Is It Simple to be a Marxist in Philosophy?' (1975) and 'The Transformation of Philosophy' (1976), is mobilized here: (1) to show how ideologies continue to function within scientific practices; and, correlatively, (2) to recruit scientists into the materialist camp. The former claim is defended in a general way in the third lecture and in detail through a close explication of biologist Jacques Monod's inaugural lecture at the Collège de France, included as an appendix in the published text of *Spontaneous Philosophy* (PSPS, pp. 145–65). This latter compares favourably with Bhaskar's critique of positivism as a philosophical ideology (see RR, pp. 49–65, and SRHE, pp. 224–308 for a more sustained treatment).

I have been arguing that no significant theoretical differences separate Bhaskar and Althusser. Althusser's early texts are premised upon a realist philosophy of science (including the realist ontology on which

¹⁶ Gregory Elliott has argued for a political determination: to wit, Althusser's over-valuation of Maoism; see *Althusser: The Detour of Theory*, chs 4 and 5, especially pp. 194–7, which deal with Althusser's text of 1966, 'Sur la Révolution Culturelle'.

Bhaskar insists), while his later texts from *Spontaneous Philosophy* onwards declare for the task of philosophical underlabouring on behalf of the sciences (especially the human sciences). What, then, distinguishes these two projects to establish a Marxist philosophy of science?

Against the grain of much commentary,¹⁷ I should insist that Althusser's first major works undertake *in philosophy* but *for science* some of the major tasks specified by his second definition of philosophy. *For Marx* and *Reading Capital* were principally concerned with reconstructing the bases of historical materialism by criticizing the various ideological deformations that had come to inhabit its theoretical problematic since the time of Marx. While this may have appeared to many a sterile exercise in Marxology (for example, the celebrated emphasis on the 'epistemological break' between the humanist and the scientific Marx), Althusser's aim was plain enough: to distinguish between the scientific and the ideological elements that inhabited (and still inhabit —now more than ever!) Marxist theory, and thus to remove obstacles to progress in historical-materialist research.

Bhaskar acknowledges both the novelty and the importance of Althusser's project, while repeating the standard criticism that 'while Althusser wishes to insist against sociological eclecticism that the totality is structured in dominance, his own positive concept of structural causality is never clearly articulated' (RR, p. 143). This last seems a peculiar charge indeed, since so much of *Reading Capital* is devoted to this concept, either in defining it explicitly (see especially RC, pp. 186–9), or in establishing its provenance in Marx as the key to his mature, scientific concept of society. Nonetheless, Bhaskar puts his finger on a notorious difficulty in Althusser's reconstruction of historical materialism: to wit, how precisely to conjugate the concept of overdetermination (and its correlate, the relative autonomy of the superstructures) with the hypothesis of determination in the last instance by the economy.

Only a Philosopher

There is no point in dodging the issue: this puzzle was never solved conceptually by Althusser. But why would one anticipate that it should be? Two considerations exempt Althusser from serious culpability on this count. First, as Althusser was wont to insist, he was only a philosopher. Clarification of concepts is indeed in the domain of philosophy, but it is no charge of this discipline, whether one conceives it either as 'Theory of Theoretical practice' or as 'class struggle in theory', to specify in advance of empirical research what the full reach of scientific concepts will be. The second reason derives from the peculiarity of the term 'structure' itself as it functions in the empirical science in question: historical materialism. While the word is the same one used to designate the enduring mechanisms that give shape

¹⁷ Though by no means all. See, for example, Gregory Elliott's introduction to PSPS, and the 'Conclusion: Unfinished History' to *Althusser: The Detour of Theory*.

and order to nature, the concept as deployed in the social sciences is, as Bhaskar has tirelessly insisted, entirely different: 'Society, then, is an articulated ensemble of tendencies and powers which, unlike natural ones, exist only as long as they (or at least some of them) are being exercised; are exercised in the last instance via the intentional activity of human beings; and are not necessarily space-time invariant' (RR, p. 79). This is to say, particularly in light of the last clause, that the only means for specifying the concept of structural causality is just to do scientific work on given social formations.¹⁸

The Althusserian reprise of this line of argument runs as follows. All societies are structured by modes of production 'visible only in their effects'; the determination of the structure by its effects—that is, the hierarchy of the material practices (canonically, the economic, the political and the ideological)—will vary not only from one mode of production to another but from social formation to social formation. This being so, structural causality can have no other theoretical significance than to distinguish the Marxist concept of the mode of existence of society from its empiricist and holistic historicist rivals; its further specification is an entirely empirical matter. As Althusser observes:

[J]ust as there is no production in general, there is no history in general, but only specific structures of historicity which, since they are merely the existence of determinate social formations (arising from specific modes of production), articulated as social wholes, have no meaning except as a function of the essence of those totalities, i.e., of the essence of their peculiar complexity. (RC, pp. 108–9)

The realist concept of structural causality licenses no more (but also no less) far-reaching conclusions for historical materialism than this.

Realism and Scientific Socialism

I claimed earlier that while one could provisionally grant the justice of Bhaskar's charge that Althusser had undertheorized the intransitive dimension of science, the opposite defect is to be observed in Bhaskar's own comparative slighting of certain aspects of the specific mechanisms operative in the transitive dimension. I have argued that Althusser's reconstruction of historical materialism aimed at just the sort of philosophical underlabouring on behalf of the human sciences recognized by Bhaskar as among the most urgent of philosophy's current tasks, and, further, that Althusser's reticence in developing the concept of structural causality prudently observes the limits of philosophical discourse in relation to the empirical sciences. On the face of it, nothing in the Bhaskar corpus to date can rival the contribution to a specific, existing scientific practice of Althusser's labours

¹⁸ In fact, there was an attempt from within Althusserianism to extend the concept of structural causality by giving a theoretical account of how it functions in explanations of the transition from one mode of production to another. This was none other than Balibar's 'The Basic Concepts of Historical Materialism' (RC, pp. 199–308). Despite Balibar's assertion of the openness of the problems he poses (p. 308), the essay's failure to capture the specificity of historical materialist practice is patent; see Elliott, *Althusser: The Detour of Theory*, pp. 160–71.

on behalf of historical materialism. Bhaskar's philosophy of science is, in Marxist terms, beyond reproach. His underlabouring for science, however, has thus far been confined largely to various interventions against positivism and (more limitedly) relativism or absolute historicism; further, his critique has been conducted on almost exclusively intraphilosophical terrain (that is, it has been aimed at philosophers of science, not scientific researchers themselves). There are other and arguably more direct means for carrying on the class struggle in theory.

Two recent studies instance just such interventions at different levels: Andrew Collier's *Scientific Realism and Socialist Thought*¹⁹ and Stanley Aronowitz's *Science as Power*.²⁰ Collier's excellent, tough-minded little book attempts, among other things, to conjugate the works of Althusser and Bhaskar, as I have done here. In his view, the latter is a necessary corrective to the errors of the former: 'But Bhaskar's results have the advantage of being more determinate than Althusser's... It is clearer what Bhaskar's conclusions "permit" and "forbid" (as Popper would say), and also what it is about the practice of science that compels these conclusions. And this is not a matter of clarity of expression, but of a different practice of philosophy' (SRST, p. x). Agreed. But the demur that quickly follows—'However, I am less sanguine than Roy Bhaskar about the prospects of scientific knowledge in the human world', Collier observes—suggests why Althusser's 'different practice of philosophy' may, as I have suggested, be a necessary corrective to Bhaskar's incipient rationalism in the transitive dimension.

For example, Collier indicts Althusser for the '*fallacy of misplaced concreteness*', by which he means Althusser 'thinks that we can distinguish separate *practices* as ideological or scientific, rather than separate *aspects* of the same practice' (SRST, pp. 27–8). This claim patently ignores the import of Althusser's redefinition of philosophy after 1966, setting aside the whole of *Spontaneous Philosophy* and its examination of the intertwined aspects of ideology and science in scientific practice. The Althusserian concept of the 'spontaneous ideology of scientists' gives precise warrant for statements like the following: 'A given practice may produce science *and* ideology... in that it may simultaneously bring into the world objective scientific knowledge and theoretical ideology that stands in the way of science and perpetuates class rule' (SRST, p. 29). In the currently available corpus of Althusser's writings, the principal examples of how this inmixing of science and ideology can occur are: historical materialism itself, psychoanalysis, the work of Jacques Monod. As an eminent American sage once observed, you can look it up.

Collier's main achievement, however, does not lie in textual exegesis, either of Althusser or of Bhaskar; nor does it lie in the realization of a new conception of Marxist philosophy of science. It consists, rather, in cashing out the realist programme in relation to scientific socialism. The disrepute into which the latter term has fallen may on the

¹⁹ Hemel Hempstead 1989; hereafter cited parenthetically in the text as SRST.

²⁰ Minneapolis 1988; hereafter cited parenthetically as SP.

face of it make his project appear quixotic, but Collier is quite persuasive in showing how no other form of socialist thought (utopian, anarchist or reformist), much less various strands of liberalism, can deal adequately with the constraints upon social transformation imposed by capitalism. He conceives social theory as distinct from the sciences proper, dubbing the former an 'epistemoid', a theoretical discipline inextricably intertwined with social practice (SRST, pp. 126–53). This conception of social theory gives full weight to the *ceteris paribus* clauses that Bhaskar recognizes must be inserted into the equation

knowledge → transforming action

but typically underplays in his own practice of philosophy. As Collier wryly remarks after lucidly presenting the broad set of constraints that operate against social reproduction: 'A philosophical book is not the place to argue that this or that constraint on social reproduction exists; but it is on precisely such arguments that the case for socialism rests . . . Though naturally, good arguments will not pierce the ideological armour of many' (SRST, p. 177).

Ideological Struggle and Socialist Emancipation

At the outset of *Reclaiming Reality*, Bhaskar proposes an agenda for socialist thought: 'I take it that whatever our politics, in the narrow party or factional sense, socialists can agree that what we must be about today is the building of a movement for socialism—in which socialism wins a cultural-intellectual hegemony, so that it becomes the enlightened common-sense of our age' (RR, p. 1). And he concludes the book with the following lapidary and somewhat laconic judgment:

[T]here is something about the market and what Marx called the value and wage forms which makes empirical realism the account of reality or ontology that is spontaneously generated therein. Within the capitalist mode of production critical realism is always going to seem a luxury its agents cannot afford. It is the argument of this book that it is a philosophy without which a socialist emancipation cannot be achieved. (RR, p. 192)

Both statements are, I think, true; the question is how the first can be brought about given the necessary recalcitrance of existing agents hypothesized in the second. My own inclination is to take the line of the later Althusser, however much this has been maligned. The crucial text, not surprisingly, is the essay on 'Ideology and Ideological State Apparatuses'.

Too much ink has been spilt over this little essay (the subtitle of which, 'Notes towards an Investigation', has seldom been heeded) to require a full-dress exposition here. It will suffice instead to observe its pertinence to socialist transformation, which both Bhaskar and Collier envisage as the ultimate aim of their work. Barring some unforeseeable advance in self-organization among the working classes of the advanced capitalist world, or some equally unpredictable collapse of the global capitalist economy (both are of course possible, the latter even likely in the long term), there are no immediate prospects for socialist transformation in Europe, the United States or

Japan.²¹ On a Marxist-realist account of society, the persistence (and general acceptance) of most aspects of bourgeois ideology among these populations seems inevitable. To hope to make socialism 'the enlightened common-sense of our age' under such conditions must appear utopian in the worst sense.

Among the enduring merits of Althusser's essay on ideology is its insistence on the materiality of ideological practice. This is, of course, already entailed by the classical Marxist concept of ideology, but, with the possible exception of Gramsci, no one in the Marxist tradition before Althusser had situated the theory of ideology so centrally on the terrain of social life. While the notion of 'ideological state apparatuses' has been much criticized for its denegation of civil society, the hypothesis that one of the principal functions of the capitalist state is to ensure the reproduction of capitalist social relations is clearly defensible. One way in which it does so is by training a technical intelligentsia, the 'savants' to whom Althusser addressed his lectures on philosophy and science and whose decisive role in the reproduction of capitalism no one can seriously doubt.

Now, it is a characteristic Blanquist error to believe that elites can make revolutions without a mass base; but the opposite populist (and anti-Leninist) one is to pin all hope on the native capacity of the working class to make and sustain one. If socialist emancipation one day proves to be on the agenda for any significant proportion of humankind, it will not be because workers in the capitalist heartlands have overnight been delivered from their ideological illusions by the sheer force of reality striking them in the face. Socialism will emerge from capitalism, if it ever does, only as part of a long historical process of re-education among elites and masses alike, maturing in the womb of capitalist society and a fortiori in those institutions where the ideological domination of the bourgeoisie is most immediately secured. A Marxist philosophy *for* science in our epoch is therefore necessarily a struggle for hegemony within the institutions of (natural- and social-) scientific training. Realist theory entails, as Bhaskar insists, ontological commitments and epistemological principles; its triumph over idealism further requires the sort of ideological struggle theorized by Althusser as well as practised by him in *Spontaneous Philosophy*.

Science as Power

The hegemony of the technical intelligentsia in modern societies is the overarching theme of Aronowitz's *Science as Power*. On his view, the position of religion (ideological common sense under feudalism) is now occupied by science (ideological common sense under advanced capitalism [SP, pp. 8–10]). The power of science is part and parcel of capitalist social relations, which require constant revolutionizing of the means of production. The increasing domination of technology over labour, and the broad acceptance of scientific method, go hand

²¹ And surely one consequence of the epochal changes in Eastern Europe and the Soviet Union over the past two years is to have rendered similar prospects in the capitalist periphery incomparably more difficult.

in hand to reproduce the dominance of the bourgeoisie in the economy and in ideology: 'Machine technology cannot be separated from the social relations that created it. The logic of domination remains embedded in the machine, which is an instrument for the perpetuation of social oppression and exploitation by virtue of not only its uses but its construction as well' (SP, p. 78).

Based on a (partial but not wholly unwarrantable) reading of Marx on the labour process (SP, pp. 45–55), Aronowitz's account of science reverses the conventional view in philosophy of science, subordinating scientific method to its instrumentalization in experiment:

Scientific discovery depends increasingly on the sophistication of the machines of experimental science. For, to the extent that science believes it relies on observation as much as mathematical calculation, its collective experience is mediated by the accuracy of the data collected by means of mechanical interventions into the nature it constructs in the laboratory. (SP, p. 41)

In Bhaskar's terms, science is constituted wholly by its activities in the transitive dimension (which in one sense is true: without experimentation and hypothesis-formation there would be no science), setting aside the possibility that what science investigates and explains pre-exists experimental intervention and persists in the same way once the experiment is over. For Aronowitz, there just are no such things as intransitive objects of knowledge.

Aronowitz's case against science is a Marxist variant of standard conventionalist (Kuhn) or constructivist (Latour and Woolgar) accounts. It is marred, among other ways, by its conflation of scientific realism with positivism. Commenting on what he terms 'positivist tendencies in Marx', Aronowitz characterizes these latter as 'the notion of the objective material world as prior to human will, possessing an independent power which can be discovered through scientific investigation, specifically through experiment' (SP, p. 74). Nothing in this characterization is specifically positivist, at least as this description has traditionally been understood. Moreover, does Aronowitz honestly believe (and expect us to believe) that there were no subatomic particles in the universe before their existence was registered in certain experiments at the end of the nineteenth century, or that such particles cease to exist after they have passed through a cloud chamber and their 'tracks' are recorded? Similarly, does he hold that prior to the discovery of DNA genetic duplication and mutation were accomplished by some other means, or that outside the laboratory species are not perpetuated by the biochemical properties determined by the double-helix structure? If we are to doubt the findings of the empirical sciences, we need to be given better reasons than that they have arisen from and been a necessary adjunct to capitalist social relations. Even if this latter is the case (and I doubt that it is, at least in the global, undifferentiated manner Aronowitz asserts), scientific discoveries could still give correct descriptions of (certain features in) nature. That the nuclear-power industry has attempted to impose on us a grossly undemocratic and capital-intensive form of energy production

is no grounds for thinking that controlled fission reactions are just a capitalist plot.

The true test of realist versus conventionalist accounts of science comes, it should by now be clear, in the social sciences, where the existence of intransitive objects of knowledge is less obvious. Given Aronowitz's insistence on the constitutive role of ideology in scientific investigation, it is worth considering how he himself conceives this key concept in Marxist theory. He is, predictably, openly scornful of orthodox accounts that link ideology to class struggle, chiding them for remaining 'grounded in an objectivist account of history' (SP, p. 110). Yet his own rendition of the history of modern science makes just such an 'objectivist' claim about the determination of scientific method by social relations of production. Aronowitz seems to have a distinctive conception of ideology (and thus of science) in mind, one that is at once grounded in social relations and yet not unilaterally determined by one's position in the class struggle. He is, rightly, critical of the preposterous Stalinist notion of 'proletarian science' (SP, pp. 111–16), while trying to 'save the appearances' of Marx's insight into the determination of consciousness by social being (see, for example, his discussion of the Lysenko affair, SP, pp. 227–9).²² Is it possible, then, to give a more or less objective account of the 'social context' in which physical laws are 'produced'? Aronowitz seems to think so, and cites many studies that aim to do just that: Fleck on the discovery of syphilis (SP, pp. 187–8); Latour on Pasteur (p. 293); Pickering on quarks (pp. 291–2). But what is the 'object' in these studies? By what criteria might they be said to pass muster as truthful accounts of how things are in the social world of science?

Aronowitz's recommendations for 'critical science' (SP, ch. 11) can only be realized with a concept of scientific inquiry that he rejects in principle (while embracing it in practice): to wit, that social relations are relatively enduring structures which it is possible to observe and describe in terms not merely subjective or bound to the local interests of a party, a guild, or even a class. Take Aronowitz's favourite bugbear: the division between manual and mental labour upon which the hegemony of modern science depends. Drawing upon Sohn-Rethal, Aronowitz locates the origins of this paradigm for knowledge production historically in Greek philosophy (and, presumably, in its motivating discourse, mathematics) and socially in commodity exchange (SP, pp. 142–3). Its true historical destiny remains unfulfilled, however, until the capitalist epoch, when workers (manual labourers) are for the first time completely alienated from the means

²² Aronowitz's own position seems to be more or less that of Christopher Caudwell's *The Crisis in Physics*, held by Aronowitz to be 'the first Marxist work to posit both the relative autonomy of scientific knowledge from its social relations, and the determination, in the last instance, of physical laws by the social context of their production' (SP, p. 120). *Posit* is the operative word here. Nothing Aronowitz can say will ever redeem Caudwell's ill-digested *mélange* of anecdotal information about the history of physics from the utter intellectual oblivion to which it has justly been consigned for over half a century. Aronowitz scarcely does any better, it should be said; see SP, ch. 10, 'The Breakup of Certainty', for his highly dubious account of analytical philosophy of science in relation to post-Einsteinian mechanics.

of production through the institution of the wage form; the culmination of this logic is the increasing technical subordination of labour to capital, and the degradation of the labour process (SP, pp. 48–59). Marx, from whom the elements (if not the conclusions) of this account are taken, thought when he was describing the capitalist mode of production that: (1) he was giving an account of social reality; (2) his account was generally perspicuous—that is, it was scientific (rather than ideological, as were those given in political economy). Having been born a bourgeois, educated in bourgeois institutions, and accepting the eminently bourgeois belief in the value of scientific inquiry seems not to have been an insuperable obstacle to his producing an account of bourgeois social relations that remains to this day without serious theoretical rival. *Soi-distant* Marxist philosophies of science that assert the total subordination of scientific practice to ideological determination need, among other things, to explain this rather striking anomaly in their theory.

Feyerabend's Challenge

Surely the most consequent—if increasingly quixotic—challenge to the realist philosophy of science has been mounted by Paul Feyerabend. The recent reissue of his classic *Against Method*,²³ as well as the publication of what he has said (perhaps in jest) will be his last book, *Farewell to Reason*,²⁴ provides an occasion for assessing the strengths and weaknesses of the 'anarchist' or 'dadaist' philosophy of science he champions. I shall begin somewhat obliquely by drawing upon Roy Bhaskar's pointed critique of 1975, 'Feyerabend and Bachelard: Two Philosophies of Science', reprinted in *Reclaiming Reality*.

Among the many difficulties of giving a coherent account of Feyerabend's work since 1970, as Bhaskar notices (RR, p. 40) and Feyerabend himself admits in his Preface to the new edition of *Against Method* (AM, p. vii), is that one never knows whether to take anything he says seriously. His posture is attractive for anyone who feels that scientists and their enterprise can usefully be taken down a peg or two. The public image of science and its practitioners (in this I basically agree with Aronowitz) is one of unquestioned cognitive superiority, so that Feyerabend's debunking of scientific hubris is necessary and timely. There is nothing sacrosanct about science in general or about any of its currently held theories in particular; scientific practice is always open to criticism—but, it should be added, not just in any form.

Nor does Feyerabend seem to feel, despite his asseveration that in the

²³ London 1988; hereafter cited parenthetically in the text as AM; the original edition appeared in 1975.

²⁴ London 1987; hereafter cited parenthetically in the text as FR. *Against Method* and *Farewell to Reason* are provocative and, above all, entertaining books, rich in historical detail and witty in presentation. Since Bhaskar's critique of the former (see below) pretty much makes my case against its faults (of which Feyerabend is predictably unrepentant in *Farewell to Reason*), I have concentrated instead on the somewhat tamer essays collected in P.K. Feyerabend, *Philosophical Papers*, 2 vols. Cambridge 1981, hereafter cited parenthetically as PP1 and PP2.

sciences ‘anything goes’ (AM, p. 21), that criticism of scientific research is just an open field where any counterposition to an existing hypothesis is warranted or licit. His conception of science and of its place in a free society is more nuanced, hence more sustainable:

Again I want to make two points: first, that science can stand on its own feet and does not need any help from rationalists, secular humanists, Marxists and similar religious movements; and, secondly, that non-scientific cultures, procedures and assumptions can also stand on their own feet and should be allowed to do so, if this is the wish of their representatives. Science must be protected from ideologies; and societies, especially democratic societies, must be protected from science. (AM, p. viii)

If it has been Feyerabend’s special crusade to aid in this second sort of protection, the task (if not always the practice) of philosophy of science is to help out in the first. And it may be added, *pace* Feyerabend, that the security of the sciences does not always go without saying, any more than does the existence of the democratic institutions he rightly insists are the necessary corollary of a humanly liberating practice of science.

Feyerabend’s career, as Bhaskar indicates (RR, p. 33), presents a curious trajectory. Friend and admirer of Imre Lakatos, once an ultra-orthodox Popperian, he has become the great scourge of the very principles he himself was intent on defending in his early work.²⁵ On Bhaskar’s construal of it, Feyerabend’s position in *Against Method* can be organized around three central claims; the first two derive from a critical evaluation of standard views in philosophy of science.

(1) In the company of Kuhn, whom he had criticized in an earlier incarnation²⁶ but now considers a comrade-in-arms (AM, pp. 229–30), Feyerabend insists on incommensurability between competing explanations of phenomena in science, and competing worldviews out of it (RR, p. 32).

(2) A corollary of this first claim is that the history of science itself reveals essential anarchism in its development, and that this is a necessary condition for its progress—although Feyerabend is predictably sceptical concerning the generality of notions like scientific progress (RR, pp. 33–4).

(3) Underlying Feyerabend’s critical appraisal of science and its justificatory philosophy is a deep commitment to human freedom, which he sees endangered by scientific enterprise. As Bhaskar summarily observes, Feyerabend is ‘*for freedom and against science*’ (RR, p. 35).

Bhaskar’s replies to (1) (RR, pp. 32–3; see also RTS, pp. 191, 248, 258) and (2) (RR, pp. 34–5) are unimpeachable and need not detain us. As he recognizes, the real force of *Against Method* derives from (3), which

²⁵ For the ‘Popperian’ Feyerabend, see, *inter alia*, ch. 8 (sections 1–8) in PP2, and especially his ‘Problems of Empiricism’, in R.G. Colodny, ed., *Beyond the Edge of Certainty*, Englewood Cliffs, NJ 1965.

²⁶ See ‘Consolations for the Specialist’, (PP2, pp. 131–61); originally published in Imre Lakatos and Alan Musgrave, eds., *Criticism and the Growth of Knowledge*, Cambridge 1970, pp. 197–230.

involves similar problems to those posed by Rorty's championing of liberal freedom (see above, pp. 127–8) and encounters a similar objection:

Knowledge may not be the most important social activity, but it is the one upon which the achievement of any human objective depends. Freedom, in the sense Feyerabend attaches to it, depends upon knowledge (praxis presupposes theory); we can only be as free as knowledge is reliable and complete. We are not free to choose what we believe if we are to attain the kinds of objectives Feyerabend mentions. Only if belief-in-itself was the sole end of human action would Feyerabend be warranted in such an assumption. (RR, p. 36)

To my knowledge, Feyerabend has never formally replied to Bhaskar's criticisms. How might he have done so?

Against Method opens with an interesting contention: 'The following essay is written in the conviction that *anarchism*, while perhaps not the most attractive *political* philosophy, is certainly excellent medicine for *epistemology*, and for the *philosophy of science*' (AM, p. 9). Feyerabend thus immediately prizes apart the social domain from science. (Here and throughout his work 'science' almost exclusively denotes the natural sciences.) There is a constitutive tension, then, between Feyerabend's conception of the social dimension of scientific inquiry and his apparent conviction that consequent political action cannot follow an anarchist line. He immediately quotes Lenin in support of his own view that no methodological imperative is entailed by the practice of science, save that method is generally—and productively—ad hoc, the scientist, in Einstein's words, 'an unscrupulous opportunist' (AM, p. 11).

The invocation of and commentary upon Lenin at this point are curious. Feyerabend takes him to be saying that politics and history are just a messy business from which no principles or rules can be derived, and that what obtains in history and politics holds for inquiries about nature. Feyerabend then quotes Mach on scientific practice to the same effect, arguing that politics (on Lenin's account) and science are methodologically similar (AM, pp. 10 n. 5). I offer two brief comments. First, as even the most casual reader of Lenin knows (and as Bhaskar points out; see RR, p. 36), nothing could be further from the concept of politics and history adduced here than Lenin's understanding of strategic calculation. For Lenin, a consequent politics can only be constructed on the basis of a particular science (historical materialism), which gives the revolutionary tactician the requisite knowledge for choosing between alternative courses of action. Second, Feyerabend's invocation of Mach on investigative procedure, however salutary the latter may be as a rule of thumb, makes for an odd bedfellow with the author of *Materialism and Empirio-Criticism*. Feyerabend was certainly acquainted with Lenin's text before penning this note,²⁷ yet he ignores the fact that Lenin wrote it

²⁷ See 'Two Models of Epistemic Change: Mill and Hegel', PP2, p. 72 n. 11. This paper was originally published in 1970 as part of Feyerabend's programmatic essay 'Against Method'.

precisely to combat Mach's increasing prestige among certain Bolshevik intellectuals (Bogdanov, Lunacharsky, Bazarov).²⁸ The omission is far from incidental; it is of a piece with Feyerabend's (mis)understanding of Marxism as a science of the history of social formations and as a political practice, and is part and parcel of his libertarian idea of freedom.

Marxism, Science and Freedom

Feyerabend has been casually invoking Marxist thought for many years, characteristically as a buttress for his own anti-rationalist epistemology. But on some few occasions a different strain has taken over. For example, in a critical treatment of the Lakatosian idea of scientific research programmes, he writes in an almost partisan vein:

And whoever has read Rosa Luxemburg's reply to Bernstein's criticism of Marx or Trotsky's account of why the Russian revolution took place in a backward country... will see that Marxists are pretty close to what Lakatos would like any upstanding rationalist to do, though there is absolutely no need for them to accept his rules. (PP2, p. 207)

In another place he quotes extensively from Mao in defence of J.S. Mill (PP2, pp. 67–8), while drawing solace from Lenin for his own reading of Hegel (PP2, pp. 74, 75, 79). Unlikely as the comparison appears at first, *this* Feyerabend sounds like no one so much as the later Althusser of *Lenin and Philosophy*. Well, not quite.

Feyerabend's diagram and explanation of the relations among the three that guide his view of knowledge production seem at first blush attractive and not wholly dissimilar to Althusser's account of Generalities I, II and III. The diagram proposes the following schema for scientific inquiry:

criticism → proliferation → realism

on which Feyerabend comments:

Nor does the arrow... express a well-defined connection such as logical implication. It rather suggests that starting with the left hand side and adding physical principles, psychological assumptions, plausible cosmological conjectures, absurd guesses and plain commonsense views, a dialectical debate will eventually arrive at the right hand side. (PP1, pp. vii–viii)

Further, he is quite adamant that the arrow stands for no general methodological protocols; its meaning lies only in the particular examples adduced. (Feyerabend gives several instances of how research has actually proceeded, but presumably others would illustrate the crucial point as well: that no method of science can be derived from examining what scientists in fact do.)

Feyerabend's point about proliferation is that the business of scientific research is just messy; his claim for realism is roughly the standard

²⁸ See Althusser, 'Lenin and Philosophy', PSPS, pp. 167–202.

one that theories do indeed refer to things.²⁹ No Marxist should be hostile to these views. Nor is his understanding of criticism likely to meet with disapproval: 'Criticism means that we do not simply accept the phenomena, processes, institutions that surround us but we examine them and try to change them' (p. vii). To Bhaskar's characterization of his work (see above, p. 141), Feyerabend might fairly reply that he is *against* (only rationalist accounts of) science just because he is *for* a practice of science that may better serve the end of human freedom. So where is the argument?

It comes, unsurprisingly, when one attempts to cash out Feyerabend's democratic (indeed populist; see, for example, FR, pp. 273–9) attachments in an emancipatory programme that possesses substantive content and respects the quite real limits of both humanity and nature. Feyerabend can invoke the manifest ecological and human disasters visited on various people by Western science (FR, pp. 3–4, 26–7), but all he can recommend to prevent their worsening (or their recurrence in different forms) is that everyone respect everyone else's right to be different. Such a notion of freedom is demonstrably vacuous; it gains no force by being dressed out in pseudo-materialist garb with citations from Lenin and Marx. The latter two held rather stronger notions about a science of society and recognized that any project for the liberation of humankind needed to understand (that is, explain) the material—social and natural—constraints on human freedom prior to changing the conditions that currently prevent its full realization.

Feyerabend is rightly critical of the collective arrogance of the technical intelligentsia (and the West generally),³⁰ but this gives no grounds for a general indictment of the scientific enterprise. It is, rather, a demonstration of Bhaskar's and Althusser's point about the persistence of ideologies in science's transitive dimension. In order to launch a consequent critique of these ideologies, more than a Mill-inspired liberal tolerance is required. It demands, simply enough, a theory of ideology as necessary illusion. Historical materialism offers, among other things, such a theory. Marxist philosophy cannot settle matters for or against this theory; it can, however, struggle against the various regnant idealisms that seek to deny it any chance of making its own way in the world. A Marxist philosophy *of* science is therefore a philosophy *for* science in just this sense. Hence, it is now possible (and necessary) to emend Marx's famous pronouncement to say: Philosophers neither interpret nor change the world; science, whose cognitive autonomy philosophy is charged with protecting, does the former in order that the latter may one day be possible.

²⁹ Feyerabend's respect for Aristotle is considerable; see PPI *passim*; PP2, pp. 12–15, 183–4, and *passim*; and especially his *Science in a Free Society*, London 1978, pp. 53–65.

³⁰ In this respect, among Feyerabend's most attractive proposals is that scientific training in the West is unconscionably ethnocentric and should be modified to include alternative cosmological traditions (see, for example, AM, pp. 11–12; FR, pp. 20–39).