

# DEFENDING SCIENCE — WITHIN REASON

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Let us remember how common the folly is, of  
going from one faulty extreme into its opposite

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## ABSTRACT

*We need to find a middle way between the exaggerated deference towards science characteristic of scientism, and the exaggerated suspicion characteristic of anti scientific attitudes — to acknowledge that science is neither sacred nor a confidence trick. The Critical Commonsensist account of scientific evidence and scientific method offered here corrects the narrowly logical approach of the Old Deferentialists without succumbing to the New Cynics' sociologism or their factitious despair of the epistemic credentials of science.*

Attitudes to science range all the way from uncritical admiration at one extreme, through distrust, resentment, envy, to denigration and outright hostility at the other. We are confused about what science can and what it can't do, about how it does what it does, about the role of science in society and the role of society in science, about the relation of science to literature, about science and religion,

Complicated as they are, the confusions can be classified as of two main kinds: the scientistic and the anti-scientific. The former is a matter of an exaggerated kind of deference

towards science an excessive readiness to accept as authoritative any claim made by the sciences, for example, and to dismiss every kind of criticism of science or its practitioners as anti-scientific prejudice. The latter is a matter of an exaggerated kind of suspicion of science — an excessive readiness to see the interests of the powerful at work in every scientific claim, for example, or to accept any kind of criticism of science or its practitioners as undermining its pretensions to tell us how the world is.

Disentangling the confusions is made harder by an awkward ambiguity. Sometimes the word “science” is used simply as a way of referring to certain disciplines — physics, chemistry, biology, and so forth, usually also anthropology and psychology, sometimes also sociology, economics, and so on. But often — perhaps more often than not — “science” and its cognates are used honorifically: advertisers urge us get our clothes cleaner with new, scientific, Wizzo, teachers of critical thinking urge us to reason scientifically, to use the scientific method, expert witnesses are believed on the grounds what they offer is scientific evidence, astrology, water-divining, homeopathy or chiropractic or acupuncture are dismissed as pseudo-science, skeptical of this or that claim, we complain that it lacks a scientific explanation, or demand scientific proof. And so on. “Scientific” has come to be an all-purpose term of epistemic praise, meaning “strong, reliable, good.” No wonder, then, that psychologists and sociologists and economists are sometimes so zealous in insisting on their right to the title. No wonder, either, that practitioners in other areas (“Management Science,” “Library Science,” “Military Science,” “Mortuary Science,” etc.) are so anxious to claim it.

In view of the impressive successes of the natural sciences, this honorific usage is understandable enough. But it is unfortunate. It obscures the otherwise obvious fact that

not all, or only, practitioners of disciplines classified as sciences are good, honest, thorough, successful inquirers. It has tempted some philosophers of science into a fruitless preoccupation with the problem of demarcating *real* science from pretenders. It encourages too thoughtlessly uncritical an attitude to the disciplines classified as sciences. This in turn provokes envy of the disciplines so classified, which encourages a kind of scientism — inappropriate mimicry, by practitioners of other disciplines, of the manner, the technical terminology, the mathematics, etc., of the natural sciences. And it provokes resentment of the disciplines so classified, which encourages anti-scientific attitudes. Sometimes you can even see the envy and the resentment working together — for example, with those self-styled ethnomethodologists who undertake “laboratory studies” of science, observing, as they would say, part of the industrial complex in the business of the production of inscriptions, or — one has, grudgingly, to admit the rhetorical brilliance of this self-description — with “creation science.” And (the point that chiefly concerns me now) this honorific usage stands in the way of a straightforward acknowledgement that science — science, that is, in the descriptive sense — is neither sacred nor a confidence trick.

Science is not sacred. Like all human enterprises, it is thoroughly fallible, imperfect, uneven in its achievements, often fumbling, sometimes corrupt, and, of course, incomplete. Neither, however, is it a confidence trick. The natural sciences, at any rate, have surely been the most successful of human cognitive enterprises.

To acknowledge this is not at all to deny the legitimacy or denigrate the achievements of other kinds of inquiry, of history or philosophy or of legal or literary scholarship, for instance, nor is it to deny the legitimacy or denigrate the achievements of literature or art. It is, however, to put on

the agenda some hard questions about whether and if so how those disciplines classified as sciences differ among themselves, about whether and if so how other kinds of inquiry differ from those classified as sciences, about how we learn from art or literature, when neither is a kind of inquiry, and about the place of imagination, metaphor, and linguistic innovation in science

The core of what needs to be sorted out is epistemological, i e , it concerns the nature and conditions of scientific knowledge, evidence, and inquiry We need an account of what the sciences know and how they know it which will be realistic in the ordinary, non-technical sense of neither over-estimating nor under-estimating what the sciences can do The task is difficult as well as urgent For mainstream philosophy of science has sometimes erred in the direction of over-estimating science, and this has left it unable effectively to answer that great shrill chorus of voices which, erring dramatically in the other direction, has of late decried its epistemic pretensions

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Once upon a time — the phrase is a warning that what follows will be cartoon history — the epistemic *bona fides* of good empirical science needed to be defended against the rival claims of sacred scripture or a priori metaphysics Before long it came to be taken for granted that science enjoys a peculiar epistemic authority because of its uniquely objective and rational method of inquiry Successive efforts to articulate what that uniquely objective and rational method might be gave rise to umpteen competing versions of what I shall call the Old Deferentialist position science progresses inductively, by accumulating true or probably true theories

confirmed by empirical evidence, by observed facts, or deductively, by testing theories against basic statements and, as falsified conjectures are replaced by corroborated ones, improving the verisimilitude of its theories, or instrumentally, by developing theories which, though not themselves capable of truth, are efficient instruments of prediction, or, etc., etc. Of course, there were many obstacles. Humean skepticism about induction, the paradoxes of confirmation, the “new riddle of induction” posed by Goodman’s “grue”, Russell Hanson’s and others’ thesis of the theory-dependence of observation, Quine’s of the underdetermination of theories even by all possible observational evidence. But these obstacles, though acknowledged as tough, were assumed to be superable, or avoidable.<sup>2</sup>

It is tempting to describe these problems in Kuhnian terms, as anomalies facing the Old Deferralist paradigm just as a rival was beginning to stir. Kuhn himself, he tells us, did not intend radically to undermine the pretensions of science to be a rational enterprise. But most readers of *The Structure of Scientific Revolutions*, missing many subtleties and many ambiguities, heard only science progresses, or “progresses,” not by the accumulation of well-confirmed truths or even by the repudiation of well-refuted falsehoods, but by revolutionary upheavals in a cataclysmic process the history of which is afterwards written by the winning side, there are no neutral standards of evidence, only the incommensurable standards of different paradigms, the success of a scientific revolution, like the success of a political revolution, depends on propaganda and control of resources, a scientist’s shift of allegiance to a new paradigm is less like a rational change of mind than it is like a religious conversion — a conversion after which things look so different to him that we might almost say he lives “in a different world.”

Even so, when, a quarter of a century ago now, Feysabend proclaimed that there is no scientific method, that appeals to “rationality” and “evidence” are no more than rhetorical bullying, that science is not superior to, only better entrenched than, astrology or voodoo, he was widely regarded — he described himself — as the “court jester” of the philosophy of science. Mainstream philosophers of science, adding “incommensurability” and “meaning-variance” to their list of obstacles to be overcome, conceding that much more work is needed on the details, sometimes trimming back their conception of the goal of science to demand only problem-solving or empirical adequacy rather than truth, nevertheless mostly continued, and continue, to be convinced that the Old Deferentialism is correct in essentials.

Of late, however, radical sociologists, radical feminists and multiculturalists, radical followers of Paris fashions in literary theory, rhetoric, and semiology, and philosophers outside strictly philosophy-of-science circles, have turned their attention to science. And they construe the difficulties still regarded in mainstream philosophy of science as obstacles to be overcome in a proper account of scientific rationality — underdetermination, incommensurability, and the rest — as radically undermining the claim of science to be a rational enterprise.

We have arrived, in short, at the New Cynicism. Now it is commonplace to hear that science is largely or wholly a matter of social interests, of negotiation, or of myth-making, the production of inscriptions, narrative, that appeals to “fact” or “evidence” or “rationality” are nothing but ideological humbug disguising the exclusion of this or that oppressed group. The natural world, Harry Collins writes, “has a small or non-existent role in the construction of scientific knowledge”,<sup>3</sup> the validity of theoretical propositions

in the sciences, Kenneth Gergen assures us, “is in no way affected by factual evidence”<sup>4</sup> According to this new orthodoxy, not only does science have no peculiar epistemic authority and no uniquely rational method, it is really, like all purported “inquiry,” just politics “Feminist science,” Ruth Hubbard writes, “must insist on the political nature and content of scientific work”<sup>5</sup> “I don’t see any difference,” Steve Fuller announces, “between ‘good scholarship’ and ‘political relevance’ Both will vary, depending on who[m] you are trying to court in your work”<sup>6</sup> “The only sense in which science is exemplary,” Richard Rorty tells us, “is that it is a model of human solidarity”<sup>7</sup>

It isn’t enough just to protest that this is ridiculous, it isn’t enough, even, to show, in however much detail, that what the New Cynics offer in place of evidence or argument for their startling claims is an incoherent farrago of confusion, *non sequitur*, and rhetoric An adequate defense against the extravagances of the New Cynicism requires an adequate account of the epistemology of science — a realistic account, in the sense explained earlier

And this the Old Deferentialism cannot supply Not, as the New Cynics imagine, because there *is* nothing epistemologically special about science, but because *what* is epistemologically special about science is subtler, less direct, and a bit less reassuring, than the Old Deferentialism supposes

Not surprisingly, perhaps, the most serious flaws in the Old Deferentialist paradigm are not quite overt, but a matter of mistaken focus The Old Deferentialism tends to focus attention too exclusively on science, or else on science and, by way of foil, “pseudo-science” — the covert presumption being that standards of good evidence and well-conducted, imaginative, thorough, honest inquiry are somehow specifically scientific, internal to the sciences It

tends to concern itself too exclusively with narrowly logical dimensions, ignoring or downplaying the epistemic significance of imagination, conceptual innovation, communication, the social character of science — and often, throwing the spotlight on observation statements, seems to relegate the world to the shadows. It asks too much of the distinction of context of discovery versus context of justification — and, in its anxiety to relegate non-logical factors to the discovery phase, has inadvertently encouraged the New Cynics in thinking of the justification of theories as a rhetorical activity in which scientists engage, rather than a matter of how good their evidence is. Old Deferentialist conceptions of the structure of evidence, furthermore, not only in their deductivist but also in their inductivist manifestations, have been too narrowly (i.e., too formally) logical.

So perhaps it is not so surprising that, to an outsider, the explanations of scientific rationality offered in recent philosophy of science sometimes sound less reassuring than unnerving, whether because, as with Critical Rationalism, granting that science seeks significant truths, it fails so spectacularly to explain how it achieves this goal, or because, as with recent variants and descendants of instrumentalism, it is able to show how science succeeds only by an implausible attenuation of what it is that science aims to do.

Perhaps some New Cynics — those who insist on the legitimacy of other ways of knowing than the scientific, or emphasize the fact that, whatever else it is, science is a large and powerful social institution, or stress the similarities between linguistic innovation in science and in literature, the role of imagination in both — vaguely sense some of these defects of the Old Deferentialist approach. But this does not justify the extravagant conclusions they invite us to draw: that there is no real difference between the supposed discoveries of science and other stories we tell to help our-



selves to cope, that what scientific theories get accepted is wholly determined by social forces or political interests, and so forth and so on

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Emphatically declining that invitation, I shall try to articulate an account — “Critical Commonsensism” — which can correct the over-optimism of the Old Deferentialism without succumbing to the factitious despair of the New Cynicism. The New Cynics’ major themes are that there are no objective epistemic standards, and there is nothing epistemologically special about science. These themes both encourage and are encouraged by the New Cynics’ strategy of shifting attention from the evaluative notion of warrant (how good the evidence is with respect to this or that scientific claim) to the descriptive notion of acceptance (the standing of this or that claim in the eyes of the relevant scientific sub-community). But there *are* objective epistemic standards, and there *is* something epistemically special about the sciences. The Old Deferentialism rightly acknowledges this, but in the wrong way. Science is not *privileged* epistemologically, but *distinguished*, the point being that distinction, unlike privilege, has to be earned. The natural sciences deserve, not uncritical deference, but tempered respect.

One might almost say that as the Old Deferentialism, once itself a rebellion against an older orthodoxy, became an orthodoxy itself, and as the hard-earned distinction of the natural sciences was allowed to congeal into uncriticized privilege, the exaggerated response of the new rebels was only to be expected. But the exaggerated response is as unnecessary as the supposed epistemic privilege of science is

undefensible. Our standards of good, strong, supportive evidence and of well-conducted, honest, thorough, imaginative inquiry are not internal to the sciences. In judging where science has succeeded and where it has failed, in what areas and at what times it has done better and in what worse, we are appealing to the standards by which we judge the solidity of empirical beliefs, or the rigor and thoroughness of empirical inquiry, generally. But the sciences, at least some of them at least some of the time, have succeeded remarkably well by those standards.

To say that standards of good evidence and well-conducted inquiry are not internal to the sciences is not to say that a lay person is able to judge the evidence for a scientific claim or the conduct of a scientific inquiry as well as someone in the relevant scientific specialism. Often — usually — only a specialist can judge the weight of the evidence or the thoroughness of the precautions against experimental error, etc., for such judgments are apt to require a broad and detailed knowledge of background theory, not to mention a familiarity with technical vocabulary, not easily available to the lay person. But, though only specialists may be in a position to judge the worth of this or that evidence, nevertheless, respect for evidence, care in weighing it and persistence in seeking it out, are neither exclusively nor essentially scientific desiderata, but are the standards by which we judge *all* inquirers — detectives, historians, investigative journalists, etc.

The presumption that epistemic standards (supposing, as they would say, that there were any) would be internal to science also plays a covert role in encouraging a dreadful argument ubiquitous among the New Cynics — an argument intimately bound up with their shift of attention away from warrant and onto acceptance. Since, the argument goes, what has passed for, i.e., what has been accepted by

scientists as, known fact or objective evidence or honest inquiry, etc , has sometimes turned out to be no such thing, the notions of known fact, objective evidence, honest inquiry etc , are revealed to be ideological humbug. The premiss is true, manifestly, however, the conclusion doesn't follow. Indeed, this dreadful argument — I call it the "Passes-for Fallacy"<sup>8</sup> — is not only fallacious, but self-undermining, for if the conclusion were true, the premiss could not be a known fact for which objective evidence had been discovered by honest inquiry. The obvious response is available to the Critical Commonsensist: scientific inquiry does not always live up to the epistemological ideal, but only by honest investigation of the evidence can we find out when and where it fails — a response which, however, is not quite so easily available to one who supposes that the epistemological ideal is set by the sciences.

More important for my purposes than the distinction of discovery and justification, is the distinction between standards of good evidence and rules or guidelines for the conduct of inquiry. These differ rather as standards for judging roses in a flower show differ from rules or guidelines for growing them — the latter, but not the former, would inevitably mention horse manure, or as standards of nutritiousness differ from rules or guidelines for menu-planning. The goal of scientific inquiry is substantial, significant, explanatory truth. And, as it is easier to produce a nutritious meal if one neglects the other aspect of the goal of menu-planning, palatability, it is easier to come up with truths if one doesn't mind the truths one gets being trivial or insignificant. Standards of good evidence are focussed on just one aspect of the goal, on truth-indicativeness, guidelines for the conduct of inquiry, however, must focus on substance and significance as well as truth. That is why there can't be rules for conducting inquiry, instructions that

could be followed mechanically, but only guidelines requiring discretion, good judgment, in their application

The structure of evidence — to use an analogy on which I have long relied — is less like a mathematical proof than a crossword puzzle<sup>9</sup> (As I only recently discovered, Einstein once observed that a scientist is like “a man engaged in solving a well-designed word-puzzle”)<sup>10</sup> Think of the controversy over that meteorite discovered in Antarctica, thought to have come from Mars about 11,000 years ago, and containing what might possibly be fossilized bacteria droppings. Some scientists think this is evidence of early bacterial life on Mars, others agree they are bacterial traces, but think they might have been picked up while the meteorite was in Antarctica, others think they are not bacteria droppings, but were formed at volcanic vents, and others again think that what look like fossilized bacteria droppings might be only artifacts of the instrumentation<sup>11</sup> How do they know that giving off these gases indicates that the meteorite comes from Mars? that the meteorite is about 4 billion years old? that this is what fossilized bacteria droppings look like? — like crossword entries, reasons ramify in all directions

How reasonable a crossword entry is depends on how well it is supported by its clue and any already-completed intersecting entries, how reasonable those other entries are, independent of the entry in question, and how much of the crossword has been completed. How warranted an empirical claim is depends, analogously, on how well it is supported by experience and background beliefs, how warranted those background beliefs are, independent of the claim in question, and how much of the relevant evidence the evidence includes

Not all scientific theories are well-supported by good evidence. Most get discarded as the evidence turns out

against them, nearly all, at some stage of their career, are only tenuously-supported speculations, and doubtless some get accepted, even entrenched, on flimsy evidence. Nevertheless, the natural sciences, at least, have come up with deep, broad and explanatory theories which are well anchored in experience and interlock surprisingly with each other, and, as plausibly filling in long, much-intersected entries in a crossword puzzle greatly improves one's prospects of completing more of the puzzle, these successes have enabled further successes.

The goal, remember, is realism, and that requires a candid acknowledgment that where the social sciences are concerned it is not so easy to think of examples of discoveries analogous to plausibly filled-in, long, much-intersected crossword entries. That, indeed, is part of the reason why some are reluctant to acknowledge the social sciences as sciences. This puts some more hard questions on the agenda, about whether the explanation for the less impressive record of the social sciences lies simply in their relative youth, or goes deeper, perhaps, as some have thought, is inevitable given their subject-matter.

Still, the natural sciences, at least, have succeeded strikingly well by our standards of empirical evidence. How have they done this? Not because they are in possession of a uniquely rational and objective method of inquiry, unavailable to historians, detectives, and the rest of us, and guaranteed to produce true, or probably true, or progressively more nearly true, or progressively more empirically adequate, etc., results. "The scientific method," P. W. Bridgman wrote, "as far as it is a method, is nothing more than doing one's damndest with one's mind, no holds barred"<sup>12</sup>. And, as far as it is a method, I would add, it is also what historians or detectives or investigative journalists or the rest of us do when we really want to find

something out — it is what I do when I try to figure out why this dish came out better this time than the last time I cooked it — make an informed conjecture about the possible explanation of a puzzling phenomenon, see how it stands up to the best evidence you can get, and then use your judgment whether to accept it, more or less tentatively, or modify, refine, or replace it

Nevertheless, there *is* something special about inquiry in the natural sciences, or rather, a lot of things — experimental contrivance of every kind, special techniques of statistical evaluation and mathematical modelling, systematic commitment to criticism and testing, and to finding ways to isolate one variable at a time, and the engagement of many persons, co-operative and competitive, within and across generations

E. O. Wilson describes his work on the pheromone warning system of red harvester ants — collect ants, install them in artificial nests, dissect freshly killed workers, crush the tiny gobbets of white tissue released, and present this stuff, on the sharpened ends of applicator sticks, to resting groups of workers — they “race back and forth in whirligig loops” — Enlist a chemist, who uses gas chromatography and mass spectrometry to identify the active substances, and then supplies pure samples of identical compounds synthesized in the laboratory — Present these to the ant colonies — same reaction as before — Enlist a mathematician, who constructs physical models of the diffusion of the pheromones — Then design experiments to measure the rate of spread of the molecules and the ants’ ability to sense them.<sup>13</sup>

Tackling one problem with the help of solutions to others,<sup>14</sup> devising techniques and instruments with the help of one theory to test another, the natural sciences have built small successes into large, which have enabled more small successes, which have been built into larger successes, ,

and so on. And this would not have been possible had natural-scientific inquiry not been, in the sense explained, a social enterprise.

The social character of scientific inquiry is neither epistemologically irrelevant, as some Old Deferentialists are too ready to assume, nor epistemologically devastating, as some New Cynics are too ready to conclude. It is one of the ways in which science has extended and deepened the method of experience and reasoning — the method all of us use when we are seriously trying to figure out some empirical question. No, more than that: acknowledging the social character of scientific inquiry is essential to understanding how it has succeeded as well as it has — and to understanding potential threats to its continued success.<sup>15</sup>

An adequate account of scientific knowledge and scientific inquiry must acknowledge a subtle interplay of logical, personal, and social aspects. The interplay begins at the beginning, of course, with talented individuals coming up with imaginative conjectures on which others build and which are subject to the scrutiny of the whole relevant community, and it is present at every stage. The warrant of any empirical proposition depends in part on experiential evidence, i.e., on what some individual observer(s) see(s) or hear(s), etc., and so, also, on how justified others are in thinking the observer(s) reliable.

The point that chiefly concerns me now, though, concerns how acceptance gets appropriately correlated with warrant. Scientific claims are better and worse warranted, and there is a large grey area where opinions may reasonably differ about whether a claim is yet sufficiently warranted to put in the textbooks, or should first be put through further tests, or assessed more carefully relative to an alternative, or whatever. There can no more be rules governing when a theory should be accepted and when rejected than

there could be rules for when to ink in a crossword entry and when to rub it out “The” best procedure is for different scientists, some bolder, some more cautious, to proceed differently

The conception of scientific inquiry I have been articulating is realistic not only in the ordinary, non-technical sense, but also in some of the technical senses in which this term has been used in recent philosophy of science<sup>16</sup> the goal of scientific inquiry is substantial, significant truth, scientific theories are normally either true or else false, and the entities, kinds and laws postulated in true scientific theories are real Perhaps I need to add that all I mean by describing a scientific claim or theory as true is that things are as it says, if it says, for example, that DNA is a double-helical, backbone-out macromolecule with like-with-unlike base pairs, it is true just in case DNA is a double-helical, backbone-out macromolecule with like-with-unlike base pairs

I haven’t much sympathy with old-fashioned instrumentalism, since I don’t believe there is sharp line to be drawn between observation statements, capable of truth and falsity, and theoretical “statements,” incapable of truth-value Nor have I much sympathy with new-fangled constructive empiricism I am happy to concede, however, that the truth-claims scientists make in presenting their theories are seldom categorical or dogmatically confident, usually guarded and tentative

And my approach is not “realist” in the strongly progressive interpretations sometimes associated with the word Yes, as the natural sciences have proceeded, a vast sediment of well-warranted claims has accumulated But there is no guarantee that at every step the sciences accumulate more truths, or replace false theories by true ones, or get nearer the truth, nor any guarantee that currently accepted theo-



ries, even in “mature” sciences, are true or approximately true. At any time, some parts of science may be advancing, some stagnating, and others, quite possibly, regressing. Where there is progress, it may be a matter of the accumulation of new truths, or of the replacement of discredited theories by better ones, in which case the new theory may entail that the old was correct in a limited domain, or may partially overlap it, and/or may introduce new concepts which can be translated into the old by clumsy circumlocution. Or progress may be not at the level of theory, but a matter, rather, of new instruments or tests or techniques, or of a better vocabulary.

Questions about objectivity require a similarly nuanced approach. A scientific claim is either true or else false — true or false objectively, *i.e.*, independent of whether anybody believes it. The evidence for a scientific claim is stronger or weaker — stronger or weaker objectively, *i.e.*, independent of how strong or how weak anybody judges it to be. But there is no guarantee that every scientist is entirely objective, *i.e.*, is a completely unbiased and disinterested truth-seeker. Scientists are fallible human beings, they are not immune to prejudice and partisanship. But the natural sciences have managed, by and large and in the long run, to overcome individual biases by means of an institutionalized commitment to mutual disclosure and scrutiny, and by competition between partisans of rival approaches — by an internal organization, in other words, that has managed on the whole to keep most scientists, most of the time, reasonably honest.

These complex issues are confused by that popular stereotype of “the scientist” as objective in the sense, not merely of being free of bias or prejudice, but as unemotional, unimaginative, stolid, a paradigmatically convergent thinker. Perhaps some scientists are like this, but not,

thank goodness, of all them “Thank goodness,” because imagination, the ability to envisage possible explanations of puzzling phenomena, is essential to successful scientific inquiry, and because a passionate obsession with this or that problem, even, not so seldom, a passionate commitment to the truth of this or that elegant but as yet unsupported conjecture, or a passionate desire to best a rival, have contributed to the progress of science

As this reveals, when I speak of “bias and partisanship” what I have primarily in mind is, so to speak, *professional* bias and partisanship—a scientist’s too-ready willingness to accept an approach or theory because it was thought up by his mentor, or because of his own many years’ investment in developing it, or his too-ready willingness to dismiss an approach or theory because it was thought up by his rival in the profession, or because of his own many years’ investment in developing an alternative, and so on. In the New Cynics’ camp, by contrast, the focus is on *political* prejudice and partisanship, on the sexism, racism, classism, etc., with which the New Cynicism perceives science as pervaded. Where the physical sciences are concerned, given the manifest irrelevance of sex, race, class, to the content of physical theory, the idea seems foolish. Where the human and social sciences are concerned, however, given the manifest relevance of sex, race, class, to the content of some theories, political and professional preconceptions come together, and it seems only exaggerated.

This, indeed, suggests the beginnings of part of an answer to one of the questions earlier put on the agenda: one relevant difference between the social and the human sciences, on the one hand, and the physical sciences, on the other, may be, precisely that, given the subject-matter of the former, some of the prejudices apt to get in the way of honest inquiry are political as well as professional. A para-

graph some pages back suggests that another relevant difference may be that, at the present stage of development of the social sciences, their adoption of the mathematical techniques and the co-operative and competitive inquiry that have helped the natural sciences to build on earlier successes have tended to encourage, instead, a kind of affected, mathematized, obscurity, and prematurely gregarious (or dogmatically factional) thinking

Another stereotype, this time perhaps more philosophical than popular, of “the scientist” as an essentially critical thinker, as refusing to take anything on authority, also obscures the picture. A systematic commitment to testing, checking, mutual disclosure and scrutiny is one of the things that has contributed to the success of natural-scientific inquiry, but this commitment is, and must be, combined with the institutionalized authority of well-warranted results. The point is not that crossword entries once inked-in never have to be revised, but that only by taking some for granted is it possible to isolate one variable at a time, or to tackle a new problem with the help of others’ solutions to older problems. This puts more questions, this time about the nature, grounds, and limits of authority in science, on the agenda.

Thus far, I have stressed the ways in which the social character of natural-scientific inquiry has contributed to its success by division of labor of course, by enabling the combination of creativity and carefulness, imagination and rigor, systematic criticism and institutionalized authority of well-warranted results, essential to successful inquiry, by overcoming, and even putting to productive use, the human imperfections of individual scientists. But it must be acknowledged that both the internal organization and the external environment of science may be more or less conducive to good, fruitful, inquiry.

The disasters of Soviet and Nazi science remind us how grossly inquiry can be distorted and hindered if scientists come to seek to make a case for politically-desired conclusions rather than to find out how things really are. Less melodramatic, but still disturbing, among the other potential hindrances that come immediately to mind are the necessity to spend large amounts of time and energy on obtaining funds, and to impress whoever supplies them, in due course, with one's success, dependence for resources on bodies with an interest in the results coming out this way rather than that, or in rivals being denied access to them, pressure to solve problems perceived as socially urgent rather than those most susceptible of solution in the present state of the field, a volume of publications so large as to impede communication rather than enabling it, and so forth.

It would be less than candid not to admit that this list does not encourage complacency about the present condition of science. Once, important scientific advances could be made with the help of a candle and a piece of string, but it seems scientists have made most of those advances. As science proceeds, more and more expensive equipment is needed to obtain more and more *recherche* observations. And, inevitably, the more science depends for resources on governments and large industrial concerns — the bodies capable of providing that kind of money — the worse the danger of some of the hindrances described in the previous paragraph. Scientific techniques and instruments grow ever more sophisticated, but the mechanisms which have thus far proven more or less adequate to sustain intellectual integrity are strained.

Scientific evidence is usually the shared resource of a whole sub-community of scientists, inquiry in the sciences, cooperative or competitive, involves many people within

and across generations, and science is not conducted in a vacuum, but in a larger social setting which can exert a significant influence on what research gets funded, what results get a wide audience, and sometimes on what conclusions get reached. Old Deferentialists, however, often assumed that social aspects of science, though they might have some bearing on the epistemologically uninteresting context of discovery, could be at best negatively relevant to the epistemologically crucial context of justification. Not a few sociologists of science, at least partly aware of the inadequacies of the Old Deferentialists' narrowly logical model, seem to have been attracted to the New Cynicism — perhaps in part because, unlike the Old Deferentialism, it offers a flatteringly large and important role to people like themselves. And this in turn has reinforced the disinclination of mainstream philosophers of science, and of scientists themselves, to take sociology of science seriously as a potential ally in the task of understanding the scientific enterprise.

This “quarrel between epistemology and sociology” has obscured the otherwise obvious fact that progress may be enabled or impeded by the way science is organized internally, and by its external context. Unlike the cynical sociology of science that has recently been fashionable, a sensible sociology of science could illuminate what aspects of the internal organization and of the external environment of science encourage good, thorough, honest inquiry, efficient communication of results, effective testing and criticism.

As we need to distinguish sensible from cynical sociology of science, we also need to distinguish the sense in which it is true and epistemologically important that science is social — that scientific inquiry is an enterprise requiring the engagement, co-operative and competitive, of many persons — from the fashionable but false interpretations prompted

by amnesia or skepticism about the question of warrant, the role of evidence that the goal of science is the achievement of socially desirable aims, that the acceptance of scientific theories comes about by a kind of “social negotiation”, that science would be improved by a more “democratic” epistemology, that scientific knowledge, even that reality, is nothing but a “social construction”, that the natural sciences are subordinate to the social sciences, and so forth

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Since both scientism and anti-scientific attitudes have their roots in misunderstandings of the character and limits of scientific inquiry and scientific knowledge, the focus thus far has been epistemological. But this is neither to deny the legitimacy nor to denigrate the importance of those difficult questions — ethical, social and political questions — about the role of science in society: who should decide, and how, what research a government should fund? who should control, and how, the power for good and evil unleashed by scientific discoveries? , and so on.

As this suggests, the vexed question of science and values is vexed, in part, because of its many ambiguities. Scientific inquiry is a kind of inquiry, so epistemic values, chief among them respect for evidence, are necessarily relevant (which is not to say that scientific inquiry always or inevitably satisfies epistemic desiderata or exemplifies epistemic values). But, as the previous paragraph reminds us, there are also moral and political questions both with respect to scientific procedure (for example, about whether some ways of obtaining evidence are morally unacceptable), and with respect to scientific results (for example, about whether and how access to and applications of potentially explosive sci-

entific results should be controlled) — *that* ambiguity, by the way, was intentional!

Some among the New Cynics seem to imagine that the fact that scientific discoveries can be put to bad uses is a reason for doubting the *bona fides* of those discoveries, and some seem to take for granted that those who think that science has made many true discoveries, or even there is such a thing as objective truth, reveal themselves to be morally deficient in some way. But it isn't enough simply to point out the obvious confusion, nor simply to protest the blatant moral one-up-personship. It is essential, also, to articulate sober answers to those difficult questions about the role of science in society — to point out, *inter alia*, that only by honest, thorough inquiry can we find out what means of achieving desired social changes would be effective. And, as always, it is essential to avoid the exaggerations of the scientific party as well as the extravagances of the anti-science crowd — to point out, *inter alia*, that decisions about what ways of handling the power that scientific knowledge of the world gives us are wise or just, are not themselves technical questions that may responsibly be left to scientists alone to answer.

\* \* \* \* \*

Shrewd as I find that observation of Reid's with which I began, I am mindful, also, of the cautionary story — I don't know whether it is myth or history — of the student who is said to have written in his Introduction to Philosophy examination "Some philosophers believe that God exists, and some philosophers believe that God does not exist, but the truth, as so often, lies somewhere in between." But in the present case the truth really *does* lie somewhere in between.

between the faulty extremes of the Old Deferentialism, on the one hand, and its opposite, the New Cynicism, on the other. Of course, this has been only a sketch of where in between, exactly, the truth lies. I'm working on it.<sup>17</sup>

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### Keywords:

Scientism and anti-science, scientific evidence, scientific method, sociology of science, science and values

### Notes

<sup>1</sup> Thomas Reid, *Essays on the Intellectual Powers* (1875), VI, 4, in Beanblossom, R. E. and Lehrer, K., eds, *Thomas Reid Inquiry and Essays*, Hackett, Indianapolis, IN, 1983

<sup>2</sup> In this and subsequent paragraphs I am drawing on my "Puzzling Out Science," *Academic Questions*, Spring 1995, 25–31, reprinted in my *Manifesto of a Passionate Moderate Unfashionable Essays*, University of Chicago Press, Chicago, 1998, 90–103

<sup>3</sup> Harry Collins, "Stages in the Empirical Programme of Relativism," *Social Studies of Science*, 11, 1981, p. 3

<sup>4</sup> Kenneth Gergen, "Feminist Critique of Science and the Challenge of Social Epistemology," in *Feminist Thought and the Structure of Knowledge*, ed. Mary M. Gergen, New York University Press, New York, 1988, 27–48, p. 37

<sup>5</sup> Ruth Hubbard, "Some Thoughts About the Masculinity of the Natural Sciences," in Gergen, *Feminist Thought and the Structure of Knowledge*, 1–15, p. 13

<sup>6</sup> Steve Fuller, e-mail posting, 5/4/94



<sup>7</sup> Richard Rorty, "Science as Solidarity," in John S Nelson, Allan Megill, and Donald M McCloskey, eds, *The Rhetoric of the Human Sciences*, University of Wisconsin Press, Madison, WI, 1987, 38–52, p 46

<sup>8</sup> A phrase I first introduced in "Knowledge and Propaganda Reflections of an Old Feminist," *Partisan Review*, LX 4, 1993, reprinted in *Manifesto of a Passionate Moderate*, 123–36 See also my "Staying for an answer," *Times Literary Supplement*, July 9th, 1999, 12–14

<sup>9</sup> See my "Theories of Knowledge An Analytic Framework," *Proceedings of the Aristotelian Society*, 83, 1982–3, 143–57, "Rebuilding the Ship While Sailing on the Water," in *Perspectives on Quine*, eds Robert Barrett and Roger Gibson, Blackwell, Oxford, 1990, 111–27, and *Evidence and Inquiry Towards Reconstruction in Epistemology*, Blackwell, Oxford, 1993

<sup>10</sup> Albert Einstein, "Physics and Reality," *The Journal of the Franklin Institute*, March 1936, reprinted in *Ideas and Opinions of Albert Einstein*, trans Sonja Bargmann, 1954, p 295, drawn to my attention by John Norton in 1996

<sup>11</sup> Adam Rogers, "Come in, Mars," *Newsweek*, 20th October, 1996, 56–7, Sharon Begley and Adam Rogers, "War of the Worlds," *Newsweek*, February 10th, 1997, 56–8

<sup>12</sup> Percy R Bridgman, *Reflections of a Physicist*, Philosophical Library, New York, 1955, p 535

<sup>13</sup> E O Wilson, *Consilience The Unity of Knowledge*, Knopf, New York, 1999, 69–70

<sup>14</sup> I borrow this happy phrase from Quine, *From Stimulus to Science*, Harvard University Press, Cambridge, MA, 1995, p 16

<sup>15</sup> See also my "Science as Social? — Yes and No," in *Feminism, Science, and Philosophy of Science*, eds Jack Nelson and Lynn Hankinson Nelson, Kluwer, Dordrecht, The Netherlands, 1996, 79–93, reprinted in *Manifesto of a Passionate Moderate*, 104–22

<sup>16</sup> For a disambiguation of the many senses of "realism," see Haack, "Realism," forthcoming in *Handbook of Epistemology*, eds Ilkka Niiniluoto, Matti Sintonen, and Jan Wolenski, Kluwer, Dordrecht, The Netherlands, 2000

<sup>17</sup> This article is the first chapter of a forthcoming book of the same title