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The Progress of Science and Implications for Science Studies and for Science Policy

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An essay review of *Prematurity in Scientific Discovery* by Ernest Hook (ed.); *Striking the Mother Lode in Science* by Paula E. Stephan & Sharon G. Levin; *Prometheus Bound* by John Ziman

PREAMBLE AND SYNOPSIS

Can the progress[1] of science be deliberately guided or accelerated?

That "science policy" is a recognized specialty implies that the answer is, "Yes." But science policy should stem from an understanding of how science works, how or why it has progressed in the past---in particular the recent past, the era of modern science. Yet there is little agreement over those matters among scholars in science studies (or science and technology studies, STS for short) or among the disciplines that STS seeks to integrate, mainly history of science, philosophy of science, sociology of science[2].

To the extent that there is any broad consensus, it would be that

- Science is a significantly intellectual activity;
- Like other human activities, it is influenced by the aspirations and emotions of those who practice it and by the institutions they have developed;
- Again as with other human activities, its institutions interact with other social institutions.

Attempts to understand, and designs to influence, the course of science should recognize all of these factors, the "internalist" intellectual as well as the "externalist" social ones.

Prematurity in Scientific Discovery examines the notion that the acceptance within science of certain scientific discoveries seems to have been unduly delayed. That calls for judgment to be exercised over past decisions, raising the specters of Whiggishness and presentism. Though the book's focus is primarily internalist, contextual considerations are raised by a number of the contributors. *Prometheus Bound* expounds the sea change undergone by the circumstances of scientific activity since the middle years of the 20th century---a change from "Little Science" and relative autonomy to "Big Science" and dependence on external institutions. This main theme might imply an externalist approach, but it is the norms under which members of the scientific community operate that is key to the whole discussion; contextual issues are here so tightly connected to scientists' practice that the internalist-externalist distinction is not very useful. *Striking the Mother Lode in Science* is chiefly externalist in emphasizing the role played by society's support for science, but it does not neglect the effects of that on the intellectual climate within the scientific community.

Prematurity in Scientific Discovery addresses, too, the question of whether it is possible to discern *contemporaneously* that a claim is premature---that it is "here-and-now" premature (Stent 1972a, 1972b, 2002a). A related question of long standing is whether one can discern contemporaneously that a new claim could for sound reasons be called pseudo-science or pathological science (Bauer 1984a, pp. 135--53).

I shall argue---in agreement with most or all of the contributors to *Prematurity in Scientific Discovery*---that, to be useful for analysis of intellectual aspects of progress, more operational definitions are called for of terms like "premature" or "resisted" discoveries. One step toward that is to view science as advancing on three fronts: the observation of striking new phenomena, the introduction of new methods, and the development of new theories[3]. Typically, however, change does not occur at the same time in all three aspects of this troika. Drastic change in any aspect is resisted; proposed simultaneous change in two of them causes a discovery to be neglected, isolated from the mainstream action. Claims that change is needed simultaneously in all three aspects tend to be dismissed as pseudo-science (Bauer 1983; 1986, pp. 152--53; 2001a, pp. 9--11; 2001b, pp. 96--99).

In analyzing specific cases, a number of distinctions need to be respected: the maturity of a given research field matters a great deal; the extent to which disciplinary boundaries are crossed also matters a great deal; and "science" should not be presumed to subsume medicine and technology.

Since the progress of science can only be viewed from some perspective or other, discussion of it leads naturally to a consideration of scholarship about progress in science, in other words progress in science studies and science policy.

PROGRESS IN SCIENCE

Modern science, dating from roughly the 17th century, differs from earlier incarnations in these respects:

- It is international; that is to say, scientific communities in all cultures agree---by and large---about the substantive content of scientific knowledge, the reliability of scientific methods, and the applicability of scientific theories.
- The feedback between theory and evidence is tight: implications of theories are quickly subjected to empirical explorations, and the acceptance of new observations hinges on their perceived fit with the reigning paradigm.
- The various scientific specialties are tightly interconnected: theories and methods of physics find use in geology, chemistry, and biology; some form of molecular biology, for example, is practiced by people trained in any of these fields. Knowledge in any specialty is expected not to contradict knowledge in any other scientific specialty.
- Observational and experimental methods display considerable technical sophistication. Noise-elimination techniques and computerized averaging permit detection of tiny signals. Analysis of computerized model systems extends exploration of theoretical implications far beyond what was previously possible.
- The scale of scientific activity, in absolute terms and also as a fraction of total social effort, is much greater.

One might immediately conclude that, at least for the nearer future[4], progress would comprise further advance along these lines. However, the scale of scientific activity cannot maintain the growth it has experienced since the 17th century; indeed, the latter part of the 20th century has already seen it slowing. The profound corollaries of that are explored in detail by John Ziman in *Prometheus Bound*. Among other things, the age structure of scientific communities is changing drastically, and implications of that are considered in *Striking the Mother Lode in Science*. The latter offers an unusually realistic, empirical approach from the viewpoint of economists, who are not often-enough heard from in STS[5].

DEFINING TERMS

Progress in science, resistance to progress, premature discoveries, even discovery itself (Gerson 2002, pp. 281, 283), are terms that seem, at first sight, to convey clear meaning; yet on examination they turn out to be problematic in application to specific historical situations.

Progress in science

Ways in which modern science represents progress over earlier incarnations have already been enumerated. But that happened on a time scale of centuries; finer-grained measures are needed if progress over periods of years or decades is of interest.

Such terms as scientific "discovery" or "revolution" imply discrete stages that might be used as markers of progress. Unfortunately, historians quite often find that the appearance of discontinuity dissipates under detailed examination of context and the identification of precursors: it is "not so obvious . . . what a 'unit' of scientific discovery is"---for example, Lavoisier is widely credited with the "discovery of oxygen," but there are several senses of what "oxygen" or its discovery might mean (Holmes 2002, pp. 165--66). Even so apparently discrete an event as the discovery of the Taung skull turns out to be a chain of events (Tobias 1984, pp. 25--27). This is of more than academic interest, since it results in disputes about priority and the award of prizes[6]. It is also worth pondering, whether the situation may vary somewhat among disciplines; a new theory in physics is more likely to arise in a creative flash than is an understanding of the biological function of an organelle.

One way of accommodating a measure of discreteness within overall continuity may lie in recognizing that scientific advances may flow from the observation of new phenomena, or from the introduction of new methods, or from the invention of a new theory, and that these do not typically occur simultaneously (Bauer 1983; 1986, pp. 152--53; 2001a, pp. 9--11; 2001b, pp. 96--99). Looking separately at these three aspects of science (the "troika") might reveal sharper discontinuities than any more holistic view of scientific progress could.

Given the difficulty of defining measures of progress, identifying a *rate* of progress is clearly problematic. That makes such ideas as resistance to discovery, premature discovery, delayed recognition, and the like equally problematic, since they imply progress slowed from some *normal* or *standard* or *ideal* or otherwise attainable rate, for which no basis has been established (Holmes 2002, pp. 172--73; Hull 2002, pp. 329--30).

Still, certain qualitative impressions about the growth of science seem sound and useful, for instance that progress has not been uniform across the natural sciences. Different disciplines reached modernity at different times: astronomy and physics first, with the Scientific Revolution; then chemistry, usually dated to the demise of phlogiston theory; later geology and biology, approximately at the time of Lyell and Darwin. As new scientific specialties continue to emerge, they do not spring fully matured from their parent disciplines, so there remains good reason to distinguish between young sciences and mature sciences. What marks progress in a young science may not be progressive in a mature one.

From the viewpoint of science policy, the level of scientific activity might seem a workable surrogate marker for progress: the greater the level of activity, the more likely that progress will ensue. That generalization must be modified, however, if progress is desired in a specific direction: for example, the vastly increased activity under the "war on cancer" declared three decades ago has not yielded progress commensurate to the investment (Bailar 1995).

Premature science

A discovery is premature, Stent proposed (1972a), when "its implications cannot be connected by a series of simple logical steps to canonical, or generally accepted, knowledge," because others then "did not seem to be able to do much with it or build on it." Stent's definition, the examples he provided, and other candidate cases are subjected to rigorous scrutiny in *Prematurity in Scientific Discovery*, a collection of papers that follows on a 1997 conference at Berkeley. There are anecdotal accounts, case studies, and analytical articles, but there is precious little agreement. In considering these instructive caveats and dissents, and as one after another claimed case of prematurity is deconstructed---"Many turn out to be not premature at all" (Hull 2002, p. 329)---we "should not dismiss the widespread feeling among scientists that some potentially important discoveries are overlooked, or resisted, or accepted only after abnormally long periods of delay" (Holmes 2002, p. 172). Stent brought attention to a phenomenon that scientists recognized as soon as it had been given a name. The lack of conclusive agreement among contributors to *Prematurity in Scientific Discovery* means not that the notion of prematurity is without merit but that it cries out for better definition.

One objection is that labeling anything as historically premature is Whiggish or presentist, and therefore misguided[7]: one should not judge past events, and especially not past actions of individuals, in the light of current knowledge and attitudes, they should be understood in the context of their own times. This objection might indeed apply to the concept of prematurity if the latter were used only with instances of now-accepted science that had earlier been denied or ignored. However, Stent argued that prematurity can be "here-and-now," as with his examples of molecular memory transfer or ESP: "There is no chain of reasonable inferences by means of which our present . . . view of the functional organization of the brain can be reconciled with the possibility of its acquiring, storing and retrieving nervous information by encoding . . . in molecules"; "until it is possible to connect ESP with canonical knowledge of, say, electromagnetic radiation and neurophysiology, no demonstration of its occurrence could be appreciated" (Stent 1972a, pp. 87, 88). Thus Stent offers criteria for judging prematurity contemporaneously[8], not by hindsight or Whiggishly. Hook (2002b, pp. 8--10) has also suggested that objections of Whiggishness could be avoided, without diluting Stent's intent, by talking not of premature "discoveries" but of premature claims, proposals, hypotheses, and so forth. Gerson (2002, p. 281) and Jones (2002, pp. 310--11) point out that speaking of "unconnected" or "disconnected" discovery, instead of premature, also avoids charges of Whiggishness.

The assertion of a premature discovery takes it as equivalent to one made later. However, such equivalence can only be approximate and never completely exact, since a discovery surely takes on some of its significance and import from the state of relevant contemporary knowledge (Holmes 2002, p. 166; Hull 2002, p. 330). At best, there is enough "family resemblance" (Löwy 2002, p. 297) between the earlier and later claims to judge them substantively equivalent[9].

Premature *discovery* must also be distinguished from the mere having of an idea that is somehow ahead of its *Zeitgeist*. An idea ventured without adequate supporting evidence can hardly be called a discovery---it "may be premature to such an extent that its full significance escapes even its own author" (Holmes 2002, p. 168): see, for example, Noddack's suggestion about nuclear fission, below. A discovery is not a "Ding an sich"; at issue is the recognition of it by the relevant community and incorporation into its canonical knowledge. What makes discoveries really significant is "demonstrating them in a way that convinces the scientific and technical establishment" (Townes 2002, p. 57). "The important part of a scientific discovery in almost any aspect of science is the reception it receives" (Zinder 2002, p. 59). One illustration of that is Stigler's (1980) Law---"eponymy is always wrong"; laws and other innovations are named after and therefore credited to, not the people who first intuited them but those who developed them sufficiently that everyone could use them.

One might take the view that *no* discovery could be called premature if it actually happened (Hook 2002b, p. 10; Comfort 2002, p. 193); or, that *every* true discovery could be called premature because it marks a break with what is generally accepted (Zinder 2002, p. 59). These suggestions again treat the notion of prematurity as though it were an exercise in abstract, logical analysis of a proposition rather than the reactions of research communities that are rarely if ever monolithic (Ghiselin 2002, p. 240). Stent's point, implicit but nevertheless surely obvious, is that one or a few people hit on something that most of their peers do not appreciate until much later; the question is, would it have been reasonable for the given community *as a whole* to take up the matter earlier than it actually did? Would it have been reasonable to expect a spate of relevant publication and a largesse of research funds?

Yet, recognizing community involvement raises another problem: what determines whether a particular claim was indeed ignored? What if a few people pay attention but most do not (Ghiselin 2002, p. 239)? What if many pay attention but some authoritative figures are dismissive of it? Instances of both situations are far from unknown. Indeed, it is quite common for a given paper to be submitted for publication to the journal of highest prestige in the field, to be rejected, and then to go *seriatim* to journals of lesser prestige until finally finding publication in some obscure place; does this mark resistance or prematurity or simply the normal course of science?

So the major problem is that Stent's original definition of prematurity is not sufficiently precise. One needs to distinguish prematurity from merely a general sense of "ahead of its time," and to clarify whether it is identically synonymous with "delayed recognition" or "delayed acceptance" or "unrecognized precursor" (Hull 2002, p. 329; Jones 2002, p. 306; Löwy 2002, pp. 295--97; Stent 1972a, p. 86; Stern 2002, pp. 262n5, 271). "Suspended judgment" seems an acceptable paraphrase for Stent's definition: not rejected nor even regarded as unimportant, but so disconnected from accepted knowledge that others do not know how to build on it; yet one writer suggested that Avery's discovery was not premature because it was neither neglected nor delayed, scientists simply suspended judgment (Stern 2002, p. 270n26). Hull (2002, p. 329, 339--40) suggests "promise" as a fruitful concept related to prematurity.

Nowhere discussed is how to differentiate a "premature" experimental observation like Avery's from what Kuhn (1962/70) calls anomalies---data that cannot be explained under the accepted paradigm. Hook (2002b, p. 14) suggests that a premature claim could become accepted as canonical knowledge grows without requiring a Kuhnian paradigm shift; but he fails to enlarge on this or to give needed illustrative examples, seemingly called for since Stent's definition says nothing about awaiting a Kuhnian shift. Some have taken Stent's definition to refer only to cognitive disconnection, suggesting, for instance, that it be broadened to include discoveries that are premature if they are not "capable of being extended experimentally because of technical reasons" (Stern 2002, p. 271), when the issue is one of changing practices rather than minds (Gerson 2002, p. 283).

Gerson (2002, p. 281) and Ghiselin (2002, pp. 239--40) note that "connected to canonical knowledge by a series of simple logical steps" does not specify what constitutes a step, nor how many steps are allowable, nor what canonical knowledge is. They might have asked as well, how much creativity one may demand for the making of a connection-step, bearing in mind Koestler's (1964) useful description of creativity as making connections that had not before been made. I have suggested elsewhere that deviating from normal science in two of the troika marks a "high-risk" venture that most scientists avoid, and that this fits Stent's chief examples of premature discovery (Bauer 1986, pp. 152--53; 2001a, pp. 9--11; 2001b, pp. 96--99): a discovery cannot be readily built upon if the needed "simple logical steps" involve unfamiliar connections in two of the three aspects of science---facts, theories, methods. This seems consonant with Ghiselin's (2002, p. 240) remark that *degrees* of connection to canonical knowledge be considered. Hull (2002, p. 334) suggests placing more emphasis on the second part of Stent's definition, that people do not know how to build on the premature discovery, because it is much easier to recognize whether work is built upon than what its connections are.

Whether or not the notion of premature discovery turns out to be a useful analytical tool, there seems little doubt that it has been and remains heuristic: "Apparent instances of prematurity . . . [can] serve as . . . signposts to subjects deserving closer study" by revisionist historians (Comfort 2002, p. 194); "relative prematurity can teach us something" (Ghiselin 2002, p. 240); it "may help to uncover occasional 'blind spots' of disciplinary practices" (Löwy 2002, p. 303). To repeat: we "should not dismiss the widespread feeling among scientists that some potentially important discoveries are overlooked, or resisted, or accepted only after abnormally long periods of delay" (Holmes 2002, p. 172).

Resistance and neglect

Resistance to scientific discovery may be passive or active: passive, if a claim is ignored, not investigated, set aside; active, if the claim is opposed or pronounced mistaken. Active resistance, as described in Barber's classic article (1961), will here be taken as the canonical definition of "resistance"; passive resistance, neglect, will be taken as equivalent to Stent's (1972a, 1972b, 2002a) concept of prematurity[10].

The connotation of "resistance" is not a positive one. Yet the reliability of science is owing to its demand that the strength of evidence be commensurate with its variation from accepted knowledge: expected results meet no resistance, but extraordinary claims call for extraordinary proof[11]. The resistance that orthodoxy offers against unorthodoxy is useful. Therefore, in considering scientific progress, it is not resistance *per se* that should be seen as hindrance but only *excessive*, *unwarranted* resistance. That, however, may be as imponderable as trying to identify a standard rate of progress.

Pseudo-science, pathological science

Two examples of premature science offered by Stent (1972a, 1972b, 2002a) have rarely been mentioned by those later citing his concept of prematurity: extrasensory perception, and transfer between individuals of memory-carrying substances. Nowadays more commonly termed PSI (for "psychic") and subsumed within the (unorthodox) discipline of parapsychology, extrasensory perception (ESP) is more usually referred to not as premature but as pathological (Langmuir 1953) or as pseudo-science---in other words, no sort of science at all.

But again there are difficulties with definitions. Langmuir's (1953) canonical criteria for how to recognize pathological science simply do not work. Philosophy of science has grappled long and hard, but without success (Laudan 1983)[12], to define a demarcation between pseudo-science and genuine science. In point of fact, any serious examination of really substantive claims that have sometimes been called pseudo-science or pathological science reveals that the terms are most often used as rhetorical epithets without appropriate justification (Bauer 1984a, p. 135--53; 2001a; 2002).

The troika approach leads to viewing pseudo-science as claims that contradict or are disconnected from generally accepted knowledge in all the three chief aspects of theory, method, and data. The phenomena claimed as evidence for ESP or PSI are in dispute---in large part, perhaps, because they are not reproducible on demand under controlled conditions; there are no generally accepted methods that can reliably produce such phenomena; and no theory is available to explain them should they turn out to be real. Parapsychology is disconnected from accepted scientific practices in every major respect. That indicates why the mainstream scientific community regards the field as outside the pale. (That by no means entails that parapsychological claims are unfounded or erroneous: a number of things once labeled pseudo-science were later admitted within the corpus of proper science---among others acupuncture, meteorites, giant squid, ball lightning (Bauer 2001a, pp. 23--24). Recall that such terms as pseudo-science refer not to abstract, logical analysis of propositions but to how scientific communities react.)

As with ESP, so too with the claimed transfer of memory. The experiments were not reproduced, thus the facts were---at best---in dispute. No theory of memory held it to be "written" into macromolecules, and so there existed also no accepted method of molecular memory transfer between individuals. These two examples given by Stent exhibit greater "prematurity" than his other examples; and "pseudo-science" describes better how they are or were greeted, than does "premature."

Scientific revolutions

Science normally expands its knowledge without dramatic discontinuities. As time goes by, however, discordant observations accumulate (Kuhn 1962/70). Ultimately these anomalies become so troubling, accepted theory so unable to accommodate them, that a new way of viewing things becomes imperative and a new paradigm subsumes the old one. According to Kuhn, a scientific revolution is the invention of a new theory required to explain an accumulation of hitherto unexplainable facts.

This surely gives too short shrift to the role of experiments and instruments in the progress of science. Within the scientific community, it has long been taken for granted that the invention of a new instrument or the devising of a new method can have revolutionary consequences. The telescope was fundamental to the progress of Galileo's ideas. Organic chemists experienced infrared spectroscopy and, later, nuclear magnetic resonance spectroscopy, as discontinuous changes in how they worked and what they could aim to do. Adaptations or applications of W.W.II radar equipment transformed astronomy from a purely optical enterprise (Edge and Mulkay 1976). Any discussion of progress in science should therefore take specific note of developments in instrumentation as well as of the accumulation of facts and the modification of theories.

It would then be natural, I suggest, to describe as "scientific revolutions" any famous episodes in the advance of science that seem marked by a discontinuity---no matter whether that discontinuity involved theory, observations, or method. The shift from an Earth-centered view to a Sun-centered one was a theory-shift, but it was based squarely on well attested facts that depended on no new observational approach; so too with Planck's quantum equation; one might call these Kuhnian revolutions. By contrast, radio-astronomy revolutionized instrumentation, but it was not immediately accompanied by any demand that theories of gravity or cosmology be overturned, and the facts---the sources of radio emissions---jibed with the facts of optical astronomy. That compounds of the inert gases could be prepared was a stunning fact and revolutionized synthetic inorganic chemistry, but the theory of chemical combination was not stood on its head, it was simply modified a little, and the methods used for preparation were only modifications of well attested techniques.

Thus a radical change in any one of the troika of theories, methods, facts, may be regarded by the scientific community as revolutionary. It seems natural, then, to describe all of them as scientific revolutions.

EMPLOYING THE DEFINITIONS

The terms premature science, pseudo-science, and scientific revolution describe reactions by a scientific community. Such a community exists only in the context of a mature or "finalized" (Ziman 1994, p. 25) subject that commands a sufficiently comprehensive body of reliable data, methods, and theories on which to base judgments about the acceptability of some new claim. Without a paradigm, and the recognition of certain things as anomalies, there cannot be a Kuhnian revolution. In the era of natural history that preceded modern science, the relevant community was little differentiated from the wider society; it was a loose assemblage, open to interested people without further ado. New observations met little opposition even as they described wondrously strange things; preternatural philosophy, which flourished from the mid-16th to the early 18th centuries, specialized in recognizing and describing irregularities, anomalies, abnormalities (Daston 2000). Thus the concept of "resistance to scientific discovery" makes little sense if applied---as it sometimes has been---to topics whose investigation is in its infancy, or within newly established disciplines or sub-disciplines. (However, resistance may stem not only from opposition within a scientific community but also from outside influences. In that case, resistance can of course be a hindrance to the progress of science no matter how undeveloped and immature the field of inquiry may be---as with, say, a political ban on stem-cell research.)

Similarly with prematurity: concerning a subject about which little is yet known, many ideas and claims will automatically be premature because too little is known to capitalize on them. For example, Tobias (1996; see below) suggests as premature some discoveries of very early hominid fossils. Certainly these were "disconnected from canonical knowledge", but that continues to be more typical than not in this field, where everyone acknowledges enormous gaps in the so-far-discovered evidence. The potential interest of the notion of premature discoveries hinges on finding instances in well established fields. By the same reasoning, the concepts of prematurity and resistance are not applicable to the social sciences since the latter do not feature over-arching agreements or paradigms; for example in political science, "any attempts to label anomalous findings . . . as 'premature' will . . . remain premature" (George Von der Muhll 2002, p. 259).

Purported examples of resistance and of premature science have often been drawn from medical practice. Those examples too are inappropriate: communities of medical practitioners or researchers differ in significant ways from communities doing fundamental research in natural science. For example, restrictions on experimenting with human beings makes proving cause-and-effect inestimably more difficult in medicine than in science. So too with examples drawn from technology: for instance, in much of technology, statistical inference has to be employed rather than direct testing of cause-and-effect under controlled conditions. Purported examples of prematurity *in science* that are drawn from medicine or technology muddy the waters that careful definitions attempt to clarify[13].

Finally, it should go without saying that only potential discoveries or claims of considerable import are worth considering in this context. Normal scientific activity comprises a succession of "discoveries" or advances that flow naturally and inevitably from investigative activity in the given direction. These advances are so obviously ripe for the plucking that no great credit for them accrues. The recognition of resistance to discovery, or of the prematurity of a discovery---and the associated priority disputes---mark occasions that are somehow out of the ordinary, when the point at issue somehow causes the relevant scientific community to take special notice.

Characteristics of Resistance

It is common for people and institutions to resist change, and so it is with change in science[14]. Major revisions of wide-ranging theories are by general agreement called scientific *revolutions*. As already pointed out, startling facts or new instruments or methods can also have revolutionary consequences. Startling facts, if they are not consonant with generally accepted phenomena and cannot be accommodated by current theory, will be strongly resisted; these are the anomalies that, in Kuhn's view, accumulate until eventually they can no longer be denied. On the other hand, unanticipated and therefore startling observations may be quite readily accepted if they are reproducible and can be accommodated theoretically with only minor adjustment; such was the case with compounds of the "inert" gases. New instruments are accepted readily enough if they prove themselves capable of giving information consonant with that obtained by methods already known to be reliable, and therefore not contradicting current theories; the impact of these instruments can be revolutionary without encountering much or any resistance.

Resisting radical change that discards rather than modifies past practice is logical enough since, typically, whatever is being discarded had worked well enough for at least some period of time. The conservatism of the scientific community, its attachment to "traditional patterns of thought", is one factor cited by Barber (1961, p. 597) as responsible for resistance; as examples he cites the Copernican revolution, Thomas Young's wave theory of light, Pasteur's biological explanation of fermentation, Lister's germ theory of disease, Mendel's theory of separate inheritance of hereditary characteristics, and Arrhenius's theory of electrolytic dissociation.

But logical or intellectual reasons are not the only factor in the acceptance of discoveries; resistance to discovery by scientists themselves is "a constant phenomenon with specifiable *cultural* and *social* sources". Barber disclaims discussion of psychological factors, while acknowledging that they must be present and giving a few relevant citations. Nevertheless, they feature in his discourse, as when he notes that those who encounter resistance have often "been excessively embittered and moralistic" about it.

Again, Barber quotes Helmholtz's sympathy for Faraday when he encountered resistance but notes that Helmholtz himself resisted Planck's new ideas about the second law of thermodynamics; surely this is human psychology in play (Barber 1961, pp. 596, 597; emphasis added)[15].

Barber reports Lord Kelvin's lifelong loyalty to the notion that atoms are indivisible and his denial of X-rays, of the production of helium from radium, and of Rutherford's discovery of the electrical constitution of atoms (Barber 1961, p. 598). This illustrates one difficulty in reaching a general conclusion that "resistance" has occurred: the situation is often one where *some* scientists resist but others accept and others again remain neutral or uninterested---for example, concerning Darwin's theory of natural selection (Ghiselin 2002; Ruse 2002).

Methodological considerations are another basis for resistance: confirmed Baconians, empiricists, experimentalists may resist discoveries just because they are theory-centered. Bacon "would have none of Kepler or Copernicus or Gilbert or anyone who would extend a few ideas or calculations into a system of the world". This could equally be seen, though, as just further illustration of a conservative cast of mind. More clearly a methodological source of resistance may be the attachment of scientists to models: Ampère's theory of magnetic currents was resisted as inexplicable by movements of Newtonian atoms, and Kelvin could "never satisfy myself until I can make a mechanical model of a thing" (Barber 1961, p. 598).

As a final example of methodological resistance, Barber cites attitudes toward mathematics: Faraday's discoveries were not accepted until Clerk Maxwell put them into mathematical formulae; contrariwise, Adams's calculations indicating the existence of Neptune were ignored by his anti-mathematical British peers, whereas the mathematics-friendly French readily accepted Leverrier's similar calculations so that the latter gained priority. Resistance to mathematical applications in biology is another reason why Mendel's discoveries were neglected, and why Karl Pearson had difficulty getting published an application of statistics to biology (Barber 1961, pp. 598--99). Here, once more, "resistance" is partial and complex: some mathematical and some non-mathematical claims will be resisted just for either of those reasons; but "the scientific community" in a given discipline may have local or national sub-sets whose approaches differ significantly. Moreover, there are clearly great differences here in degree of resistance: Mendel was neglected for decades whereas it took Pearson just a year to get his paper published.

The religious beliefs of scientists can make them resistant to scientific discovery, as with the Copernican scheme or Leibniz's criticism of Newton for "failing to make providential destiny part of physics"; also much resistance to Darwin's work. Clearly a social factor in resistance is the reluctance of recognized senior authorities to accept contrary views of more junior researchers, as illustrated by the instances of T. H. Huxley, Niels Henrik Abel, Ohm, and, again, the "insignificant provincial" Mendel. Barber cites here also the amusing anecdote that a paper by Lord Rayleigh, from which his name "was either omitted or accidentally detached," was rejected, only to be found acceptable after his authorship became known (Barber 1961, pp. 599, 600).

Disciplinary specialization can be a source of resistance when "outsiders" make a discovery---as with the conservation of energy because the proponent, Helmholtz, was not a member of the community of physicists. Disconnects between disciplines can unquestionably contribute to resistance to new claims, very prominently for instance in the fuss over "cold fusion" (Beaudette 2000) that divided electrochemists and nuclear physicists. Barber also refers to "medical specialists hav[ing] a long history of resisting scientific innovations from what they define as 'the outside'" (Barber 1961, pp. 600, 601); however, as already pointed out (note ***13***), cases from medicine should not be adduced---at least not without argued justification---as instances of "science."

Incompetent staff of journals and scientific organizations represent another possible source of resistance, according to Barber (1961, p. 601). He cites rejection of a paper by Waterston that was rescued from oblivion 45 years later---but he does not show that the original referee was incompetent; if the paper contradicted contemporary views, a competent referee might well have rejected it for that reason. After all, one can hardly include in the definition of competence, an ability to discern when a scientific revolution should occur. Barber discounts the folklore that differing "schools" in a given specialty may be responsible for resisting each others' claims, arguing that some definition of "school" is needed and also that empirical evidence has not been offered to support the folklore; yet most practicing scientists are aware of what can happen when referees are chosen whose views differ substantially from the author's.

Some have claimed that "the older resist the younger." In Barber's view, insofar as this occurs it is not age as such that matters but rather cultural accumulations of preconceived ideas. The infamous Planck's Principle is an extreme suggestion that age matters: "A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it" (Planck 1949, pp. 33--34). Hull et al. (1978) found that not to apply in the case of Darwin's theory. On the other hand, Stephan and Levin (1992, pp. 77--83), considering "whether an older community retards the speed with which new ideas are integrated into scientific theory and practice", found that age can be a relevant factor, bearing in mind that creativity is seen more in younger than in older researchers.

Characteristics of prematurity

I have already argued that prematurity can be a useful concept only in a mature field; that cases should not be adduced from technology or medical practice; and that it refers to passive rather than active resistance---premature claims are not so much decisively disproved or rejected as consigned to limbo. Many of the suggested examples of prematurity rule themselves out on those grounds. As with resistance, in specific instances one is likely to face the dilemma of claims that were not premature in the minds of some but which the invisible college[16] as a whole does not take up---possibly for social or ideological reasons (Tobias 1996; Zuckerman and Lederberg 1986). Again as with resistance, disciplinary considerations are likely to feature: some discovery may leave one invisible college perplexed while another sees how one might build on it (Glen 2002, p. 101n26; Gerson 2002, p. 285; Jones 2002, pp. 313, 324). Such "disciplinary dissonance" (Hook 2002, pp. 124--25) may involve questions of method as well as interpretation (Jones 2002, pp. 313--14) or even subtler, "cultural" dimensions (Bauer 1990a, b).

Stent cited five examples of prematurity: Avery, Mendel, Polanyi, molecular memory, ESP. However, few have followed Stent in treating ESP or molecular memory as examples of prematurity; as noted above, pseudo-science seems a more apt description for them. Authors who cited Stent referred to 22 possible cases of prematurity all but two of which (in chemistry and psychology) were in life sciences (Stern 2002, p. 270). Tobias (1996) proposed eleven cases of which ten were in the life sciences (the other was continental drift). These samples are likely to be biased by the authors' own disciplinary affiliations, however, for candidate cases of prematurity can also be found in physics, for example, claimed detection of gravity waves[17] and of magnetic monopoles[18] (Bauer 2001a, p. 10).

Mendel

The most famous case of premature discovery is that of Gregor Mendel; "Mendel obviously discovered *something* that was little noticed immediately but was seen later as very important". Holmes (2002, pp. 166--169) agrees that Mendel's application of mathematics to biology contributed to the lack of attention his work received; but, he asks, was this a matter of timing (and hence prematurity) or of disciplinary specialization? Moreover, what he discovered is not exactly what he is generally credited with: Mendel's simple mathematical ratios in descendants of hybrids have been re-interpreted by re-discoverers and pundits as the discovery that hereditary characteristics are transmitted in a discrete manner, but Mendel never said that. However, Stent (2002b, pp. 351--52) counters by citing, as conceptually equivalent to genes, Mendel's inference of "formative elements" that remain constant throughout generations.

Mendel's work was disconnected in both (mathematical) method and facts from contemporary work on heredity, and so would be classed as premature also under the troika view.

Avery

Several commentators have questioned whether Avery's work is an example of prematurity (Stern 2002, pp. 269--71). If it is, then Avery is thereby credited with discovering that DNA is the hereditary material, whereas he showed only that DNA was the "bacterial transforming principle" for a single organism (Holmes 2002, p. 171). Zinder (2002, p. 67) agrees that there was a "lack of definitive and corroborative experiments", and moreover Avery himself did not argue for the subsequently recognized chemical and genetic implications of his work.

Holmes (2002, pp. 169--172) also disputes Stent's claim that Avery's finding was not widely discussed: it was, but not in the invisible college of phage geneticists to which Stent then belonged. The problem was more a disciplinary than a temporal one. The now-familiar specialty of molecular biology was still in process of formation, so geneticists and bacteriologists were not accustomed to looking for important clues in each other's work.

Again the troika view would agree with Stent that Avery was premature: there was no theory to accommodate the findings, and the method, while familiar to bacteriologists, was unfamiliar to geneticists. Avery's facts were universally accepted; thus there were disconnects in two of the three aspects.

Polanyi

The long-delayed approval for Michael Polanyi's theory of gas adsorption was one of Stent's original examples of prematurity. In *Prematurity in Scientific Discovery*, however, it is included among "disputable" cases, and the detailed analysis by Mary Jo Nye concludes that it does not exemplify prematurity: "Polanyi's work . . . did connect to contemporary knowledge and it was part of a lively and long-term scientific discussion" (Nye 2002, p. 160).

Four chapters in *Prematurity in Scientific Discovery* are grouped as "Relatively Unproblematic Examples" of prematurity: scurvy; controversies in the earth sciences; expanding-universe theories; Noddack's suggestion of nuclear fission. To the contrary, the majority of these cases turn out to be quite problematic examples.

Scurvy

"By the late 1500s it had been discovered that sour fruits" were effective against scurvy. But a wrong theory was evolved that led the British Navy for several decades to use "treatments" that were not effective; until, finally, a well placed physician with high contacts in the Navy presented data proving the utility of citrus fruits (Carpenter 2002).

Far from being unproblematic, it seems to me, this example does not illustrate lack of connections with canonical knowledge; there were connections, just misguided ones for a time. Further, this is more a case of medical practice than of science, and moreover medical practice was well before its scientific days: cupping and bleeding were standard treatments through much of the 19th century, and it was well into the 20th century before any medical practices could be said to have a scientific basis.

Earth sciences

According to William Glen, three major controversies in earth science illustrate premature ideas as well as resistance to unorthodox ideas and serendipity. But Glen's focus is more on controversy than on prematurity: "the community of scholars, like the individual mind . . . is virtually incapable of holding a suspended judgment" (Glen 2002, p. 104). Suspended judgment, however, seems the very essence of Stent's concept of prematurity. Glen (2002, p. 101n26) also suggests that objections of Whiggishness might be avoided by speaking of "radical" rather than "premature"; yet that would seem to broaden quite out of recognition Stent's definition, to which a period of neglect is central.

Earth sciences -- Global warming

The danger of global warming through the accumulation of man-made carbon dioxide in the atmosphere "was first recognized by Svante Arrhenius in 1896" (Glen 2002, p. 92); but beliefs in uniformitarianism, a homeostatic self-balancing Earth, and naturally occurring cycles all weighed against admitting that humankind could be making the Earth hotter.

Arrhenius might well be credited with an interesting idea, but he hardly seems a genuine precursor to contemporary claims concerning possible global warming. Carbon dioxide is not the only "greenhouse" gas; it is absorbed into the oceans; and moreover its influence may still be less than that of naturally occurring methane. In Arrhenius's time, climatology was a minor, descriptive branch of meteorology, which itself was hardly a mature let alone "finalized" discipline; nothing could be done with that idea---or with any comparable ones.

Earth sciences -- Dinosaur extinction

Evidence that a meteorite impact extinguished the dinosaurs came serendipitously through discovery of anomalous amounts of iridium at the K-T (Cretaceous-Tertiary) boundary. But this is no case of prematurity. The impact hypothesis was anything but neglected: it brought instant controversy. What this case best illustrates is disciplinary dissonance: paleontologists are loath to accept so simple a reason for the extinction, whereas physicists readily do so.

Earth sciences -- Continental drift

Wegener "drew his case for continental drift from several elegant lines of evidence" (Glen 2002, p. 102). This case does seem to fit the spirit of Stent's proposal. It also fits the troika viewpoint: Wegener's hypothesis was disconnected in two out of three ways. While the facts, about complementary coast-lines and relatedness of fauna and flora, were not in dispute, there existed no theory to accommodate them, and to deduce continental movement from such facts was without precedent.

Expanding-universe theories

Norris Hetherington considers whether theories of an expanding universe proposed in the 1920s by Lemaître and by Friedmann were premature. But the theories were not disconnected from canonical knowledge: Einstein accepted that Friedmann had found a possible solution of his field equations (Hetherington 2002, p. 111). In the 1920s, scant data were available for testing cosmological models, and so there was little opportunity for the unorthodox suggestions of universe expansion to confront, let alone overcome, the prevailing conviction of a static universe. This case might better be regarded as an idea ahead of its time, or perhaps as an incipient Kuhnian revolution awaiting the necessary weight of anomalous evidence; it hardly fits Stent's definition of premature discovery.

Noddack's suggestion of nuclear fission

During the 1930s, several groups claimed to have produced transuranic elements. By 1939, it was generally realized that the observations stemmed instead from nuclear fission. Ida Noddack then asserted priority for this realization, based on a letter published in 1934. Hook (2002c) provides an exhaustive analysis of this claim[19]. One possible reason for neglecting Noddack's suggestion was her already low reputation following a claimed major discovery that had not been reproduced. Also, she was personally disliked. The most telling point, however, is that the purpose of Noddack's letter was not to propose fission but to criticize the procedures used to identify transuranics: "one could assume equally well . . . that the nucleus breaks up into several *larger* fragments". Noddack never pressed the suggestion further, nor did she propose or carry out experiments to test it.

Certainly the suggestion was premature from the viewpoint of nuclear physics, for there was neither theory nor factual support for it; but there seems little ground for giving Noddack credit for a *discovery*. As already noted, important for that is to demonstrate something sufficiently that others find it convincing, at least in retrospect. One could doubtless find in the literature innumerable suggestions never followed up by those who made them, a few of which later turned out to have been good ones; Stent's concept would be diluted beyond meaning if all such suggestions were accorded the status of premature discoveries. Noddack's suggestion seems rather akin to Arrhenius's worry about global warming.

Four chapters in *Prematurity in Scientific Discovery* feature "Disputable Cases" of possible prematurity. One of them, Polanyi's adsorption theory, does not exemplify Stent's notion, as already noted above.

Human genetics

Joseph Adams (1756-1818) deduced from careful observation "principles of expression of genetic disease that seem remarkably modern." But he could not go beyond the observations, some of his examples were wrong, and "some ideas . . . do not make much sense today" (Motulsky 2002, p. 202). Adams does not seem a premature discoverer in Stent's sense; rather he may illustrate why Stent's concept does no useful work when applied in an immature discipline and to medical practice.

Archibald Garrod (1857-1936), regarded as the founder of biochemical genetics, especially human biochemical genetics (Motulsky 2002, p. 204), is a more promising case. For one thing, his 1931 book, *The Inborn Factors in Disease*, was re-issued, with added commentary, in 1989. Garrod also recognized and emphasized "the chemical individuality of every human being," and the corollary that individuals will react uniquely to infections and to drugs. But "appropriate methods were not available to follow up his observations," nor were explanations available until the "one gene, one protein concept became the cornerstone of human biochemical genetics" (Motulsky 2002, p. 208). Since that cornerstone has now been eroded, and the unique reactions of individuals to drugs and infections is still not an effective part of medical science, Garrod's view continues to remain premature; but it seems unclear whether it is more a prescient idea than a premature discovery.

R. J. Williams (1893-1988) worked in nutritional biochemistry and, similarly to Garrod, held that "every individual has a unique genetic background with distinctive nutritional needs" (Motulsky 2002, p. 209). But once again this idea remained only an idea. Not only because of the nascent state of the subject, perhaps, but also by reason of disciplinary dissonance: researchers in genetics had no contact with researchers in nutrition.

Löwy (2002, pp. 301--302) adds Richet and Carrel, in the early 20th century, as additional premature discoverers of human biological individuality. Similar caveats as for Adams, Garrod, and Williams also apply to Richet and Carrel, however.

McClintock

According to the "standard story" (Keller 1983), "Barbara McClintock's discovery of movable genetic elements seems to provide a case study in prematurity." Stent's description that others do not know how to build on a premature discovery seems to fit perfectly remarks that McClintock's discovery "fell like a lead balloon" amid "puzzlement, frustration, even hostility" (McGrayne 1993, p. 169); it encountered "stony silence"---"with one or two exceptions, no one understood" (Keller 1983, p. 139).

A critical historical re-analysis comes to a quite different conclusion, however. The evidence for transposition was immediately accepted, but McClintock's *interpretation* was not accepted then *or later*: McClintock was not ignored, and she was not later proved right, in her belief that the transpositions were *the controlling elements in development* (Comfort 2002). Disciplinary dissonance did play a part, however: McClintock's discovery of transposition was not extended beyond maize genetics for some 20 years.

Further potential cases of prematurity include the notion that plastids and mitochondria are descendants of previously free-living bacteria (Maynard Smith and Szathmáry 1999, pp. 59--60) and the conception of species as *individuals* (Ghiselin 2002, pp. 246--47). Moreover, some of what Merton (1973, pp. 357--61) calls "re-discovered multiples" or "forestalled multiples" may be either prescient ideas or premature discoveries. Contributors to *Prematurity in Scientific Discovery* also discussed the following as candidate cases of prematurity:

Darwin

The popular image credits Darwin with the revolutionary impact of the theory of natural selection. But in point of fact, that theory was premature---not only when Darwin conceived it in 1838 but even when he and Wallace published it in 1858. It "never caught fire. People simply had no great use for it". The biologists who adopted and used the theory were "the exception rather than the rule" (Ruse 2002). What Darwin indubitably accomplished was to have the *fact* of evolution generally accepted (Ghiselin 2002).

Natural selection was a premature idea because the fashion was to look (statically) for isomorphisms or homologies between organisms, whereas natural selection contemplates (dynamically) adaptation. Moreover there was no theory of heredity to make specific a mechanism by which selection could work; and furthermore Kelvin's estimate for the age of the Earth seemed too short to accommodate the leisurely process of selection (Ruse 2002, pp. 229, 230)---an instance of disciplinary dissonance. Thus method and theory were disconnected from accepted practice though the facts were clear: using the troika criterion, this was indeed a premature discovery.

Personality also played a role: Darwin was a recluse, not a publicist; and those who, like Huxley, took up the evangelical task on his behalf were actually more interested in hammering home the *fact* of evolution than any mechanism. There were institutional factors as well: biology was being professionalized, and curricula featured aspects like physiology and morphology that were of obvious use, for example in the education of doctors. Only the museum world welcomed natural selection; but studying selection required work with fast-breeding organisms like butterflies, and museums were more focused on displaying remains of dead species (Ruse 2002, pp. 230--231).

Natural selection may illustrate how any major discovery is bound to be premature in a young science. The theory is now seen as central to the *coherence* of biological science, but that could hardly be appreciated before the several specialties had themselves developed sufficiently for their coherence to emerge.

Radio astronomy

Radio waves from outer space were detected by Jansky in 1932 and investigated by Reber in 1940; but disciplinary dissonance between these engineers and the community of astronomers rendered the discoveries premature (Townes 2002). Townes, who later invented masers and lasers through following much the same interest, had himself wanted to pursue the Jansky-Reber lead but was discouraged from doing so, with the result that the pioneers in radio-astronomy were in Britain, Australia, and Holland. It was much the same with radio detection of molecular species in space: "radio waves involved a field with which the people in mainstream astronomy were not familiar". Apparently, a former student of Townes's failed to get tenure because he searched unsuccessfully for years for OH in outer space before detecting it. Theorists were sure, but quite wrong, about which molecules could and which could not be found in outer space.

Townes also ascribes to lacking cross-disciplinary connections that he was the first to make a maser (the microwave precursor of the laser). The theory was known to physicists, but many did not appreciate that it entailed *coherence* of the emitted radiation. Moreover, they did not know how to make the instruments. Townes's chapter is rich in fascinating anecdotes. He cites precursors, and notes that people with similar backgrounds often had similar ideas.

This account is certainly instructive about the progress of science, but perhaps more about the difficulties of getting support for innovative projects than about premature discoveries.

Bacterial variability

Observations of variability in bacteria were dismissed until it had become accepted that bacteria form true, stable species (Löwy 2002, p. 301). In my view, this illustrates the situation of a young specialty, particularly where reproducibility is problematic, rather than exemplifying premature discovery.

Teratology

From the 18th century on, zoologists were studying drug-induced fetal malformations. Medicine remained oblivious to this into the 1950s, and the first paper describing teratogenic effects of thalidomide was rejected by *Lancet* in 1961 (Löwy 2002, p. 301). This seems a clear case of disciplinary dissonance, with the additional caveat that prematurity in medicine, by contrast to science, raises additional questions.

Mosquitoes and yellow fever

That yellow fever is spread by mosquitoes was only accepted two decades after Carlos Finlay suggested the connection on epidemiological grounds (Löwy 2002, pp. 302--303). Once again, we are dealing with medicine, where demonstrating cause-and-effect is hindered by prohibitions against experimenting on humans. Löwy points out, however, that the correlations found by Finlay could have been tested experimentally, albeit a little indirectly, by programs to eliminate mosquitoes.

Phillip Tobias (1996) lists a possible eleven instances of premature discovery, two of them being Stent's canonical exemplars (Mendel and Avery). A third is continental drift, and Tobias adds to the name of Wegener (1912) as discoverer the names of F. B. Taylor (1910), A. Snider (1858 [!]), and A. L. DuToit (1921). But two further cases seem mere ideas rather than discoveries: Benjamin Franklin's (1751) anticipation of Malthus and therefore Darwin; and John Frere's (1797) recognition of Stone-Age implements. Chagas, early in the 20th century and "half a century ahead of his time," recognized the cause of American trypanosomiasis; for reasons already given, however, cases of prematurity in medicine should not be conflated with prematurity in science (see note ***13***). That caveat applies also to Tobias's citing of Fleming's 1929 discovery of penicillin: it came into medical use only with W.W.II, when the social demand was great enough. Curiously, Tobias regards as a possible case of prematurity, the 1953 revelation of the structure of DNA, because only much further work led to its full appreciation and confirmation in all details; but this discovery was anything but ignored or neglected, which are *sine qua non* characteristics of prematurity.

Another two of Tobias's cases are the recognition of the Taung skull as being hominid and of *Homo habilis* as a new hominid species. But these claims were "strongly opposed" and "repudiated", not neglected, and therefore qualify as illustrations of resistance, not of prematurity. Moreover human paleoanthropology could hardly be called a mature discipline even now, given the great gaps in evidentiary fossils and the lack of discipline-wide consensus over relationships among the available evidence.

Tobias's final case is his announcement, at a conference in 1973, that skulls of *Homo habilis* show impressions of motor speech cortices. Over the years, this claim has slowly gained support; by the early 1990s, half-a-dozen other people agreed that *H. habilis*---who lived 2 million years ago---likely had spoken language. That does seem to mark it as a premature discovery---but with strong caveats because of the generally undeveloped state of human paleoanthropology and the study of language origin.

Magnetic monopole and gravity waves

These, I suggest, fully satisfy the concept of premature discovery. In each case, theory accommodates the claim---these entities are believed to exist or to possibly exist; but only one observation of each has been accomplished, and there are no attested or reliable methods for reproducing the observations[17,18]. For decades, physicists have neither strongly believed not strongly disbelieved the observations. They simply don't know what to do about them, until more sophisticated instruments can be brought to bear.

Brownian motion and photoelectric effect

Jones (2002, p. 322) cites as examples of some sort of prematurity, Hacking's (1983, p. 158) description of these as "meaningless phenomena" because they could not be integrated into the body of accepted knowledge before the existence of atoms and molecules (Brownian motion) and of energy quanta (photoelectric effect) had become established. One might equally view these as Kuhnian anomalies, since their explanation required a change in the governing theoretical paradigm.

SOCIAL AND INSTITUTIONAL FACTORS

Barber discussed "social factors" in resistance to some scientific discoveries, but he focused on those within the scientific community. Discussions of premature discovery have generally sought intellectual reasons for prematurity; but one factor quite frequently seems to be disciplinary dissonance and therefore social interactions in the same sense as used by Barber, internal to the scientific community. Since the disciplines also function as cultures (Bauer 1990a, b), social influences are not readily disentangled from intellectual ones.

Tobias (1996) suggested that political factors during the apartheid period in South Africa played a part, though he talked of it as prematurity rather than resistance. Social influences from outside the scientific community can unquestionably hinder progress in science, notoriously so in Nazi Germany and in the Soviet Union and its satellites during the 20th century. But such open societies as the United States, too, can hinder or facilitate research, directly by legislation or more indirectly through control of funding: *vide* the example of stem-cell research.

Modern science developed in those parts of Europe where the social climate was most conducive, putatively the least authoritarian societies, those most open to intellectual innovation and to social changes stimulated by the activities of independent entrepreneurs (Marks 1983). The traditional and still common view of the scientific community sees it as largely autonomous, delivering useful knowledge to the wider society and, in return, being given material support and a high degree of independence. Those circumstances have begun to change, markedly and in all likelihood irreversibly. The prescient recognition by Derek Price that science was in transition from a state of roughly exponential growth to one of maturity and a continuing decline of growth is recalled in *Striking the Mother Lode in Science*, which examines corollaries of that change, especially implications of the changing age structure of scientific communities. John Ziman, in *Prometheus Bound*, looks in detail at the enormous ramifications of the end of growth at a time when scientific progress "continually raises the level of resources required for further research"; resources, moreover, that can become obsolete at so rapid a pace that there is the temptation to "carry on" with programs whose *raison d'être* is gone (Ziman 1994, pp. 49--51, 53).

Ziman summarizes the transformation of scientific activity by pointing to changes in its norms: the traditional Mertonian ones that Ziman terms CUDOS have been supplanted by others that Ziman terms PLACE; academics no longer "publish or perish," they "apply or die" (Ziman 1994, p. 97). The self-motivated and independent scientist's ideals were **C**ommunalism, **U**niversalism, **D**isinterestedness, **O**riginality, and **S**kepticism; the work of a contemporary research scientist employed by an institution is, by contrast, **P**roprietary, **L**ocal, **A**uthoritarian, **C**ommissioned, and **E**xpert. "Knowledge-creation, the acme of individual enterprise, is being collectivized"; that "affects the whole research system, from the everyday details of laboratory life to the politics of national budgets." Yet "science cannot thrive without social space for personal initiative and creativity, time for ideas to grow to maturity, openness to debate and criticism, hospitality towards innovation, and respect for specialized expertise." Ziman's concern is "how to reshape the research system to fit a new environment without losing the features that have made it so productive in the past" (Ziman 1994, pp. vii--viii). For instance, as scientific information increasingly becomes a commercially valuable commodity, how to maintain the openness and transparency of "public assessments of research claims, which are the ultimate arbiter of scientific validity"? (Ziman 1994, pp. 38--41).

Under CUDOS, scientists believed themselves beholden primarily or even only[20] to an international community that pooled its results about universal laws of nature; conflicts of interest were minor, since personal rewards were presumed to accrue automatically to those who best advanced human understanding. How very different is the situation under PLACE[21] for the increasing proportion of researchers who work in industry, in government labs, or in academe while dependent on funding from government or industry. Conflicts of interest become unavoidable when researchers are expected to be directly and immediately accountable to patron or employer, and to the wider society, and also to colleagues, when many still feel primary allegiance to the universality of science. Under PLACE, a whole set of unresolved issues has arisen, for example as to whistle-blowing and the role of professional societies in developing guidelines for ethical conduct. Ziman mentions the increase in teamwork and multiple authorship of publications in the era of PLACE, but leaves for others or for another occasion (Ziman 1994, p. 99) discussion of the ensuing ethical dilemmas, some of which have been recently prominent: To what extent is each author of a publication responsible for the honesty of what co-authors have contributed? How to balance the need to trust one's colleagues with the need to be accountable to a wider audience? Some of these points are mentioned by Stephan and Levin (1992, p. 163) as corollaries of increased competition for research funds.

PLACE and CUDOS are attitudes internalized by members of the scientific community: "the question of who actually does the piping or pays the pipers may not be so significant as the fact that they are all playing the same repertoire of tunes". Scientists' thoughts and plans incorporate instinctively the motives of the patrons. "The civilian science that was enlisted" in war-time "was demobbed as a veteran, hardened and rather coarsened" (Ziman 1994, pp. 76, 77).

The centerpiece of *Striking the Mother Lode in Science* is an econometric study of scientific productivity as a function of age and of cohort, but the book's discussions cover much broader ground. The empirical work supports the conventional wisdom about youngsters being responsible for the greatest advances, though the correlation is considerably less than the shibboleth would have it. Throughout, the book emphasizes the advantages accruing from being in the right place at the right time (RPRT): the training one receives, the opportunities to make worthwhile discoveries, the likelihood of remaining relevant rather than becoming obsolete. There are (good and bad) vintages of scientists as there are of wines, for environmental and not internally inherent reasons. The book's chief conclusions are (Stephan and Levin 1992, p. 9):

> 1. Exceptional contributions . . . are most likely to be made by scientists under . . . 40.
> 2. Age matters, but not nearly as much, for "average" Ph.D. scientists.
> 3. RPRT clearly matters, but . . . is significantly more difficult to study.
> 4. In the past twenty-five years, the average quality of Americans choosing careers in science has declined.
> 5. Job market . . . and quality . . . have conspired to make those who entered science in the late 1960s, 1970s, and 1980s less productive than their counterparts who became scientists at an earlier time.
> 6. Increased competition in U.S. science is acting to stymie creativity and productivity.

Few of those are likely to seem controversial to older scientists, though younger ones might take umbrage with the fourth and fifth points. Nevertheless, Stephan and Levin provide plenty of evidential underpinnings for their argument, whose value lies largely in the manner in which it makes logical connections among such things as the vagaries of federal funding and the changing age structure and productivity of the scientific community. Few economists have directed their attention so sharply on science and science policy, and the insights in this book are a major contribution.

PROGRESS IN SCIENCE, IN A NUTSHELL

There are preconditions for scientific progress, and there are contingencies that facilitate or hinder it. One very general precondition is curiosity. A more specific one is the state of knowledge about a particular subject. Among contingencies are the degree of interest in the subject and the particular people who display that interest and act on it. *Prematurity in Scientific Discovery* makes plain that little-developed fields provide fertile ground for discoveries that later appear to have been premature, because the field was not ready to build upon them; in mature fields, perhaps the salient cause of premature discovery is disciplinary dissonance: the discovery required some merging of knowledge from otherwise distinct specialties. Such discoveries are particularly noteworthy because so few people are in the right place, at the right time, with the right combination of qualities and background knowledge, to put everything together. As Merton (1973, p. 366) puts it, such individuals do what otherwise would require the combined effort of several people.

Discoveries are actively resisted when they are unwelcome for intellectual reasons (contrary to canonical knowledge) or for social or cultural reasons; and in extreme cases they may be labeled pseudo-scientific or pathological science. Discoveries that seem unconnected to canonical knowledge are passively resisted in that they are largely ignored, and---if valid---may later be re-discovered and labeled premature.

Prematurity and resistance are *reactions by and within the scientific community*. They signal clues to understanding how the scientific community works. Dismissing as Whiggish the concept of prematurity misses this point, treating prematurity as though it were an abstraction. Or, it stands on its head the meaning of Whiggishness: The plain fact is that scientists, in the context of the time of the original claim, did not do anything much with or about it. The fact is also plain that scientists, in the context of their own later time, see it as significantly prescient. What could be more Whiggish than to substitute a pundit's later view for the contemporaneous views of the actors?

A related unwillingness to grant validity to the perceptions of scientists is the Marxist insistence that science is brought about by material circumstances, not by the individuals who scientists regard as notable discoverers: "breakthroughs are not necessarily a product of individual genius but are rather simmering in the scientific consciousness at any given time. . . . the process of discovery is independent of any inquiring mind, for scientific development has its own autonomy" (Lamb and Easton 1984, p. 173)[22]. Under this approach, every discovery is re-interpreted as having been at least potentially made by a number of people, some of whom just happened to be held back, diverted, or ignored[23]. But the upshot of this argument is to make it perhaps even more remarkable that scientific communities agree to give large credit to people like Darwin and Einstein. Of course the state of the art in a given field is a precondition to progress and discovery; but the everyday experience of scientists is that, at any given time, only one or perhaps a few of them see connections and possibilities that others do not until they are pointed out to them. Once again: we "should not dismiss the widespread feeling among scientists that some potentially important discoveries are overlooked, or resisted, or accepted only after abnormally long periods of delay" (Holmes 2002, p. 172). Such widespread feelings should signal to the student of science that there is something worth looking into.

THE PROGRESS OF STS

Far less widely discussed than Kuhn's approach to scientific revolutions have been the approximately contemporaneous articles by Barber (1961) on "resistance by scientists to scientific discovery" and by Stent (1972a, b) on prematurity[24] of some discoveries, though their import is similar. That is to say, they have been far less widely discussed in the academic literature of science studies[25]. Stent's concept of premature discoveries was cited 68 times[26] (in periodicals indexed by Science Citation Index and Social Sciences Citation Index) in the 25 years after it was published; only 25% of those citations were in the STS literature, and almost twice as many were in life-sciences and medical-sciences literature. Possible cases of prematurity identified by citation analysis were not followed up (Stern 2002). Hull (2002, p. 334) points out that the notion of prematurity was premature because there existed no canon of knowledge about the nature of science to which the concept might be connected. (By Stigler's (1980) Law, Stent would not be the first to note the phenomenon of prematurity; and indeed it seems to have been anticipated by Taton in 1957 (Hook 2002b)). Barber's description of resistance to discovery encountered the same circumstances as Stent's discussion of prematurity, being cited with similar frequency, 70 times in the 20 years after it was published, and also twice as often in scientific periodicals as in science-studies literature (Bauer 1980).

A rate of several citations per year[27], over periods of decades, does reflect more attention and greater staying power than the vast majority of scholarly or scientific articles (Stern 2002, pp. 262--63); clearly, the notions of resistance and of prematurity resonate strongly, especially with scientists themselves (Hook 2002b, p. 9). Yet no systematic, comprehensive, critical assessment of either concept was attempted before the conference organized in 1997 that foreshadowed publication of *Prematurity in Scientific Discovery*.

Evidently, what seems to scientists to be important about how science works has not always seemed salient to STS scholars. Scientists' insights have not had a controlling or even strong influence on the topics that STS deals with. Some further indications of that are:

- Kuhn's notion of scientific revolutions was accepted without further ado by most scientists whereas it was deconstructed and tested almost to destruction in STS scholarship.
- John Ziman's writings are generally regarded by scientists as the most authentic, comprehensive, integrated discussions of scientific activity; yet these works are not regarded as canonical throughout the institutions of STS. For example, Ziman is missing from the list of "Selected monographs in the history of science, philosophy of science, sociology of science, 1972-1999" whose citations were surveyed by Stern (2002, p. 264, table 18.1)[28].
- The belated recognition by STS scholars[29], in the 1980s, of the central importance to scientific progress of instruments and experimentation, something that had been intuitively obvious to scientists for decades (at least)[30]. Even in 1992, it was thought worth pointing out that "change in science is not limited to theory" (Stephan and Levin 1992, p. 100)[31]. This neglect of the significance of instruments seems particularly inexplicable given that awards of Nobel Prizes so clearly demonstrate the significance: Nobel Prizes in physics and in chemistry have been awarded three or four times as often for experimental novelties as for theoretical ones[32].

STS sprang from at least two sets of roots (Bauer 1996, pp. 49--50; Donovan et al. 1992, p. xi). One ("science-based") can be traced to the end of W.W.II and the post-war concern among scientists, physicists in particular, to have some influence on national policy regarding, primarily, nuclear power and nuclear weapons. Courses and programs and academic units were established, typically by scientists and engineers and political scientists, usually under the rubric of "Science and Society." Another source of STS ("humanities-and-social-science-based") was the increasing interaction among history of science, philosophy of science, and sociology of science that became noticeable in the 1970s. As illustrated in the preceding paragraphs, the policy-based and academe-based threads in STS have not coalesced effectively. One cannot speak of a body of canonical knowledge in STS or of an over-arching paradigm (Donovan et al. 1992, p. xiii). Perhaps this is only to be expected, since STS draws on humanities and social science (Gerson 2002, p. 289) which themselves do not work by way of such over-arching paradigms. Be that as it may, the implications for science policy are truly unfortunate. Governments have no obvious experts to consult for advice, so *faute de mieux* they call on senior people from the scientific community, whose understanding may be meager indeed of how to stimulate desired activity in science, technology, and medicine. We are not so far beyond the "two cultures" problem described by C. P. Snow (1963).

The meager attention paid within STS to concepts of resistance and prematurity can thus be ascribed to disciplinary dissonance between the science-based and the humanities-and-social-science-based strands within STS, whereby the concept of prematurity itself becomes premature (Ghiselin 2002, p. 248). One problem is that scientists can hardly become actively engaged in STS while remaining fully active in their scientific research: among the twenty-two contributors to *Prematurity in Scientific Discovery*, eight are doctors or scientists, but six of those eight are emeriti. Scientists may also be disinclined to participate because hegemony over STS scholarship has come to be exercised, over the last several decades, by the "sociology of scientific knowledge" and the "strong programme" of the Edinburgh school of science studies (Hull 2002, p. 338), which most scientists find unpalatable[33]. By contrast, the earlier, Mertonian, sociology of science had seemed congenial to scientists (Bauer 1996, 1997); correspondingly, scholars in the Mertonian tradition have shown continuing interest in concepts of resistance and prematurity and the closely related ones of priority disputes and simultaneous discoveries (Holmes 2002, p. 164; Stern 2002, pp. 272--73).

Among academic disciplines, science has for decades found it easiest to gain respect and support, because of society's conviction that it lays golden eggs. The humanities and social sciences also gained support, but by trickle-down and spin-off. Now that support for science has become increasingly tied to immediate societal benefits, the humanities and social sciences will need to make their own cases that they have benefits to offer. STS purports to explain how science works and how it can be guided; if it cannot deliver those goods, its support may soon run dry. Science has been transformed during the 20th century, as described in *Prometheus Bound*, because it had to become more directly and immediately accountable to the wider society. STS should consciously follow the same path, for much the same reasons.

Science policy might plausibly be viewed as an application of what is known about politics, science, technology, economics, and so on. One might then expect that it could best be guided by scholars in those fields. "Unfortunately, the 'metascientists' . . . have not yet come up with a coherent account of just how the research process actually works"; moreover critiques from the viewpoint of constructivism, relativism and the like, no matter how intellectually instructive they may be, "do not offer direct guidance to the person faced continually with practical decisions, small and large, how to keep the system going" (Ziman 1994, p. 275). Since one of the aims of STS graduate programs is to prepare graduates for work in science policy, this recognition surely ought to inform the curriculum and tone of STS programs.

TOWARD EFFECTIVE SCIENCE POLICY

Serendipity

The importance of serendipity, because of our inability to harness creativity deliberately, should never be forgotten---see, for example, "Foreseeing the unpredictable" (Ziman 1994, pp. 106--111). Even multi-disciplinary attacks on well-defined problems, and multi-disciplinary centers of excellence established for particular purposes, ought to bear in mind "the diversity of expertise around the common room coffee table, where unplanned, informal contacts between specialists in widely different subjects are often so fruitful" (Ziman 1994, p. 66).

Given that understanding, one can envisage a variety of tactics, for example sabbatical or mini-sabbatical leaves to be spent in very different environments; and conferences at which people from a wider variety of backgrounds are invited to kibitz the multi-disciplinary project or program of the center. That might have been useful as physicists and meteorologists were experimenting with rain-making, for instance: had they known what cell biologists knew, they might have found better materials to serve as rain-seeds (Franks 1981).

Disciplinary dissonance

Perhaps the most common feature of purportedly premature discoveries is that what one relevant discipline understands, another does not. A related point is that "solutions to problems having 'no socially and cognitively defined disciplinary home' are . . . especially likely to be postmature" (Zuckerman and Lederberg 1986).

A possible way of decreasing barriers to progress stemming from this source would be the organizing of interdisciplinary conferences with very broad agendas, the chief expected benefit being not so much concerning the specific topics under discussion as the bringing together of people from very different backgrounds in an atmosphere of intellectual ferment free from the conflicts of interest---guarding of turf, defending status, etc.---that beset professional academic meetings. A good model for an appropriate time schedule and physical environment might be the Gordon Conferences[34], and a good model for the desired eclectic agenda and diversity of invited participants might be the International Conferences on the Unity of the Sciences[35].

Löwy (2002, p. 303) gives examples where the passing of information across disciplinary boundaries might even have "saved many lives and eliminated much suffering."

Resistance and neglect

Many---if not most or all---revolutionary breakthroughs have been opposed or resisted for a time; the history of science teaches that one-time unorthodoxies have contributed mightily to the progress of scientific understanding. Yet the conservatism of science is largely responsible for the reliability of science, so one should not accede to such sometimes-offered suggestions as that peer review not be a condition of publication. However, given the definite---albeit small---probability that unorthodox views will eventually win out, it would seem an excellent idea to hedge society's bets when funding research: some small percentage---say, 5%?---should be allocated to competent researchers in relevant fields who take views dismissed by the majority. Nowadays that would support work, for example, by astronomers who hold that astronomical red-shifts do not uniformly arise from motion of their source and are sometimes quantized (Arp 1998) and by biologists who have cited evidence that HIV is not the sole and only cause of AIDS (Duesberg 1996; Root-Bernstein 1993)[36]. But no specific examples are really needed to establish the desirability of this principle.

The end of growth

Stephan and Levin (1992) and Ziman (1994) argue convincingly that the creative vitality science has shown in the recent past will be severely impaired by the increasing competition for funds and a lack of opportunities for new recruits into research communities. One way of ameliorating that would be to encourage senior researchers to make career changes, perhaps into administration, perhaps into consulting, perhaps into teaching in undergraduate colleges or in high-schools; something that an appreciable proportion of them might welcome, if they could make the change without loss of benefits or standard of living, and if only they could be persuaded out of the fixed mind-set that is so common among researchers (Ziman 1987).

Funding research

In these times when governments appear agreed that free economic markets are the best ways of allocating resources even in health care, it is well to remember that "It would be absurd to suppose . . . that a market dealing in such intangible commodities as 'research' or 'education' could ever approximate to 'perfection' in the classical economists' sense" (Ziman 1994, p. 137). One popular way of enlisting free enterprise in research has been though direct and immediate cooperation between academe and industry. Again Ziman has an instructive caution: "A shotgun marriage between such different cultures may produce offspring that are much less intellectually or technologically fertile than either of their parents" (Ziman 1994, p. 266).

Stephan and Levin (1992, p. 165) demonstrate cogently the deleterious effects on science of "stop-and-go" funding. The "go" lessens quality, the "stop" discourages potential recruits, and both impacts last far beyond the time at which the funding decisions were made.

Necessary distinctions

Distinctions between science, technology, and medicine are often neglected but may be important, as noted earlier. Making such distinctions can be illuminating even when they become blurry. Thus it is instructive about the change from individual to collectivized science that in "a supposedly finalized science, such as fluid dynamics, or plant genetics---or even economics---it becomes almost impossible to make a sharp distinction between 'basic' and 'applied' research in terms of what is actually discovered"; and "the convergence of basic and applied research at the level of the laboratory bench has a profound effect on the way that science is organized" (Ziman 1994, pp. 25, 26).

Mathematics differs in many ways from natural science, yet it is often lumped together with them. Young specialties differ in many ways from well-established, mature fields. Each scientific discipline and sub-discipline has its own culture (Bauer 1990a, b) even as its knowledge base connects and overlaps with other specialties. The relative reliance on theory and experiment differs from field to field and over time within any given field (Bauer 1992, ch. 2). What counts as progress is not the same at different times, nor in different specialties. The rates at which different specialties advance are not the same even when they interact closely with one another; publication rates and styles vary wildly among disciplines; altogether, assessing the merit of research performance is anything but straightforward (Ziman 1994, pp. 37--38, 102--106).

Science policy would also do well to distinguish between goals that are technically feasible with existing knowledge and those whose success depends on discoveries yet to be made. As Ziman (1994, p. 30) points out, society presently over-values the power of science and technology to the extent that "apparently responsible people can now be persuaded that targeted R&D can blast its way to any technological goal, however implausible this may seem to the great majority of the relevant experts."

NOTES

[1] Some might prefer the more neutral term "change," but the books under review---and this writer---take for granted that there has been genuine progress in science.

[2] In the remainder of this article, to avoid continual repetition of "STS, including history of science, philosophy of science, sociology of science," I shall implicitly include those when using the terms "science studies" or "STS"; for example, "STS scholars" would include philosophers of science even if they do not or have not participated in the interdisciplinary discourse that science studies aims for. Again, "the STS literature" includes the literatures of philosophy of science, history of science, and sociology of science. Where I refer specifically to, say, philosophy of science, it will be to emphasize the philosophical approach by contrast to others.

[3] I am not suggesting that the three aspects are quite independent of one another. I do suggest that the commonly cited notion of the "theory-ladenness of facts" be modified to recognize the significant role that instruments and methods play. Facts are tied more closely to method than to theory: they are primarily *method*-laden (Bauer 2001b: 99). Methods, of course, are themselves theory-laden, so facts remain theory-laden---but at one important remove. (Theories are---or should be!---heavily fact-laden; but they are also method-laden.)

Löwy (2002, p. 300) suggests four rather than three types of steps for making Stent's definition operational: knowledge taken for granted; questions viewed as legitimate; accepted methods; ways of evaluating evidence.

[4] Unpredictable things are bound to happen. The science and technology of the next millennium would likely be incomprehensible from the standpoint of our current state of understanding.

[5] Another insightful work about science from someone better known as an economist is Tullock (1966).

[6] For example, the Nobel Prize for the discovery of pulsars (Bell Burnell 1977). When a discovery is made by picking signals out of background noise, as is increasingly the case in some fields of astronomy and physics, how much credit should go to the person who wrote the computer software? How much to the person who scans the computer output? How much to the person who had the idea for the search and then let others do the necessary work?

[7] Indeed, some invitations to participate in the conference were declined on those grounds (Hook 2002b, p. 9).

[8] Hull (2002, p. 333) says that "instances of here-and-now prematurity are commonplace and not very interesting" because so many scientific papers are never cited by anyone, but this suggestion is convincingly countered by Munévar (2002, p. 342).

[9] The same point arises in connection with "multiple" discoveries (the "same" thing discovered by several people) and the ensuing priority disputes (Lamb and Easton 1984, p. 207n31).

[10] Jones (2002, ch. 21) considers whether the concept of prematurity should include cases where a new claim *contradicts* canonical knowledge and therefore is not disconnected from it. His conclusion differs somewhat from the stance taken in this essay. In my view, if something is seen to contradict, by the same token its validity can be tested; Stent, however, pointed at cases where it was not clear, what to do with a claim, how to "build on it".

[11] Often attributed to Carl Sagan, this *bon mot* actually originated with Marcello Truzzi.

The Bayesian approach to statistics takes the same view: great weight of evidence is needed to make significantly more probable, something that seems inherently (*a priori*) very improbable.

[12] Dissent from or quibbling with the details of Laudan's (1983) summation carries no weight in absence of a generally accepted set of demarcation criteria. No such set now exists.

[13] Not that sharp, clean boundaries can be drawn between science, technology, and medicine. But for clear-cut cases, well away from the fuzzy and overlapping boundaries, certain stereotypical characteristics have considerable explanatory power.

In medicine, for example, the phenomenon of "hidden events" (Westrum 1982) is instructive. Doctors overlooked or misinterpreted evidence of child-battering for a long time. Perhaps part of the explanation is that doctors wish to heal, which means they seek to categorize complaints within what they know, and they recognize something as new only as a last resort. By contrast, it is the job and passion of scientists to notice new or unusual things.

Again, Oliver Sacks gives several examples of medical syndromes well described in the older literature that were then neglected for decades, only to reappear again later. He suggests that the 20thcentury demand for explanations, and denigration of mere descriptive reports, may be responsible. One such phenomenon is that of "alien limbs"; "why is it so difficult . . . to give the syndrome its due place in our neurological knowledge . . . ?," asks Sacks (2002, pp. 74--76). In considerable part, I suggest, because this is not straightforward natural science. Alien limbs are a psychosomatic phenomenon, and medical science does not yet comprehend the mind-body relation. A striking illustration of that is medicine's ambivalence about the placebo response, an indubitably real and important phenomenon whose investigation has long been neglected (Brody 2000; Harrington 1997; Shapiro and Shapiro 1997).

In science, the lack of an explanation is a common basis for resisting a claim, for example Wegener's notion of drifting continents. By contrast, in medicine and also in technology, things are accepted if they work, even if no explanation is available for why they work (Hook 2002b, pp. 15--17). In technology, it is (a lack of) commercial demand that determines prematurity (Löwy 2002, p. 301n18); many well known examples of technology were put into use only decades after their invention, say, video-phones.

[14] Albeit this was not much recognized before Barber and Kuhn. Löwy (2002, pp. 297--98) credits Kuhn with bringing to the fore the conservatism of the scientific community at a time when science was regarded as "a permanent search for new knowledge."

The popular image of science, and the self-image still common among scientists, hold science to be endlessly eager to discover new things. The rub is that eagerness is not in evidence if the novelty is *too* new, in other words if it fails to fit seamlessly with generally accepted, pre-existing knowledge: "new ideas need the more time for gaining general assent the more really original they are" (Helmholtz, cited by Barber 1961, p. 596).

[15] Again, Barber (1961, p. 598) describes as socially based resistance the fact that experimenters, who observed that rabbits' ears become floppy after injection of papain, recognized only long afterwards that this showed cartilage to be a reactive, not an inert substance. But one might more naturally see this as the psychological difficulty of seeing things in a new way.

[16] "Invisible college" is Derek DeSolla Price's term for the international community of researchers in a given specialty.

[17] Periodically updated information is at www.astro.gla.ac.uk/users/hewitson/ downloads/detectors.pdf (accessed 25 January 2003).

[18] Periodically updated information is at http://pdg.lbl.gov/2002/s028.pdf, revised December 1997 (accessed 25 January 2003).

[19] He also mentions an interesting sidelight on the process of discovery in science. Theory "was more of an obstacle than a help for the discovery of fission." Quantum mechanics had yielded excellent results for radioactive decay by treating it as a tunneling process. Calculations showed that the probability was negligible that fission could ensue; but, as it turns out, fission should not be treated as a tunneling process (Hook 2002c, p. 135n33).

[20] The degree to which scientists once felt loyalty to "science," embodied in an international invisible college, rather than to employers or even to their own country has been delineated in fictional but authentic terms in Balchin (1949) and in Hilton (1947). Hints to that effect are found at various points in such histories as those of the Manhattan Project and the hearings as to the security risk possibly posed by J. Robert Oppenheimer.

[21] The CUDOS ethos is described in such works as Sinclair Lewis's *Arrowsmith* (1925), which some older scientists continue to recommend to their graduate students as appropriate introduction to what doing science is about. (I learned of such instances while giving seminars about ethics in science during the early 1990s in a variety of science departments.) The modern fashion under the PLACE ethos is rather to emphasize the competitive and self-serving side of the contemporary research scene in what I have called scientific docu-novels (Bauer 1992, pp. 84, 166), a literary genre that dates perhaps from James D. Watson's (1968) autobiographical faction.

[22] "Simmering in the scientific consciousness" reifies the scientific consciousness from a metaphor to an active agent.

[23] Merton (1973, pp. 343--70), on whose work Lamb and Easton draw extensively, does not play down the role of individuals in the progress of science even while pointing out how frequently many are thinking along similar lines.

[24] Also mentioned by several contributors to *Prematurity in Scientific Discovery* is the notion of "postmature" discoveries that could have been made earlier than they actually were (Zuckerman and Lederberg 1986). This notion has not drawn the attention that prematurity and resistance have, and is not considered further in this essay. It raises somewhat different questions, and calls for more care in avoiding Whiggish judgments. From the troika viewpoint, a postmature discovery is connected to generally accepted knowledge in every way, it just happened not to be made.

[25] Jones (2002, p. 314) notes the absence of "premature" from the vocabulary of philosophy of science. According to Hull (2002, p. 340), Stent received numerous letters but his idea was ignored "by nearly all students of science."

[26] Stent's articles were cited 76 times but 8 of those referred to Stent's discussion of uniqueness in science and art, not to the prematurity concept (Stern 2002, p. 262).

[27] Presumably there were more citations in places not indexed by the Citation Indexes.

[28] The list included works by Barnes, Bloor, Feyerabend, Fleck, Hanson, Holton, Kuhn, Lakatos, Laudan, Merton, Nagel, Polanyi, Popper, Toulmin. (Unfortunately, Stern does not indicate whether these works cited Barber or Stent.)

[29] Hacking (1983, p. 150) "experimentation has a life of its own"; Franklin (1986): *The Neglect of Experiment*.

[30] Half a century ago (in 1951), candidates for the degree of Bachelor of Science with Honors in chemistry at the University of Sydney (Australia) encountered this essay question: "Every advance in science is an advance in method. Discuss."

[31] The obsession of philosophy of science with theory, in contrast to the essential empiricism of science, is nicely encapsulated in the remark that "Philosophers do not like infinite regresses, but biologists are used to them, in the form of the classic chicken-and-egg problem" (Maynard Smith and Szathmáry 1999, p. 11).

[32] Prizes up to 2002 inclusive were listed at http://www.almaz.com/nobel/ when accessed 4 February 2003.

[33] One sticking point is the "symmetry tenet" of the Edinburgh school: insistence that sociological analysis leave aside whether discoveries turn out to be real or spurious. That can lead to taking seriously, as instances of science, such patently incompetent claims as those ventured in popular books by Immanuel Velikovsky (Bauer 1984b).

[34] http://www.grc.uri.edu/grc_home.htm (accessed 7 February 2003).

[35] I attended three of these conferences, which bring together people from an eclectic mix of cultural and intellectual backgrounds. The meetings are organized at irregular intervals by the Unification Church through the International Cultural Foundation, and are presided over by such renowned individuals as Lord Adrian, Sir John Eccles, Robert Mulliken, Alvin Weinberg, Eugene Wigner. See http://www.icus.org/ (accessed 7 February 2003).

[36] Evidence to this effect is continually being added at http://www.virusmyth.net/aids/

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