Diálogos, 64 (1994) pp. 201-215.

KITCHER ON THE ADVANCEMENT OF SCIENCE* ROBERTO TORRETTI

For the man of the street few things are more obvious than the progress of science. He may dismiss the naïve idea that increasing knowledge will essentially improve the human lot, but there can be no doubt in his mind that we who imitate planetary motion with our space probes know more about it than Cardinal Bellarmino, and that our synthetic fibers and drugs bear witness to a much deeper understanding of the composition of dead and living bodies than anything one can read in Aristotle. Even granting a questionable distinction between know-how or τέχνη, displayed in material achievements, and "know-that" or iπιστήμη, expressed in informative statements, the fact remains that our more impressive technical abilities rest squarely upon a massive accumulation of epistemic lore. Therefore, it is no wonder that undergraduates suffer a shock when their instructors tell them that since the late fifties a small but growing number of philosophers and sociologists of science —whom I shall here designate, collectively and indiscriminately, as 'the Negators'1— contend that the very idea of an advancement of science is illusory and ultimately nonsensical, because there are no non-conventional, "transcultural," criteria by which to judge an absolute increase in knowledge.

Philip Kitcher's new book musters a complex set of ideas designed to show that, notwithstanding the validity of some of the Negators' insights, science can effectively progress towards strictly cognitive goals and that it has in fact done so in some episodes which the Negators cite as proving

* Review of Philip Kitcher, The Advancement of Science: Science without Legend, Objectivity without Illusions (New York: Oxford University Press, 1993). ix + 421 pp.

¹ Kitcher gives a partial list on p. 198: "Feyerabend, Barnes, Bloor, Shapin, Schaffer, Collins, Latour."

201

their case. The book contains a wealth of lovingly researched examples from the history of science and is on the whole too rich to be profitably summarized in a few pages. I shall therefore address only a few major points, viewed in isolation, and disregard the tightly knit argument that connects them. To sense the full force of the latter one must read the book itself. I strongly recommend it to everyone who cares for the subject.

Kitcher speaks of science in the singular, as of one, synchronically and diachronically coherent, human project, beginning thousands, perhaps millions of years ago (cf. the references to "our hominid ancestors" on pp. 241 and 299), and continuing, presumably through every known civilization, to the present day. This is reminiscent of the novelist H.G. Wells' much derided approach to history and will no doubt irk our historical sensitivities. But Kitcher's argument could well vindicate his monism (unless the latter performs as a tacit premiss of the former). On the other hand, a proof that our modern sciences have been making genuine epistemic progress from the 17th century onwards would be hailed by most of us as sufficient rebuttal of the Negators.

Kitcher sees science, at any given stage, as an ensemble of shared "consensus practices". "A scientist's practice" includes the following "components":

- 1. The language that the scientist uses in his professional work.
- 2. The questions that he identifies as the significant problems of the field.
- 3. The statements (pictures, diagrams) he accepts about the subject matter of the field.
- 4. The set of patterns (or schemata) that underlie those texts that the scientist would count as explanatory.
- 5. The standard examples of credible informants plus the criteria of credibility that the scientist uses in appraising the contributions of potential sources of information relevant to the subject matter of the field.
- 6. The paradigms of experimentation and observation, together with the instruments and tools which the scientist takes to be reliable, as well as his criteria for experimentation, observation, and reliability of instruments.

202

Exemplars of good and faulty scientific reasoning, coupled with the criteria for assessing proposed statements (the scientist's "method-ology").

Kitcher acknowledges that his notion of consensus practice has links to Kuhn's concept of a paradigm. However, Kuhn's paradigms are meant "to divide the history of a scientific discipline into segments, such that there are significant epistemological differences between the course of science within a segment and the intersegment transitions" (p. 87 n. 39). Kitcher's approach implies no such differences, inasmuch as consensus practices may be changing in a roughly continuous way.

With the phrase "the language that the scientist uses" in (1) Kitcher does not so much point to what one normally understands by it —say, Darwin's English or Einstein's German— as to the elusive entities variously referred to in philosophy as 'categorial frameworks' or 'conceptual schemes'. I suppose that Kitcher avoids these expressions because they are loaded with hidden assumptions and too strongly associated with the Negators' standpoint. But the following explanation discloses the purport of (1).

Learning a language [...] involves acquiring propensities for forming certain types of generalizations. Alternatively, the language embodies a view of where the divisions in nature are, and to learn the language is to acquire a propensity to see those divisions as natural.

The word "accept" in (3) is sufficiently vague to leave room for modes of acceptance which do not amount to belief. Such alternative modes of acceptance are common in physics. For example, due to the incompatibility between General Relativity and quantization, no physicist in his right mind would *believe* that the world is a semiriemannian 4manifold with a metric determined by the distribution of matter in accordance with the Einstein field equations. Yet most cosmologists currently *accept* this statement as a working principle providing the best available conceptual framework for their ongoing research. Kitcher, however, does not dwell on this matter and when, later on, he goes into details he apparently identifies acceptance with belief. This raises the fear that, despite the sophisticated talk about complex practices, his book may ulti-

(p. 74)

(p. 80)

mately tend to reinvigorate the boorish view of science as a stockpile of beliefs (the village atheist's substitute for religion).

Scientific practices have goals and their progress must be judged by their capacity for attaining them. Kitcher distinguishes practical goals and epistemic or cognitive goals, and concentrates exclusively on the latter. "The most obvious pure epistemic goal is truth" (p. 93). However, "truth is very easy to get" (p. 94). But science is not interested in such cheap truths as "the minutiae of the shapes and colors of the objects in your vicinity" or "the infinite number of disjunctions you can generate with your favorite true statement as one disjunct". So "what we want is significant truth. Perhaps, as I shall suggest later (Section 8), what we want is significance and not truth" (p. 94). In the next paragraph (still on p. 94), Kitcher spells this out as "the impersonal epistemic goal of fathoming the structure of the world," or, "in a less aggressively realist language," of organizing "our experience of nature". I find this whole passage very attractive, both for its off-hand dismissal of silly scepticism and its implicit admission that little or nothing is won by overcoming it. Unfortunately, Section 8 - to which we are referred - contributes very little to clarify the full implications, say, for the oneness and the historical continuity of science, of understanding it as a quest for "significance and not truth". And Chapters 7 and 8, which take up nearly half the book, discuss scientific decision making as a choice between statements, based on their apparent truth, not their significance.

With science comprising so many strands, there must be as many distinct ways in which it may advance (or regress). Kitcher pays special attention to three varieties of scientific progress —which he terms conceptual, explanatory and erotetic- corresponding to the first, fourth and second components of scientific practices, and also, indeed, to progress in belief, which ends up by dominating the scene.² Progress along components (5), (6) and (7) is not discussed, presumably because informants, instruments and inferences can be readily judged by the beliefs they tend to support.³

Kitcher gives precise definitions of conceptual and explanatory progress (pp. 104f., 111). The former turns wholly on his novel and important notion of the reference potential of terms. This can be briefly explained as follows. Whatever makes it the case that a given token of a term refers to certain object is the mode of reference of that token. "The compendium of modes of reference for a term (type)" is the reference potential of that term (p. 78). Typically it will include a baptismal mode reference of the term's first token-,4 descriptive modes of reference which pick out anything that satisfies a specific description, plus the conformist modes of reference of the many tokens of the term whose utterer "intends that her usage be parasitic on those of her fellows (or her own earlier self)" (p. 77). Thus, "reference potentials are typically heterogeneous: [...] the linguistic community [...] allows a number of distinct ways of fixing the reference of tokens of terms" (p. 78). This holds in particular of theory-laden scientific terms: The theoretical hypotheses

² I wonder why he has no special name for the latter. Why not call it 'pistic'? Or is it that in Kitcher's mind progress in belief, the increasing quality and quantity of accepted statements, is epistemic progress κατ' έξοχήν?

³ Still, most scientific beliefs, even about particular matters of fact, must be evaluated in the light of the inferences, personal and impersonal observations and reports leading to them, so a special study of the criteria of progress for components (5), (6) and (7) may well be warranted.

⁴ According to Kitcher one must admit "the possibility of autonomous baptismal modes of reference" because without them "how is any link between language and nature ever established?" (p. 79). Nevertheless, he mentions a difficulty that, in my view, stands on the way of such a possibility. He illustrates it with this example: "Imagine a brave soul ostending a tiger and courageously introducing 'tiger'. The object before him is a tiger-but it is also a carnivore, a mammal, a quadruped, a vertebrate and a striped animal. What makes 'tiger' refer to the set of tigers and not (say) to the set of quadrupeds?" (p. 79). Then he sketches his solution: "Without supposing that the linguistic innovator has stored and ready for production a description that would delimit the tigers, we can suppose that there are features of his cognitive state -- propensities-that would discriminate some things as relevantly similar and others as not. There is no reason to think that these discriminatory dispositions as I shall call them are completely determinate. But, I shall imagine, if there is a unique natural kind that includes the object ostended and that conforms to the discriminatory dispositions, then the kind is the referent of 'tiger'" (pp. 79f.). So far so good. However, Kitcher's listing of what his speaker ostends is too short (and categorially too uniform). His ur-zoologist is also gesturing towards a piece of fur, a striking color pattern, a source of danger, a process of combustion, a surface homeomorphic to the torus, and many other things, some of which defy our ontological imagination. Surely anything we point at belongs to many "natural kinds" and without a clear grasp of the specific "discriminatory dispositions" at work in any given act of ostension neither the audience nor the ostender himself can understand his meaning. Moreover, even adding this proviso to Kitcher's explanation of ostensive reference, I doubt it would work quite as he says. Imagine a Laputan mathematician whose "discriminatory dispositions" are confined almost exclusively to the topological attributes of surfaces. Oblivious of the digestive tract, she points at a tiger and utters 'tiger', meaning sphere. Then, according to Kitcher's semantics, she would have established 'tiger', in her parlance, as the word for torus.

with which they are laden are jointly equivalent to "the assertion that all the modes of reference fix reference to the same entity" (p. 103).

set C_1 of expressions in the language of P_1 such that

- ence potential
- referring to that kind
- out the pertinent kind.

Conceptual change is change in reference potential—a characterization that sounds very well to me provided that such change is not understood as a mere reshuffling of existing modes of reference, but allows for radical innovation. (Think of the advent of modern physical concepts like 'mass', 'energy', 'field', 'chance', conferring new, undreamt of, modes of reference on old nouns). With these resources at hand, Kitcher can introduce a notion of *conceptual progressiveness*: A practice P_2 is conceptually progressive with respect to a practice P_1 just in case there is a set C_2 of expressions in the language of P_2 and a except for the expressions in these sets, all expressions that occur (a) in either language occur in both languages with a common referfor any expression e in C_1 , if there is a kind to which some token (b) of *e* refers, then there is an expression e^* in C_2 which has tokens for any e, e*, [as in (b)], the reference potential of e* refines the ref-(C) erence potential of e, either by adding a description that picks out the pertinent kind or by abandoning a mode of reference determination belonging to the reference potential of e that failed to pick (pp. 104f.) The definition is rather subtle so I shall illustrate it with an example. We would like to say that the current practice P_2 which distinguishes between positive and negative electrons is conceptually progressive with respect to the early 20th century practice P_1 which only acknowledged electrons endowed with a negative electric charge. The expression 'electron' is common to both languages, but its referent potential differs. Thus this expression is not covered by (a). Consequently, 'electron' as used in P_1 belongs to the set C_1 . If, as we believe, there is natural kind to which J. J. Thomson or Niels Bohr referred when they said 'electron', there must be in the language of P_2 an expression which has tokens that refer to that same kind (clause (b)). That expression is, of course, 'negative electron'; its reference potential refines the reference potential of P_1 's 'electron' by adding a description that picks out the pertinent kind (cf. clause (c)).

One might object, however, that the definition does not cover those rare but extremely important cases in which the practitioners of a new practice consider it conceptually progressive precisely because it involves rethinking the field of inquiry in such a way that some terms which were central in the language of the earlier practice turn out to have no reference whatsoever. I have reflected about an example of this sort and find that, contrary to my initial impression, it can be brought under Kitcher's definition. My example is the expression 'simultaneous events', as used before Einstein. From the standpoint of Special Relativity, Newtonian, i.e., frame-independent 'simultaneity' is, strictly speaking, a term without referents. However, one would usually grant that pre-relativistic use of the expression 'event A is simultaneous with event B' is meaningful if subject to caveats such as these: (i) 'simultaneous' is understood to within a reasonable margin of imprecision; (ii) 'simultaneous' is tacitly referred either to the momentary rest-frame F_A at the time of A of a body α involved in A or to the momentary rest-frame F_B at the time of B of a body β involved in B; (iii) when distances are measured in one of these frames events A and B are fairly close to each other; (iv) F_A moves fairly slowly relative to F_B . The caveats provide then —as required by clause (c)— a refinement of the reference potential of the term 'simultaneous' naïvely used in the earlier practice. I surmise that a treatment along similar lines can supply relativistic referents for 'mass' and 'force' as used in prerelativistic mechanics, and general relativistic referents for 'straight line' and 'inertial motion' as used in special relativity. Of course, this approach to the old practice is only possible for someone who endorses or at any rate prefers the new one. Indeed, as the reader will readily recognize, this is true also of the 'electron' example discussed in the previous paragraph (for a follower of Thomson's practice, the adjective 'negative' prefixed to 'electron' is not a refinement but a pleonasm). But then, as a matter of fact, any actual judgements of conceptual progressiveness will normally be dictated, pace Kitcher, by what he calls "the chauvinism of the present".

Explanatory progressiveness is defined in terms of the familiar notion of explanatory schema, which Kitcher illustrates with three well-chosen examples on pp. 107f.

 P_2 is explanatorily progressive with respect to P_1 just in case the explanatory schemata of P_2 agree with the explanatory schemata of P_1 except in one or more cases of one or more of the following kinds.

- P_2 contains a correct schema that does not occur in P_1 . (a)
- P_1 contains an incorrect schema that does not occur in P_2 . (b)
- P_2 contains a more complete version of a schema that occurs in P_1 . (c)
- P_2 contains a schema that correctly extends a schema of P_1 . (d)

Here everything hangs on the notion of *correctness*. Kitcher admits two ways of understanding it, between which he does not make a final choice. On the one hand, we have "strong realism", as exemplified by Wesley Salmon's Scientific Explanation and the Causal Structure of the World (Princeton 1984). It holds that there exists a ready-made world neatly articulated into interacting things and interdependent processes, so that a correct explanatory schema is one which appeals in effect to the way the world works. Kitcher does not embrace this creed but he remains silent with regard to the fierce difficulties facing the strong realist who seeks to justify ascriptions of correctness to explanatory schemata developed in the course of the last few centuries on what appears to be a fairly untypical planet with the ludicrous means afforded by our R&D budgets. He mentions, however, an alternative -- "Kantian in spirit"which he has explained elsewhere⁵ and is here summarized as follows: Scientific explanation consists in achieving a unified vision of the phe-

nomena. We can conceive of the sequence of practices in a field as attempting to modify the language and the set of explanatory schemata so as to achieve the greatest unity among the set of accepted statements. Unity, in this case, is, to a first approximation, understood in terms of generating the largest set of consequences through the use of the smallest number of patterns (schemata). More precisely, let a systematization of a set of statements be a collection of derivations, all of whose constituent statements (premises, conclusions, intermediate steps) belong to the set. Each systematization can be seen as instantiating a set of schemata, the basis of the systematization. The greatest unification of our system of beliefs is obtained when we use a systematization which

generates as many conclusions as possible and whose basis contains the smallest number of most stringent schemata.

From this standpoint, what does it mean to say that an explanatory schema is correct, that it "records the objective dependencies in nature"? Here is Kitcher's answer to this question:

Consider science as a sequence of practices that attempt to incorporate true statements (insofar as is possible) and to articulate the best unification of them (insofar as is possible). As this sequence proceeds, certain features of the organization of beliefs may stabilize: predicates of particular types may be used in explanatory schemata and employed in inductive generalization; particular schemata may endure (possibly embedded in more powerful schemata). The "joints of nature" and the "objective dependencies" are the reflections of these stable elements. The natural kinds would be the extensions of the predicates that figured in our explanatory schemata and were counted as projectible in the limit, as our practices developed to embrace more and more phenomena. Objective dependencies would be those recorded in the schemata that emerged in the limit of our practices.

The causal structure of the world, the divisions of things into kinds, the objective dependencies among phenomena are all generated from our efforts at organization. To say that a particular predicate picks out a natural kind is thus to claim that marking out the extension of that predicate would figure in the ultimate (ideal) practice. Hailing a schema as correct is predicting that that schema will have a part in the ideal unification of the phenomena.

I agree wholeheartedly. Yet, frankly speaking, I am unable to see why one should not also countenance a variety of such pursuits, each advancing by its own lights, in accordance with its own standards, simultaneously or successively, towards divergent or at any rate non-conver-

6 A footnote inserted after "in the ultimate (ideal) practice" explains that this phrase properly means "in all practices beyond a particular point in a properly developed sequence" and that "here the notion of proper development of a sequence of practices is partially understood in terms of the attempt to conform to the principle of unification" (p. 173 n. 67).

(p. 111)

(p. 171)

(pp. 172f., my italics)6

⁵ Kitcher, "Explanatory Unification", Philosophy of Science 48: 507-531 (1981); "Projecting the Order of Nature", in Butts, ed., Kant's Philosophy of Physical Science (Dordrecht: Reidel, 1986), pp. 201-235; "Explanatory Unification and the Causal Structure of the World", in Kitcher and Salmon, eds., Scientific Explanation (Minneapolis: University of Minnesota Press, 1989), pp. 410-505.

gent "organizations of experience". Anyway, this is what even a cursory glance at the history of man's cognitive endeavors will disclose to us if we hold our Procrustean instincts in check.

Chapters 6, 7 and 8 are concerned with scientific decision making, the process by which some consensus practices are dropped by the community of scientists and others are adopted instead. After discussing several, more or less demanding, standards for judging such shifts,7 Kitcher presents a rationalist and an antirationalist model of that process -the latter summarizing the views of the Negators- followed by his own compromise model:

- The community decision is reached when sufficiently many suffi-(C1) ciently powerful subgroups within the community have arrived at decisions (possibly independent, possibly coordinated) to modify their practices in a particular way.
- Scientists are typically moved by nonepistemic as well as epis-(C2) temic goals.
- There is significant cognitive variation within scientific communi -(C3)ties, in terms of individual practices, underlying propensities, and exposure to stimuli.
- During early phases of scientific debate, the processes undergone (C4)by the ultimate victors are (usually) no more well designed for

(ES) The shift from one individual practice to another was rational if and only if the process through which the shift was made has a success ratio at least as high as that of any other process used by human beings (past, present, and future) across the set of epistemic contexts that includes all possible combinations of possible initial practices (for human beings) and possible stimuli (given the world as it is and the characteristics of the human recipient)."

ES reeks indeed of philosophy-of-science-fiction, but it is only meant to get the argument going and is subsequently relaxed in various respects.

(p. 189)

promoting cognitive progress than those undergone by the ultimate losers.

Scientific debates are closed when, as a result of conversations (C5)among peers and encounters with nature that are partially produced by early decisions to modify individual practices, there emerges in the community a widely available argument, encapsulating a process for modifying practice which, when judged by [the standards canvassed in earlier sections], is markedly superior in promoting cognitive progress than other processes undergone by protagonists in the debate; power accrues to the victorious group principally in virtue of the intention of this process into the thinking of members of the community and recognition of its virtues.

Clauses (C1)-(C3) reproduce to the letter the first three clauses of the antirationalist model, and embody Kitcher's acceptance of the Negators' chief insights. Clauses (C4) and (C5) follow the corresponding clauses of the rationalist model, but differ from them significantly. Kitcher grants that "it may be too optimistic to think that every debate, even every major debate, in the history of science has been resolved in accordance with the compromise model" (p. 202). But he is persuaded that if scientific decision follows the compromise model we can rest assured that "we discover more and more about the world while simultaneously learning how to investigate the world". However, those who, believe this are, Kitcher says, "vulnerable to skeptical challenges to the effect that cardinal tenets of contemporary science are mistaken". Since he rejects the possibility of an a priori foundation for science or for methodology, he can only answer the skeptics "by pointing out that our current knowledge is the product of a self-correcting process". He concedes that this defense will not hold "if there are transitions in the history of science which could have been made differently, on the basis of cognitively equivalent processes, and which would have yielded very different contemporary practices". But he apparently believes that this possibility does not exist if "all transitions in the history of science accord with the compromise model" (p. 202).

This belief is correct if, but only if, one views cognitive processes as deterministic and takes "cognitively equivalent" in a very narrow sense (so that, for instance, the slightest difference in the blood composition of two copies of Einstein on Dec. 4, 1903 at noon GMT betokens an in-

(p. 201)

The tone of the discussion is set by Kitcher's formulation of the "extremely demanding" external standard ES. "Let A include all the processes of cognitive modification that have been, are, and will be used by human beings. Let C include all ordered pairs of possible practices and sequences of stimuli that the world will afford human subjects. I shall assume that C is large, but finite. Let the improvement set of [a process of cognitive modification] P be the set of epistemic contexts in which P would yield a progressive shift in practice. The success ratio of P is the ratio of the cardinality of P's improvement set to the cardinality of C. The criterion of adequacy demands that the success ratio of a process be the maximal success ratio for members of A. Putting all this together:

equivalence of the cognitive processes each copy is going through). On this view it is of course trivially impossible for two series of transitions based on cognitively equivalent processes to yield different practiceseven if the compromise model is not followed! But if we take a more sensible view of cognitive equivalence and social causation, we must countenance the possibility that, say, in Putnam's Twin Earth,8 at twin 1905 A.D., someone lighted on an idea or started a practice that did not occur to anyone on Earth in 1905, so that thereafter the self-correcting process led there to a very different sort of science from the one that developed here. Thus, if the compromise model is at all relevant to our discussion, it is possible that transitions based on cognitively equivalent processes eventually lead to very different practices. For any decision made in accordance with the compromise model rests on the actual scientific debates, not on what could have happened if, in the course of some cognitively equivalent process, one of the participants had lighted on a different idea. Of course, if one resolutely embraces the alternative "Kantian in spirit" delineated in the above quotations from pp. 171 and 172f., one can indeed dismiss developments on Twin Earth as wholly irrelevant to human science and the history of human reason. But the "strong realist" must take them seriously: if there is a ready-made world, the fact that science evolves reasonably is not sufficient to ensure its truth. Which makes me wonder why Kitcher treats "strong realism" so respectfully. But perhaps he seeks thus to undermine "strong realism" in the reader's mind without openly confronting this socially powerful adversary. Chapter 7, "The Experimental Philosophy", tackles the self-correcting

process of scientific change. We are expressly reminded of the multidimensionality of scientific practices (p. 221), but the discussion in effect concentrates on change of belief. Simple observation by itself rarely warrants such change: "justifications of the acceptance and rejection of statements, even at the level of 'empirical data,' are usually complex" (p. 222). Tempering one's observations --moving, say, from 'the temperature is 37.5 C' to 'the mercury column reached the line half-way between the 37 and 38 marks'— "does not yield reports that are entirely innocent of commitment to doctrine, neutral descriptions of what is given to the senses. The point of tempering is to achieve descriptions that only com-

mit the subject to points of doctrine that are shared with her rivals and detractors. At this level, the opponents can agree on what they see, and they can use their agreement to attempt to decide whether either of their more ambitious ways of reporting observations is warranted" (p. 227). The dialectics leading to such decisions is considered by Kitcher both at the general level and in a few illuminating historical cases. His approach turns on the notion of an inductive propensity, i.e., "a disposition to generalize with respect to certain ways of classifying entities and picking out their properties [...,] given certain beliefs about the observed instances" (p. 235). To introduce this notion, Kitcher bids us "imagine that scientists [...] have isolated a set of entities, the A's, and that the question 'How do the A's exemplify D(B)?' is significant for them—where B is some determinate property (for example, being blue) and D(B) is the corresponding determinable (in this instance, color)" (pp. 233f.). Let O(A) be the set of observed instances of A's and R a condition that O(A) must meet in order to be judged as a representative sample.

The normal form of an inductive generalization is as follows: . .

- All members of A have some form of D(B). [1]
- The distribution of the determinate forms of D(B) among the ob-[2] served members of A, O(A), is $(p_1, ..., p_n)$.
- O(A) satisfies R. [3]

Therefore

The distribution of the determinate forms of D(B) in A is [4] $(f(p_1), \dots, f(p_n))$, where f is a function that maps the frequencies $(p_1, ..., p_n)$ of the determinate properties within O(A) onto the answer $(f(p_1), \dots, f(p_n))$ that is accepted when the subject believes that O(A) meets R.

To my mind, the crucial stage is [1], "the framing of the problem through the specification of A and D(B)" (p. 234). Problem-framing is, of course, the main occasion for discontinuity and radical novelty in the history of science. I have a feeling that Kitcher does not sufficiently emphasize this fact when he deals with the problem-framing stage of his historical examples. But thirty years after Kuhn's Structure of Scientific Revolutions such emphasis is perhaps unnecessary, and one may simply take novelty and discontinuity for granted.

(paraphrased from p. 235)

⁸ Hilary Putnam's Twin Earth is a remote planet which resembles ours in everything except some specified feature (and its consequences); see Putnam, Philosophical Papers, Vol. I (Cambridge: Cambridge University Press, 1975), pp. 223-27.

Kitcher argues effectively for the "old-fashioned" idea that induction proceeds through the elimination of alternatives. In Section 4 of Chapter 7 he shows "how to characterize an inductive propensity that works eliminatively and how to understand certain types of scientific inference in terms of the activation of this propensity" (p. 237). Since eliminative induction operates on previously given generalizations and there must have been "some initial stage at which the eliminative propensity could first be put to work", the following question inevitably arises: "How did people arrive at the views about explanatory dependencies that were embodied in the practice on this stage?"

My answer is that the eliminative propensity is overlaid on a more primoitive propensity to generalize. As a consequence of our genotypes and our early developmental environments, human beings come initially to categorize the world in a particular way, to view certain kinds of things as dependent on others, to generalize from single instances of especially salient types. Moreover, just as there is a propensity to form certain generalizations, so too there is a propensity to restrict those generalizations in particular ways when matters go awry. I suggest that this primitive apparatus works tolerably well in confronting the problems that our hominid ancestors encountered: it is relatively well designed for enabling primates with certain capacities and limitations to cope with a savannah environment and with the complexities of a primate society. Whether it is well designed for advancing scientific investigations, the primitive apparatus stands behind our primitive scientific practices. With those practices in place, the eliminative propensity can be activated, and the use of that propensity (together with other types of inference to be considered in Section 7) allows for the modification of practice, the revision of the primitive categorizations and views of dependence.

(p. 235)

Kitcher can feel comfortable with his genetic deus ex machina because he consistently ignores what is to me the most striking fact in the history of science: the recurring irruption "out of the blue" of genuinely new concepts and explanatory patterns (think only of the classical conception of time as a linear continuum, or of Riemann's idea of an n-fold extended continuous quantity, i.e., a differentiable manifold). Kitcher does not even mention this when he tackles -alas, all-too-briefly- the question "How is conceptual reform itself initiated?" (pp. 259f.).

According to Kitcher, "the general problem of social epistemology is to identify the properties of epistemically well-designed social systems, that is, to specify the conditions under which a group of individuals, operating according to various rules for modifying their individual practices, succeed, through their interactions, in generating a progressive sequence of consensus practices" (p. 303). Chapter 8, "The Organization of Cognitive Labor", contains a very innovative, admittedly preliminary, discussion of this problem, which employs "an analytic idiom inspired by Bayesian decision theory, microeconomics, and population biology" (p. 305). This makes for precision, which helps in working out implications and exposing hidden assumptions. Of course, precision requires idealization. As Kitcher gallantly admits: "My toy scientists do not behave like real scientists, and my toy communities are not real communities" (p. 305). No matter what one may think of this exercise, it deserves credit for what Kitcher (p. 388) regards as its "minimal contribution", viz., the rebuttal of the notion that "the existence of social pressures and nonepistemic motivations" implies that true epistemic progress is impossible. Nowadays it is generally agreed that profit-seeking entrepreneurship is more conducive to the wealth of nations than socially-minded central planning. There is some analogy between this seeming paradox and Kitcher's conclusion that credit-seeking, rival-tripping scientists need not hinder and may well favor the quest for truth. Piquantly enough, he proves it by the mathematical methods of capitalist managerial science.

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