Nicholas Maxwell, Philosophy Department, University College London

What's Wrong with HPS and What Needs be Done to Put it Right?

After a sketch of the optimism and high aspirations of HPS when I first joined the field in the mid-1960s, I go on in this essay to describe the disastrous impact of "the strong programme" and social constructivism in history and sociology of science. Despite Alan Sokal's brilliant spoof article, and the "science wars" that flared up partly as a result, the whole field of HPS and STS is still adversely affected by social constructivist ideas. I then spell out how in my view Philosophy of Science ought to develop.

It is, to begin with, vitally important to recognize the profoundly problematic character of the aims of science. There are substantial, influential and highly problematic metaphysical, value and political assumptions built into these aims. Once this is appreciated, it becomes clear that we need a new kind of science which subjects problematic aims - problematic assumptions inherent in these aims - to sustained imaginative and critical scrutiny as an integral part of science itself. This needs to be done in an attempt to improve the aims and methods of science as science proceeds. The upshot is that science, philosophy of science, and the relationship between the two, are all transformed. HPS becomes an integral part of science itself. And becomes a part of an urgently needed campaign to transform universities so that they become devoted to helping humanity create a wiser world.

1 High Aspirations of HPS in the 1960s

I discovered the work of Karl Popper in the early 1960s, partly as a result of attending his seminars at the LSE, and I was immensely impressed. Here was a philosopher passionately concerned with profound, real problems of the real world which he tackled with fierce intellectual integrity and great originality. There was first his transformation of science - or at least of our conception of science. Laws and theories cannot be verified in science, but they can be empirically falsified, and that is how science makes progress. As a result of subjecting theories to fierce sustained attempted empirical refutation, we eventually discover where they go wrong, and are thus provoked into thinking up theories which do even better, until they are in turn refuted. Scientific knowledge is simply made up of our best, boldest imaginative guesses that have survived all our most ruthless attempts at empirical refutation.[1]

Then there was his generalization of this falsificationist conception of science to form a radically new conception of rationality. To be rational is to be critical. Just as science makes progress through subjecting our best conjectures to fierce attempted falsification, so more generally, in all areas of human life, we can best hope to make progress by subjecting our best attempts at solving our problems to fierce criticism. Empirical testing in science is just an especially severe form of criticism.[2]

The entire tradition of western philosophy had got it wrong. Scepticism is not the enemy to be vanquished - or to be indulged until it can go no further, thus revealing a bedrock of certainty, as with Descartes, and many empiricists. Quite the contrary, scepticism is our friend, the very soul of reason. It is by means of imagination subjected to sustained, ferocious scepticism that we can learn, and make progress. Science is institutionalized scepticism.

What impressed me most, however, was the application of these ideas to the profound problem of creating civilization or, as Popper called it, "the open society". Rationality is

the critical attitude. But this is only really possible in an "open" society, a society, that is, which tolerates a diversity of views, values and ways of life. In a "closed" society, in which there is just one view of things, one set of values, one way of life, there can be no possibility of criticism, since to criticize A we need, at least as a possibility, some alternative view B. Thus the rational society is the open society - not a society enslaved to some monolithic, dictatorial notion of "reason", but simply a liberal society that tolerates and sustains diversity of views, values and ways of life, and can, as a result, learn, make progress, and even create and pursue science.[3]

But the move from the closed to the open society has a severe penalty associated with it. We move from certainty to doubt. Living in the open society requires that we shoulder the adult responsibility of living in a state of uncertainty, of doubt. Everything we believe, everything we hold most dear, and value - the very meaning and value of our whole way of life - may be wrong or misconceived. Doubt is the price we pay for civilization, for reason, for humanity, and for science. In his masterpiece *The Open Society and Its Enemies* (1945), Popper calls this essential doubt "the strain of civilization", and he points out that all too many people cannot bear it, and seek to return to the false certainties of the closed society. Even some of our greatest thinkers have sought to do this, and they are the enemies of the open society - above all, for Popper, Plato and Marx.[4]

Popper demonstrated, it seemed to me, that it was possible to be an academic philosopher and yet retain one's intellectual integrity.[5] I moved down to London from Manchester (where I had studied philosophy) and got a job as lecturer in philosophy of science in the Department of History of Philosophy of Science at University College London. Larry Laudan and Paul Feyerabend were among my departmental colleagues.

It was an exciting time and place to be doing history and philosophy of science (HPS). London felt like the HPS capital of the world. HPS seemed to be a fledgling academic discipline, having associated with it all the excitement, freshness, high aspirations and optimism of a new discipline. There was the idea that each wing needed the other: history of science would be blind without philosophy of science, which in turn would be empty without history of science. Natural science seemed to be the one great human endeavour that undeniably made progress across generations and centuries. Aside from mathematics, in no other sphere of human endeavour did this happen - not in art, music, literature, politics, or morality. There was technological progress, certainly, and economic progress too, but these were closely linked to, and dependent on, scientific progress. It was the great task of HPS to work out how science did make progress, and what might be learned from scientific progress about how to make progress in other areas of human life: art, literature, law, education, politics, economics, international relations, personal flourishing and fulfilment. Popper had shown the way. But he could hardly be the last word on the subject. Popper's philosophy needed to be applied to itself, and subjected to sustained critical scrutiny in an attempt to improve on it. And there were plenty of contending ideas around, most notably, those of Thomas Kuhn, Imre Lakatos and Paul Feyerabend.

2 Decline of HPS

But then HPS fell into a sad decline, and lost its way. Feyerabend argued for methodological anarchy, for the view that, in science, "anything goes".[6] Barry Barnes and David Bloor. [7] launched "the strong programme": science must be understood in purely sociological terms, there being no such thing as scientific truth, fact, reason, method or progress.[8] Something similar came from postmodernism, French

philosophy, Foucault, Derrida and others. The upshot was a whole new way of construing science, which may be called "social constructivism". Scientific knowledge is merely a social construct, having nothing to do with knowledge, truth and falsity, or reason. Sociologists and historians of science took to social constructivism, while philosophers of science looked on in amazement and horror, at the idiocy of it. As a result, HPS broke asunder. The integrated enterprise, bringing together history and philosophy of science, which had started out with such high hopes and aspirations, and which was still alive and kicking when I began my academic career around 1965, was no more. The fundamental problem of History of Science - How has scientific progress come about? - could not even be asked.

Then, as if matters were not bad enough already, Philosophy of Science began to degenerate into a kind of scholasticism that splintered into a multitude of specialized disciplines: philosophies of the specialized sciences - physics, chemistry, neuroscience, astronomy, botany, and so on. As a result, Philosophy of Science lost sight of the magnificent endeavour of natural science as a whole, and came to ignore the great, fundamental problems that were, initially, the whole raison d'être for its existence: the problem of induction, the problem of the rationality of science, the problem of how, by what means, science makes progress.

In 1996 the worst excesses of the social constructivists and anti-rationalists were brilliantly satirized by a spoof article by Alan Sokal.[9] The "science wars" exploded onto the scene, some scientists and philosophers of science springing to the defence of science against the corrosive acid of social constructivism, anti-rationalism and postmodernism. Paul Gross and Norman Levitt wrote a book assailing the worst excesses of postmodernist writing about science, and subsequently edited a book that continued the argument.[10] Alan Sokal and Jean Bricmont outraged French intellectuals with devastating criticisms of French philosophers' writings about science: Jacques Lacan, Luce Irigaray, Bruno Latour, Gilles Deleuze and others.[11] Noretta Koertge edited a book *Exposing Postmodernist Myths About Science*.[12] Others joined the affray. Social constructivists protested that distinctions were being ignored, contexts overlooked.

3 What Was Overlooked

Both parties to this dispute profoundly missed the point. The social constructivists were right to hold that orthodoxy could not make rational sense of science, but disastrously wrong to interpret science in purely sociological terms. Those who defended orthodoxy, the view that science does make progress and acquire knowledge, were right to criticize and reject social constructivism, but wrong to defend current orthodox views about science. Gross, Levitt, Sokal and company sprang to the defence, not of scientific rationality, but to a very seriously irrational conception of science masquerading as rationality. Everyone ignored the crucial questions: What are the real aims of science? Granted that they are profoundly problematic, how can they be improved?

The irrational view of science I have in mind, taken for granted by most scientists and philosophers of science, may called *standard empiricism* (SE). This holds that the basic intellectual aim of science is factual truth (nothing being presupposed about the truth), the basic method being to assess claims to knowledge impartially with respect to evidence. Considerations such as the simplicity, unity or explanatory character of a theory may influence what theory is accepted, but not in such a way that the universe or the phenomena are permanently assumed to be simple, unified or comprehensible. According to SE, what theory is accepted may even be influenced for a

time in science by some paradigm or metaphysical "hard core" in the kind of way depicted by Kuhn and Lakatos[13] as long as, in the end, empirical success and failure are the decisive factors in determining what theories are accepted and rejected. The decisive tenet of SE is that *no substantial thesis about the nature of the universe can be accepted as a permanent part of scientific knowledge independently of empirical considerations* (let alone in violation of empirical considerations).

Even those who - like Feyerabend, social constructivists and postmodernists - reject the whole idea that science is rational, delivers authentic knowledge, and makes progress, nevertheless tend, in a way, to uphold some version of SE as the only possible rationalist conception of science. No rational account of science is possible, they hold in effect, because the only candidate, SE, is untenable (as shown by the failure of SE to solve the problem of induction).

Despite being almost universally taken for granted by scientists, SE is nevertheless untenable. SE very seriously misrepresents the aims of science. The intellectual aim of science is not to improve knowledge of factual truth, nothing being presupposed about the truth. On the contrary, science cannot proceed without making a very substantial and highly problematic *metaphysical* hypothesis about the nature of the universe: it is such that some kind of unified pattern of physical law governs all natural phenomena. Science seeks, not truth per se, but rather *explanatory* truth - truth presupposed to be explanatory. More generally, science seeks *valuable* truth - truth that is of intrinsic interest in some way or useful. This aim is, if anything, even more problematic. And science seeks knowledge of valuable truth so that it can used in social life, ideally so as to enhance the quality of human life. There are, in other words, problematic *humanitarian* or *political*assumptions inherent in the aims of science. In holding that the basic intellectual aim of science is *truth per se*, the orthodox position of SE misrepresents the real and highly problematic aims of science.

The vital task that needs to be done to develop HPS in fruitful directions - a task not performed because of the influential absurdities of "the strong programme", social constructivism and the science wars debate - is to give absolute priority to two fundamental questions: What are the real aims of science? What ought they to be? Ever since around 1970, when I began to consider these questions, those associated with HPS and STS ought to have put these two questions at the heart of science studies. If this had been done, science studies, in conjunction with sympathetic scientists, science journalists and others, might have helped develop a conception of science, and even a kind of science, both more rigorous and of greater human value than what we have today. Indeed, a new kind of academic inquiry might have emerged that is rationally devoted to helping humanity make social progress towards as good a world as possible. We might even have begun to see the beginning of a new kind of social world capable of tackling its immense global problems in increasingly effective and cooperatively rational ways. None of this has come about because the academic disciplines most directly responsible for helping to initiate these developments, HPS and STS, have been distracted by intellectual stupidities.

4 Why Standard Empiricism (SE) is Untenable

The key step that needs to be taken to permit these urgently needed intellectual, institutional and humanitarian developments to unfold is the widespread recognition that standard empiricism (SE) is indeed untenable, and needs to be replaced by something better. So, let us see why SE is untenable.

As it happens, reasons for rejecting SE have been spelled out in the literature again and again, ever since 1974.[14] But these refutations of SE have been ignored.

In outline, the refutation goes like this. Theoretical physics persistently only ever accepts *unified* theories - theories that attribute the same dynamical laws to the phenomena to which the theory applies. Given any such accepted theory - Newtonian theory, classical electrodynamics, quantum theory, general relativity, quantum electrodynamics, or the standard model - endlessly many disunified rivals can be easily concocted to fit the available phenomena even better that the accepted unified theory.[15] These disunified rivals that postulate different laws for different phenomena in a "patchwork quilt" fashion, are (quite properly) never taken seriously for a moment despite being empirically more successful. This persistent acceptance of unified theories in physics even though endlessly many empirically more successful, patchwork quilt rivals can readily be formulated means that physics makes a persistent assumption about the universe: it is such that all seriously disunified theories are false. The universe is such that some kind of underlying unified pattern of physical law runs through all phenomena.

If physicists only ever accepted theories that postulate atoms even though empirically more successful rival theories are available that postulate other entities such as fields, it would surely be quite clear: physicists implicitly assume that the universe is such that all theories that postulate entities other than atoms are false. Just the same holds in connection with unified theories. That physicists only ever accept unified theories even though endlessly many empirically more successful, disunified rival theories are available means that physics implicitly assumes that the universe is such that all such disunified theories are false.

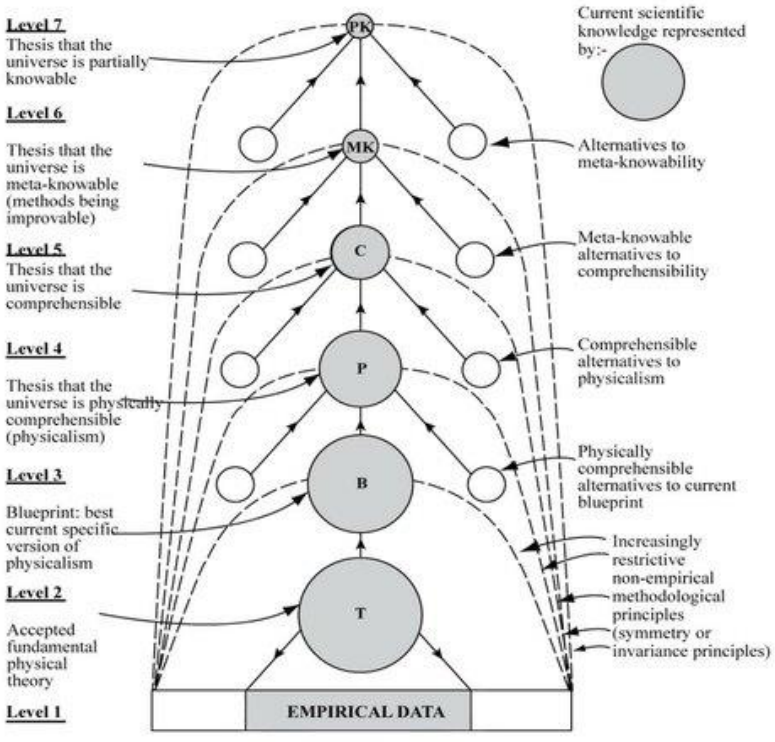

Figure 1: Aim-Oriented Empiricism (AOE)

In accepting the unified theories that it does accept - Newtonian theory, classical electrodynamics and the rest - physics thereby adopts a big, highly problematic metaphysical hypothesis, H, about the nature of the universe: it is such that all rival, grossly disunified, "patchwork quilt" but empirically more successful theories are false.

H, though a metaphysical hypothesis, is nevertheless a permanent, even if generally unacknowledged, item of theoretical knowledge. Theories that clash with it, even though empirically more successful than accepted physical theories, are rejected - or rather, are not even considered for acceptance. Whenever a fundamental physical theory is

accepted, endlessly many empirically more successful rivals, easily formulated, are not even considered just because, in effect, they clash with H. Thus, H is a permanent item of theoretical knowledge in physics, more securely established in scientific practice indeed than any physical theory. Physical theories tend eventually to be shown to be false, but H persists through theoretical revolutions in physics.[16]

Nevertheless, H is a hypothesis, a pure conjecture. How can we make sense of the idea that science is rational and delivers authentic knowledge if the whole enterprise depends crucially on accepting such an unsupported hypothesis as a secure item of scientific knowledge - a hypothesis that exercises a major influence over what theories are accepted and rejected in physics?

5 Aim-Oriented Empiricism (AOE)

In order to answer this question, we need to adopt a conception of science that I have called *aim-oriented empiricism* (AOE). Precisely because H is a substantial assertion about the nature of the universe, an assertion that, though purely conjectural in character, nevertheless exercises a major influence over what theories are accepted and rejected, even to the extent of over-riding empirical considerations, it needs to be made explicit within physics so that it can be critically assessed, rival hypotheses if possible being developed and assessed, in the hope that H can be improved on. We need a new conception of science which represents the metaphysical hypotheses of physics in the form of a hierarchy of hypotheses, as one goes up the hierarchy hypotheses becoming less and less substantial, and more nearly such that their truth is required for science, or the pursuit of knowledge, to be possible at all. In this way, we create a relatively unproblematic framework of hypotheses, and associated methodological rules, high up in the hierarchy, within which much more substantial and problematic hypotheses, and associated methodological rules, low down in the hierarchy, can be critically assessed and, we may hope, improved, in the light of the empirical success they lead to, and other considerations: see figure 1.

All this can be reformulated in terms of aims and methods. The aim of science is not truth per se, as SE holds. It is rather truth presupposed to be explanatory - or at least knowable. Precisely because this aim of science presupposes a problematic metaphysical hypothesis, the aim (or the hypothesis presupposed by the aim) needs to be represented in the form of a hierarchy of aims (or hypotheses) as indicated in figure 1, so that attempts to improve aims (or hypotheses) may receive the best possible help. As our scientific knowledge and understanding improve, so aims and methods improve as well. There is something like positive feedback between improving scientific knowledge and improving aims and methods - improving knowledge about how to improve knowledge. Science adapts itself to what it finds out about the universe.

It is this positive feedback, this interaction between improving scientific knowledge on the one hand, and improving aims and methods (improving assumptions and methods) on the other, that helps explain the explosive growth of modern science. For all this has gone on in scientific practice despite scientists paying lip service to SE. Allegiance to SE has been sufficiently hypocritical to permit aim-oriented empiricism (AOE) to be put into scientific practice, to some extent at least. Allegiance to SE has nevertheless obstructed full implementation of AOE, and has had damaging consequences for science as a result.[17]

There are now three key points to note about AOE.

1. It is not just theoretical physics that has a problematic aim because of problematic hypotheses inherent in the aim. This is true of most - perhaps all - scientific disciplines. Thus most, or perhaps all, scientific disciplines need to be understood in terms of diverse versions of the hierarchical, meta-methodological structure of AOE depicted in figure 1. The aims and methods of science change as we move from one science to another, and as we move within any given science from one time to another. The common factors are (a) something like the hierarchical, interacting structure depicted in figure 1; (b) the common endeavour to improve knowledge and understanding of the universe, and ourselves and other living things as a part of it. AOE provides a general solution to the problem of the nature of the progress-achieving methods of science.[18]
2. AOE solves fundamental problems in the philosophy of science: in particular, the problem of induction (the problem of the rationality of science); the problem of verisimilitude; and the problem of what it means to say of a physical theory that it is unified.[19]
3. AOE transforms the nature of science, the nature of philosophy of science, and the nature of the relationship between the two. And all this impacts on the nature of HPS and STS. Traditionally, philosophy of science has been conceived of, and practised, as a meta discipline, studying science in the same way as astronomers study the moon or distant galaxies. This might make sense if science had a fixed aim and fixed methods, as SE holds science does. But AOE asserts that, because the basic aims of science are profoundly problematic, they evolve as scientific knowledge evolves, and change from one science to another. AOE demands that there is a two-way interaction between science itself, on the one hand, and its aims-and-methods, or philosophy, on the other hand. Metaphysics and the philosophy of science become vital ingredients of science itself, concerned to help science make progress. The nature of science, the philosophy of science, and the relationship between the two, all change dramatically.[20]

Exploring probing questions about what the aims of science are, and ought to be, goes much further. For science seeks truth presupposed to be explanatory - explanatory truth as one might say - as a special case of the much more general aim of *valuable truth* - truth that is of intrinsic interest in some way, or of use. A science which increased our knowledge of irredeemably trivial, useless, utterly uninteresting truth would not be said to be making progress. Science both does, and ought to, seek truth that is of use or of value. Merely in order to be accepted for publication, a scientific paper must report a finding that meets some threshold of potential interest. Counting leaves on trees or pebbles on beaches does not, in itself, contribute to scientific knowledge even if the information is new and true.

But the aim of valuable truth is almost more problematic than that of explanatory truth. Of value to whom? And in what way? Is what science seeks to discover always of value to humanity, to those whose needs are the greatest? What of the links that science funding has with the military, corporations of one kind or another, and governments? Do the aims of science always respond to the curiosity and wonder of scientists, or sometimes to their career ambitions and vanity? Given that modern science is expensive, is there not always going to be an inherent conflict between the interests of those who pay for science - the wealthy and powerful - and those whose needs are the greatest - the poor and powerless?

If science is to pursue the problematic aim of valuable truth rationally, and in such a way that justice is done to the best interests of humanity, it is vital that science is pursued within the framework of a generalized version of AOE - humane AOE I have called it - so that three domains of discussion are recognized: (1) evidence; (2) theory; and (3)

aims. The third domain of discussion, aims, is as important as the first two. At present it is "repressed"; it goes on in fund giving committees, and in private between scientists, but not openly in journals and conferences along with (1) and (2).

Sustained exploration of the problematic aim of valuable truth needs to attempt to articulate (a) what we conjecture to be scientifically discoverable, and (b) what we conjecture it would be of value to discover, so that we may try to determine the all-important region of overlap between the two. The scientific community may have expertise when it comes to (a), but cannot have any exclusive expertise when it comes to (b). If science is to come to serve the best interests of humanity, it is vital that scientists and non-scientists alike cooperate in engaging in sustained imaginative and critical exploration of what it would be of most value for science to attempt to discover - what ought to be the aims and priorities of scientific and technological research. The institutional/intellectual structure of science needs to be changed to facilitate such aim-exploration. Journals and conferences need to be set up. Science journalism needs to contribute. SE, in misrepresenting the aim of science to be truth per se, in effect "represses" the real, problematic aim of valuable truth, and thus damages science by inhibiting the kind of sustained, cooperative exploration of actual and possible valuable aims science does, and might, pursue.[21]

It is important to appreciate that all this comes within the province of philosophy of science which is centrally concerned with problems about the aims and methods of science. Philosophy of science, in order to be done properly, must concern itself with moral, social, value questions about science. It must seek to call into question the less praiseworthy human aspirations science may seek to fulfil - the greed of corporations, the military might of some governments, the self-interests of some scientists. And it must explore neglected avenues of research that might lead to discoveries and technological developments of great potential value to humanity.

It does not stop here. For of course science seeks knowledge of valuable truth so that it may be used by people in life - ideally, so as to enhance and enrich the quality of human life. Science is to be used by people, either culturally, to aid the quest to know, to understand, or practically, as a means to the realization of other goals of value - health, security, travel, communications, entertainment, and so on. Science aims to contribute to the social world. There is a political dimension to the aims of science - once again, profoundly problematic. Everything said above about the value dimensions of the aims of science applies here too to the social, humanitarian or political dimensions. And this, too, comes within the province of philosophy of science, properly conceived. The orthodox distinction between "internal" factors (purely intellectual) and "external" (social, political, economic, evaluative) is a nonsense. At least, the way this distinction is usually drawn is a nonsense.[22]

6 Broader Implications

Elsewhere, I have argued that these considerations about the problematic aims of science have broader implications for social inquiry and the humanities, for academic inquiry as a whole, and for social life. In these fields, too, aims are profoundly problematic. A proper, basic task of social inquiry and the humanities is to help humanity build into the fabric of institutions and social endeavours - politics, industry, agriculture, economics, the law, the media, international relations - a generalization of the hierarchical, aim-improving methodology I have depicted above in connection with science.

The upshot of the argument is that we need a revolution in academia, so that the basic aim becomes, not just knowledge, but rather wisdom - this understood to be the capacity, the active endeavour, and the desire, to achieve what is of value in life, for oneself and others. Wisdom, in this sense, includes knowledge, technological know-how and understanding, but much else besides. The revolution in the nature, the aims and methods, of science is a special case of a broader revolution we need in academia, and in the social world, so that we may learn how to make social progress towards a wiser world.[23]

References

Barnes, B., 1977, *Interests and the Growth of Knowledge*, Routledge & Kegan Paul, London.
_____, 1982, *T. S. Kuhn and Social Science*, Macmillan, London.
_____, 1985, *About Science*, Blackwell, Oxford.
Barnes, B., D. Bloor, and J. Henry, 1996, *Scientific Knowledge: A Sociological Analysis*, University of Chicago Press, Chicago.
Bloor, D., 1976, *Knowledge and Social Imagery*, Routledge and Kegan Paul, London.
Feyerabend, P., 1965, 'Problems of Empiricism', in Colodny, R. G., ed., *Beyond the Edge of Certainty*, Prentice-Hall, New York, pp. 145-260.
_____, 1970, 'Problems of Empiricism, Part II', in Colodny, R. G., *The Nature and Function of Scientific Theories*, University of Pittsburgh Press, Pittsburgh, pp. 275-354.
_____, 1975, *Against Method: Outline of an Anarchistic Theory of Knowledge,* New Left Books, London.
_____, 1978, *Science in a Free Society,* New Left Books, London.
_____, 1987, *Farewell to Reason*, Verso, London.
Gross, P. and N. Levitt, 1994, *Higher Superstition: The Academic Left and Its Quarrels with Science*, John Hopkins University Press, Baltimore.
Gross, P., N. Levitt and M. Lewis, eds., 1996, *The Flight from Science and Reason*, John Hopkins University Press, Baltimore.
Koertge, N., 1998, *A House Built on Sand: Exposing Postmodernist Myths About Science*, Oxford University Press, Oxford.
Kuhn, T. S., 1962, *The Structure of Scientific Revolutions*, Chicago University Press, Chicago.
Lakatos, I., 1970, 'Falsification and the Methodology of Scientific Research Programmes', in Lakatos and Musgrave (1970, pp. 91-195).
Lakatos, I. and Musgrave, A., eds., 1970, '*Criticism and the Growth of Knowledge*, Cambridge University Press, London.
Maxwell, N., 1974, 'The Rationality of Scientific Discovery', *Philosophy of Science*, vol. 41, pp. 123-153 and 247-295.
_____, 1976, *What's Wrong With Science? Towards a People's Rational Science of Delight and Compassion*, Bran's Head Books, Hayes, Middlesex (2nd ed., 2007).
_____, 1980, 'Science, Reason, Knowledge and Wisdom: A Critique of Specialism', *Inquiry*, vol. 23, pp. 19-81.
_____, 1984, *From Knowledge to Wisdom: A Revolution in the Aims and Methods of Science*, Blackwell, Oxford.
_____, 1992, 'What Kind of Inquiry Can Best Help Us Create a Good World?', *Science, Technology and Human Values*, vol. 17, pp. 205-27.
_____, 1993, 'Induction and Scientific Realism: Einstein versus van Fraassen', *British Journal for the Philosophy of Science*, vol. 44, pp. 61-79, 81-101 and 275-305.
_____, 1998, *The Comprehensibility of the Universe: A New Conception of Science*, Clarendon Press, Oxford.
_____, 2000, 'Can Humanity Learn to Become Civilized? The Crisis of Science without

Civilization', *Journal of Applied Philosophy 17*, pp. 29-44.
_____, 2001, *The Human World in the Physical Universe: Consciousness, Free Will and Evolution*, Rowman and Littlefield, Lanham, Maryland.
_____, 2002, 'The Need for a Revolution in the Philosophy of Science', *Journal for General Philosophy of Science*, vol. 33, pp. 381-408.
_____, 2004, *Is Science Neurotic?*, Imperial College Press, London.
_____, 2005, 'Popper, Kuhn, Lakatos and Aim-Oriented Empiricism', P*hilosophia*, vol. 32, nos. 1-4, pp. 181-239.
_____, 2007a, *From Knowledge to Wisdom: A Revolution for Science and the Humanities*, Pentire Press, London (2nd ed. of my 1984, revised and extended).
_____, 2007b, 'From Knowledge to Wisdom: The Need for an Academic Revolution', *London Review of Education*, vol. 5, no. 2, pp. 97-115.
_____, 2008, 'Do We Need a Scientific Revolution?', *Journal for Biological Physics and Chemistry*, vol. 8, no. 3, pp. 95-105.
_____, 2009a, 'How Can Life of Value Best Flourish in the Real World?', in McHenry (2009a, pp. 1-56).
_____, 2009b, 'The Metaphysics of Science: An Account of Modern Science in Terms of Principles, Laws and Theories (review of book by Craig Dilworth), *International Studies in the Philosophy of Science*, vol. 23, no. 2, pp. 228-232.
_____, 2010, *Cutting God in Half - And Putting the Pieces Together Again: A New Approach to Philosophy*, Pentire Press, London (available online at http://discovery.ucl.ac.uk/view/people/ANMAX22.date.html).
_____, 2011, 'A Priori Conjectural Knowledge in Physics', in *What Place for the A Priori?*, ed., M. Shaffer and M. Veber, Open Court, Chicago, pp. 211-240.
_____, 2012a, 'Arguing for Wisdom in the University: An Intellectual Autobiography', *Philosophia*, vol. 40, pp. 663-704.
_____, 2012b, 'How Universities Can Help Humanity Learn How to Resolve the Crises of Our Times - From Knowledge to Wisdom: The University College London Experience', *Handbook on the Knowledge Economy*, vol. 2, ed. G. Heam, T. Katlelle and D. Rooney, Edward Elgar, Cheltenham, pp. 158-179.
_____, 2014a, *How Universities Can Help Create a Wiser World: The Urgent Need for an Academic Revolution*, Imprint Academic, Exeter.
_____, 2015, What's Wrong with Science and Technology Studies? What Needs to Be Done to Put It Right?, in Pisano R., ed., *A Bridge between Conceptual Frameworks*: *Sciences, Society and Technology Studies*, Springer, Dordrecht, pp. vii-xxxvii.
http://discovery.ucl.ac.uk/1400296/
_____, 2014b, ' Unification and Revolution: A Paradigm for Paradigms', *Journal for General Philosophy of Science*, vol. 45, pp. 133-149, http://philpapers.org/rec/MAXUAR.
_____, 2017a, *In Praise of Natural Philosophy*, McGill-Queen's University Press, Montreal.
_____. 2017b. *Understanding Scientific Progress*, Paragon House, St. Paul.
Popper, K., 1959, *The Logic of Scientific Discovery*, Hutchinson, London (first published in German in 1934)..
_____, 1963, *Conjectures and Refutations*, Routledge and Kegan Paul, London.
_____, 1969, *The Open Society and Its Enemies*, Routledge and Kegan Paul, London (first published in 1945).
_____, 1970, 'Normal Science and Its Dangers' in Lakatos and Musgrave (1970, pp.51-58).
_____, 1974, *The Poverty of Historicism*, Routledge and Kegan Paul, London.
Sokal, A., 1998, 'Transgressing the Boundaries; Toward a Transformative Hermeneutics of Quantum Gravity', in Sokal and Bricmont (1998, pp. 199-240).
_____, 2008, *Beyond the Hoax: Science, Philosophy and Culture*, Oxford University

Press, New York.
Sokal, A. and Bricmont, B., 1998, *Intellectual Impostures*, Profile Books, London.

Notes

[1] See Popper (1959; 1963).

[2] "inter-subjective *testing* is merely a very important aspect of inter-subjective *criticism*, or in other words, of the idea of mutual rational control by critical discussion." Popper (1959, p. 44, note 1*). Popper refers the reader to his (1969, chs. 23 and 24) - first published in 1945.

[3] See Popper (1969).

[4] As in note 4.

[5] See my (2012a, pp. 688-699).

[6] Feyerabend (1975; 1978; 1987).

[7] Harry Collins, John Henry and others were, and still are (at the time of writing) associated with the movement.

[8] Bloor (1976); Barnes (1977; 1982; 1985); Barnes, Bloor and Henry (1996).

[9] Sokal (1998). See also Sokal (2008) for an annotated version of the hoax article, and essays on related matters.

[10] Gross and Levitt (1994); Gross, Levitt and Lewis (1996).

[11] Sokal and Bricmont ((1998).

[12] Koertge (1998).

[13] Kuhn (1962); Lakatos (1970).

[14] See my (1974; 1993; 1998; 2000b; 2002; 2004; 2005; 2007a, chs. 9 and 14; 2008; 2009b; 2011; 2014b; 2017a; 2017b).

[15] Here is a demonstration of this point. Let T be any accepted fundamental physical theory. There are, to begin with, infinitely many disunified rivals to T that are *just as empirically successful* as T. In order to concoct such a rival, T1 say, all we need to do is modify T in an entirely *ad hoc* way for phenomena that occur after some future date. Thus, if T is Newtonian theory (NT), NT1 might assert: everything occurs as NT predicts until the first moment of 2050 (GMT) when an inverse cube law of gravitation comes into operation: F = Gm1m2/d3. Infinitely may such disunified rivals can be concocted by choosing infinitely many different future times for an abrupt, arbitrary change of law. These theories will no doubt be refuted as each date falls due, but infinitely many will remain unrefuted. We can also concoct endlessly many disunified rivals to T by modifying the predictions of T for just one kind of system that we have never observed. Thus, if T is, as before, NT, then NT2 might assert: everything occurs as NT predicts except for any system of pure gold spheres, each of mass greater than 1,000 tons, moving in a vacuum, centres no more than 1,000 miles apart, when Newton's law becomes F = Gm1m2/d4. Yet again, we may concoct further endlessly

many equally empirically successful disunified rivals to T by taking any standard experiment that corroborates T and modifying it in some trivial, irrelevant fashion - painting the apparatus purple, for example, or sprinkling diamond dust in a circle around the apparatus. We then modify T in an *ad hoc* way so that the modified theory, T3 say, agrees with T for all phenomena except for the trivially modified experiment. For this experiment, not yet performed, T3 predicts - whatever we choose. We may choose endlessly many different outcomes, thus creating endlessly many different modifications of T associated with this one trivially modified experiment. On top of that, we can, of course, trivially modify endlessly many further experiments, each of which generates endlessly many further disunified rivals to T. Each of these equally empirically successful, disunified rivals to T - T1, T2, … T∞ - can now be modified further, so that each becomes *empirically more successful* than T. Any accepted fundamental physical theory is almost bound to face some empirical difficulties, and is thus, on the face of it, refuted - by phenomena A. There will be phenomena, B, which come within the scope of the theory but which cannot be predicted because the equations of the theory cannot (as yet) be solved. And there will be other phenomena, C, that fall outside the scope of the theory altogether. We can now take any one of the disunified rivals to T, T1 say, and modify it further so that the new theory, T1*, differs further from T in predicting, in an entirely *ad hoc* way, that phenomena A, B and C occur in accordance with empirically established laws LA, LB and LC. T1* successfully predicts all that T has successfully predicted; T1* successfully predicts phenomena A that ostensibly refute T; and T1* successfully predicts phenomena B and C that T fails to predict. On empirical grounds alone, T1* is clearly more successful and better corroborated, than T. And all this can be repeated as far as all the other disunified rivals of T are concerned, to generate infinitely many empirically more successful disunified rivals to T: T1*, T2*, … T∞*.

[16] For expositions of this argument see Maxwell (1974, part 1; 1993, part 1; 1998, ch. 2; 2000b; 2002; 2004, ch. 1; 2005; 2011; 2013.

[17] For expositions of, and arguments for AOE see works referred to in note 22.

[18] Maxwell (2004, pp. 39-47).

[19] Maxwell (1998, chs. 3-6; 2004, chs 1, 2, and appendix; 2007a, ch. 14; 2014b; 2017a; 2017b).

[20] See works referred to in note 15.

[21] See my (1976; 1984; 2001; 2004; 2007a; 2010; 2014a; 2017a; 2017b).

[22] See previous note.

[23] See Maxwell (1976; 1980; 1984; 1992; 2000, 2004; 2007a; 2007b; 2008; 2009a; 2012a; 2014; 2017a, ch. 8).