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# What Are the Irreducible Components of the Scientific Enterprise?[1]

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**Abstract** — In this essay the author argues that many scientists (and laymen also) have come to take a narrow view of what constitutes science. They would exclude as "unscientific" observations that do not include a falsifiable theory with predictions testable and repeatable in laboratory experiments. In contrast, the author contends that science is more an attitude than a method. The attitude is one of testing interpretations of observed phenomena against further observations until one interpretation emerges as the best. Unfortunately, judgments about the best interpretation vary greatly, and scientists with new ideas and new observations must often overcome biases in their colleagues before their work becomes accepted as scientific.

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Perhaps I am unqualified to address the topic of this paper. The philosopher Hobbes "was wont to say that if he had read as much as other men, he should have known no more than other men" (Aubrey, 1949, p. 154). I have read so many papers and books on the philosophy of science that I surely know no more than other men. Therefore, in emulation of Hobbes, of whom Aubrey said that "his contemplation was much more than his reading" (p. 154), I have sought to neutralize my extensive reading by contemplation. This paper derives from my contemplations. In it I ask whether we can eliminate certain components that some experts describe as essential to scientific research. If we can do this, we would then expose an indispensable remainder. I will offer a list of what I reject as inessential and then describe the residue.

Before offering my list of eliminable features I wish to disclaim asserting that what is inessential is necessarily undesirable; in saying that we can do scientific research without some particular feature, I do not mean to disparage its inclusion when we find this feasible. I am searching for features without which we would not say that we are doing scientific research.

Perhaps the least essential feature is the location of our endeavors. We cannot conduct scientific research anywhere and everywhere; but we do not require a special room like a laboratory for its conduct. We can do it in a jungle, in a desert, and in many other places.

[1]Editor's Note: This essay is an expanded version of the Dinsdale Prize Lecture presented by the recipient, Ian Stevenson, at the 17th Annual Meeting of the Society for Scientific Exploration.

We can also dispense with experiments. These are of two types: those in which we make observations in a controlled setting and those in which, while maintaining all other features constant, we change one feature and record the result. Neither type of experiment seems essential. Astronomers would be unemployed if they were.

Observations do not have to be repeatable to qualify as scientific. The observations of paleontology and many of those of geology and astronomy are not repeatable, but we do not value them less for that. Indeed, it seems to be true that few reported observations are ever exactly repeated, even in laboratory experiments. Partly from additional curiosity and partly from a desire to be original, scientists repeating earlier observations frequently vary the details of their observations from what a predecessor has described.

I come next to the claim that we can only assert negatives (falsifiability) (Popper, 1959). Several factors warrant our rejection of falsifiability. First, scientists may disagree about whether a particular observation constitutes an adequate refutation of a theory (Lakatos, 1970; Naess, 1972). Second, a negative instance refuting a generalization may itself be trivial and the remainder much more important. For example, in the often cited example of black swans in Australia it is still the case that, so far as we know, all swans except the black ones in Australia are white. Mere falsifiability or actual falsification therefore tells us nothing about the importance of our observations, let alone what is important to investigate. Third, correlations may suggest causal relationships in which we can have confidence even when we cannot falsify our conclusion. No one who has examined the evidence can doubt that tobacco smoking is an important cause of lung cancer; the incidence of this disease clearly correlates with the duration and amount of tobacco smoking. Fourth, some of the most fruitful theories of science, such as that of evolution, are not falsifiable. Fifth, the belief that only falsifiable ideas are scientific may snuff out innovative ideas before they have had a chance to survive testing (Bohm & Peat, 1987).

I also believe that prediction is not an essential feature of scientific research. I include here predictions both of what exists but has not yet been "discovered" and of what will happen in the future. Three well-known examples will help me to justify my rejection of prediction as indispensable to scientific research.

Edmund Halley predicted in 1705 the return in 1758 of the comet later named after him. He based his prediction on his analysis of the orbits of comets reported in 1531, 1607, and 1682 (which latter appearance he had himself observed) and concluded that the same comet had been observed on each occasion and would return. Its return in 1758, however, added only a fourth observation to the three of which Halley took account before he made his prediction. This, in my view, made it only a little more likely that the comet would come back a fifth time. To this it may be replied that Halley had studied the orbits of the appearing comets and therefore had more data than their simple appearance at intervals of 75 years. Even so, how many of us would bet that an event observed four times would occur a fifth time? We would probably wish to have more instances before predicting a recurrence. Since the return of the comet in 1758 it has appeared in 1835, 1910, and 1986. Moreover, examination of earlier records, many of them Chinese, suggest that the comet has been observed at least 30 times altogether (Tattersfield, 1984). A person skeptical about predicting a recurrence after 4 instances would probably accept the likelihood of another recurrence after 30 instances. I go into these details in order to emphasize that Halley's prediction showed his confidence in the physics and cosmology of his friend Isaac Newton, but the predicted return of the comet in 1758 added little to the evidence justifying belief (at that time) that it would return after the appearance of 1758–59.

A second example occurred in the case of the discovery of Neptune in 1846. In that year Urbain Leverrier in France and John Couch Adams in England almost simultaneously predicted the existence of an outer planet whose gravitational force would account for the observed anomalies in the motion of Uranus. Each then tried to persuade his country's astronomers to search for the planet and told them where to look. Neither was successful with their own seniors, who controlled the observatories. Leverrier then appealed to the German astronomer Johann Gottfried Galle, who searched for the conjectured planet and found it. The predictions of Neptune's existence and location are widely regarded as the most important confirmation of Newton's scheme of the universe up to that time. I contend, however, that the predictions of Leverrier and Adams showed their confidence in Newtonian cosmology, but it was the existence of Neptune and its orbital behavior, not the predictions of them that provided a confirmation of the cosmology.[2] Neptune and its behavior like a planet might have been found without having been predicted. Indeed, it very nearly was. Later examinations showed that the French astronomer Joseph Lalande had observed Neptune's movement in 1795; he made two observations of what he thought was a star but that were different, and because of the discrepancy between them, he concluded that they were both wrong or doubtful. If he had made a third observation, he would have discovered Neptune 50 years before Galle did (Grosser, 1962).

My third example is that of Mendeleev's description of the periodic table of the elements. In this table Mendeleev arranged the 60 elements known at his time (1869) in the order of their atomic weights. He left four gaps in the table for elements whose discovery he predicted; he even described the properties that three of these would have, and these were identified within 20 years of the publication of his table (Asimov, 1964; Kelman & Stone, 1970). I again say that the prediction added little evidence to what was already available. Three elements added to 60 are not many. Mendeleev's greater accomplishment was ordering the 60 elements in a system far superior to previous ones. His

[2]One reader of a draft of this paper strongly disagreed with me regarding the discovery of Neptune. He pointed out that the computations of Adams and Leverrier based on Newton's cosmology permitted the prediction and discovery of a hitherto unknown planet.

predictions of elements not yet known showed his confidence in his system as did his correction of some of the experimentally derived atomic weights. Mendeleev's theory exemplifies Eddington's (1935) aphorism according to which one should not "put overmuch confidence in the observational results that are put forward until they have been confirmed by theory" (p. 211).

I will make two more remarks about predictions in science. First, a reading of the statements and correspondence of Halley, Leverrier, Adams, and Mendeleev concerning their predictions and the theories from which they made them shows the great confidence they had in the theories from which the predictions derived. Second, although I contend that predictions are not a necessary part of science I think they are part of the joy in doing science.

Some readers may think I have belabored prediction. It is not difficult, however, to find writers who assert the indispensability of prediction for scientific research. One of Whewell's aphorisms states: "It is a test of true theories not only to account for, but to predict phenomena" (Butts, 1968, p. 138). Carnap (1966) wrote that "The supreme value of a new theory is its power to predict new empirical laws" (p. 231). Claude Bernard asserted that "real science is not being conducted until one can accurately predict the occurrence of phenomena and control them" (Bernard, 1865/1952, pp. 199–200; my translation). More recently, Ayala (1996) has made an even stronger statement: "...being predictive of unknown facts is essential to the process of empirical testing of hypotheses, the most distinctive feature of the scientific enterprise" (p. 442). On the other hand, I have good company for my position. John Stuart Mill (1846), differing from Whewell, stated:

> ...it seems to be thought that an hypothesis... is entitled to a more favorable reception if, besides accounting for all the facts previously known, it has led... to the prediction of others which experience afterwards verified.... Such predictions and their fulfilment are, indeed, well calculated to strike the ignorant vulgar.... But it is strange that any particular stress should be laid upon such a coincidence by scientific thinkers. (p. 296)

Albert Einstein (1930) believed the predictions made by the general theory of relativity are less important than the "simplicity of its foundation and... its logical consistency" (p. 183). Eddington, whose measurement in 1919 of gravitational light bending did much to arouse interest in the general theory of relativity, believed that "the observational predictions form only a minor part of the subject" [relativity] (Eddington, 1923, p. v; Brush, 1989). Bauer (1992) cited the example of the concept of phlogiston, which, although fecund with predictions, some of them correct, became overthrown by a better theory of combustion.

If, as I believe, prediction is not an essential feature of science, why have so many scientists asserted that it is? The answer may be that predictions prevent theorists from elastically adapting their theories to include new ill-fitting observations. Predictions add to the discipline of science.

Resuming my list of dispensable features I come to quantification, either in measurement or in counting. I know that some notable scientists believe these features essential for science. Kelvin (1891), for example, wrote that “when you cannot measure it [what you are speaking about], when you cannot express it in numbers... you have scarcely... advanced to the stage of science” (p. 80). Nevertheless, measurement had no part in a small number of significant observations that no one would exclude from a history of advances in science. I count as examples: Jenner’s discovery of the effect of cowpox in preventing smallpox (1798); William Beaumont’s observation that gastric juice could “digest” food not only in the stomach, but when removed from the stomach and added to food placed in a vial (Beaumont, 1833); and Pasteur’s experiments that refuted the idea of spontaneous generation (Vallery-Radot, 1920).

The seventh and last feature that I believe dispensable is a control group. One can certainly engage in medical science without one. A single patient may be studied over a period of time by observations of what modifies a disease and what does not. Much of clinical medicine has to follow this method of trial, disappointment (or perhaps error), and a new trial of something different. Furthermore, such a process can produce results applicable to other patients, and we should not withhold the name of scientific research from it (Berwick, 1996; 1998).

In sum, I believe that seven features frequently mentioned as essential to scientific investigations are not (they are listed in Table 1). By eliminating them I am trying to reach a core of features that I believe are indispensable. As I cautioned earlier, however, some of the features that I have excluded from this core are frequently important — even essential — to certain kinds of research. In particular, repeatability is always desirable, and measurement and control groups are often essential, even though not always.

TABLE 1
Features That Are Inessential to the Scientific Enterprise

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| 1. Location |
| 2. Experimental Control of Conditions |
| 3. Repeatability |
| 4. We can only assert negatives (falsifiability) |
| 5. Prediction |
| 6. Quantification (as measurement or counting) |
| 7. Control Groups |

What then remains of the core components that I consider essential to the scientific enterprise?

First, it is a public process. The methods of observing and analyzing the results must be openly described so that, in principle, another qualified person could duplicate the observations and obtain the same results.

Second, the events and objects of observation must also be public and capable of being observed by others. This excludes subjective states, although not a person's report of his or her internal thoughts and feelings. This requirement distinguishes scientific research both from philosophy and from religious inspiration. Private knowledge occurs and may be extremely important; but so long as it remains private it is not within my definition of science.

Third, the observer's beliefs and expectations should not modify his observations. Darwin wisely wrote that "...there was much talk that geologists ought only to observe and not theorise; and I well remember someone saying that at this rate a man might as well go into a gravel-pit and count the pebbles and describe the colors. How odd it is that anyone should not see that all observation must be for or against some view if it is to be of any service!" (Darwin, 1903, Vol. 1, p. 195). True enough, but the view should not interfere with the observation. This is a counsel of perfection, because our hopes and expectations can easily influence what we see and hear. We can strive toward objectivity by exposing as fully as possible all observations that tend to weaken our own preferred interpretation of the data. If adversaries fire at us, let them use ammunition that we have given them. (How we achieve, or even approach, objectivity is a major problem in science to which I shall return later.)

Fourth, scientific research should be an advance on simple observation and common sense. It should provide a comparison between some property (or it may be a group of properties) that seems unknown with what is already known, or thought to be known. Thus if I wish to say that reincarnation is the best explanation for a group of observations that I have recorded, I must name and disqualify rival explanations. Reincarnation is an unknown, at least in current science. For fraud, cryptomnesia, distorted memories on the part of informants, and even — some would say — for extrasensory perception, as well as for several other explanations of the data, we have independent evidence, and therefore they are known, or thought to be known.

Fifth, the new observations should generate a theory of greater explanatory power for both new and old observations. I do not satisfy my concept of scientific research by giving a new name to a group of familiar observations. I must also show that the interpretation that I favor can explain all related observations better than the alternative explanations that I should exclude before asking other scientists to accept mine. For example, the concept that the earth is a sphere can explain observations that the concept of a flat earth cannot. It explains the round shape of the earth's shadow on the moon during eclipses; why when a ship moves away from us on the sea its hull disappears from our view before its mast does; why when we continue to sail or fly in a westward direction, for example, we come back to our starting place; why different stars are seen in the sky as one travels north or south from the equator; and why if two travelers, one in Spain and one in Greece, are 1500 kilometers apart and then begin to travel north, they will find themselves at the level of England and Poland, only 900 kilometers apart.

The greatness of Kepler's accomplishment lay not especially in his finding — after 19 previous conjectures — that the different positions of Mars fitted an ellipse; instead, it was in the colligation that all the other planets also move in elliptical orbits.

There is, however, a hazard in explanatory power. The history of science, especially of medicine, offers numerous instances of concepts for which their adherents claimed great explanatory power. Phrenology, homeopathy, and psychoanalysis are three examples, among many, of such theories. Later observations showed the claims made for them to be largely or entirely baseless. Mere explanatory value is unconvincing to most scientists (Stevenson, 1977).[3]

What was missing from the work of the proponents of superseded concepts? They were unable or unwilling to continue testing their theories against observations. My sixth essential element of scientific research is, therefore, the repeated testing of theory against later observations. Equally important is the continued testing of one's preferred theory against theories that other scientists may propose. This repeated testing of a theory against observations distinguishes scientific research from the dogmatic claims of cranks and pseudoscientists (Feyerabend, 1964). They assert their conclusions without testing them against observations.

I have already said that a scientist should publicly describe his or her methods so that another scientist can replicate them. We can rarely trust the observations of a single person in scientific matters. "All men are liable to error, and most men are, in many points, by passion or interest, under temptation to it" (Locke, 1690/1824, vol. 2, p. 235). Given the difficulty of making observations devoid of distortion due to the observer's expectations, replication by other investigators provides our best corrective of such distortion. Every scientist with confidence in his work and conclusions should welcome replication by other investigators. This is indeed the only way by which a scientist can win acceptance of his observations and ideas. Independent replication is therefore my seventh essential element in scientific research.

Table 2 lists the seven features of research that I consider essential. It also lists, separately, an eighth feature that I believe necessary for success in any scientific enterprise: the persuasion of other scientists to accept the validity and importance of one's observations.

This task brings us back to the problem of objectivity. We need to ask what its criteria are. I believe it is nothing more than the acceptance of a scientist's views by a sufficient number of other scientists. Ziman (1968) has written that "the objective of science... is a consensus of rational opinion over the widest possible field" (p. 9). Elsewhere Ziman (1978) wrote that the goal of science "is to achieve the maximum degree of consensuality. Ideally the general body of scientific knowledge should consist of facts and knowledge that are firmly established and accepted without serious doubt" (p. 6).

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[3]In the paper cited I described the explanatory value of the idea of reincarnation for six important unsolved problems in biology and medicine. The paper drew considerable attention at the time, judging by an immense number of requests for reprints and considerable attention from the media. Nevertheless, no independent investigator undertook to replicate my observations about children who claim to remember previous lives for more than a decade (Mills, 1989).

TABLE 2
Features that are Essential to the Scientific Enterprise

1. Methods must be public and publicly reported
2. The observed events must be public
3. Objectivity in observations, that is, investigator's beliefs and expectations should not influence the observations and reports of them
4. Comparison of one observation or set of observations (usually new ones) with another observation or set of observations (usually what is known or thought to be known)
5. Superior explanatory power of theory derived from the new observations
6. Repeated testing of theory against new observations and alternative theories
7. Independent replication of observations

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8. Persuasion of a majority of scientists of the accuracy of the observations and the usefulnes of the derived theory

Ziman's idea of what we may call a majority vote that is needed to authenticate new observations or theories leads to the question of authority in science. Who is to say what facts "are firmly established and accepted without serious doubt?" Ziman's answer to this question is, continuing the quotation just cited, "an overwhelming majority of competent well-informed scientists" (p. 6). In practice these scientists, or other persons of authority, who control appointments, promotions, funding, and access to publication in journals and books make judgments about objectivity.

Unfortunately, even the most experienced scientists may be found lacking in objectivity (Arp, 1987; Hetherington, 1988[4]; Marshall, 1990). Moreover, they too often believe that current knowledge will endure forever. What we now teach, they tell us, will not be changed in any substantial way. In recent years two scientists have published confident assertions of the immutability of current knowledge (Cromer, 1993; Wilson, 1998). Such certitude has been given the name of a disease called "kelvinitis." The name derives from Lord Kelvin's statement, at the end of the 19th century, that "all the discoveries in physics had been made and... it remained only to adjust the last decimal point in a few measurements" (Asimov, 1964, p. 306)[5]. Another term for the same condition is "presentism," whose advocates assure us that present, reliable knowledge has finally replaced long periods of bleak ignorance (Micale & Porter, 1994). "They live under perpetual illusion of fundamental understanding" (Bauer, 1992, p. 75).

Persons afflicted with kelvinitis or presentism are usually unaware of the social and economic forces that facilitated the development of scientific ideas in one direction instead of in another. To them the way things are seems natural, immutable, and just the way they should be.

They are terribly wrong. The history of science offers us many examples of

[4]Hetherington took his examples of lack of objectivity from astronomy. In an endnote on bibliography he gives references to lack of objectivity among scientists in other disciplines.

[5]About a decade later, Michelson (1902) repeated the same assertion in different words. Three years after that, in 1905, Einstein published his special theory of relativity.

the supersession of one theory and the rise to dominance of a successor. Personal decisions and motives different from the search for truth frequently influence the succession of currently favored theories. To take only one example, the history of the ascent of genetics to its current hegemony in biology cogently demonstrates the influence of social and political forces in the evolution of one branch of scientific research. T. H. Morgan (1866–1945) began his professional career as an embryologist. Later, he became interested in the inheritance of traits. Considering that Darwin's *The Origin of Species* was published in 1859 and that Mendel's discoveries concerning inheritance were brought to the attention of a wider scientific readership in 1900, Morgan was slow to convert to the idea of evolution and to embrace genetics as a legitimate field of scientific research (Bowler, 1988; Shine & Wrobel, 1976). When he did convert in 1910, however, he, with his students and colleagues, established a scientific society and a journal of genetics; they trained graduate students who took teaching positions and propagated other students until their view of what is important in biology came to dominate the field and still does. Early in his enthusiasm for genetics Morgan separated it from embryology, and in the ensuing development of biology, embryology, and the problems of morphology became neglected, almost overlooked (Sapp, 1987; Bowler, 1988). The few biologists who tried to draw more attention to morphology and its problems (Russell, 1916/1982; Thompson, 1917) were respected but largely ignored.

The history of science shows that today's facts may become tomorrow's falsehoods. At different times the following were regarded as facts: the earth stands still and the stars move; the sun goes around the earth; the earth is flat; stones cannot fall out of the sky because there are none there; atoms are indivisible; atoms may break up, but this can never be exploited as a source of energy; space is filled with ether; machines heavier than air can never fly; continents cannot split and the parts move away from each other.

Let me now list some of the current "facts" accepted by most of today's scientists: natural selection fully explains evolution; the mind is only the behavior of a physical object called the brain; a human mind cannot communicate with another mind without the mediation of the currently known sensory organs; personality derives entirely from genetic inheritance and postnatal environmental experiences. Research now under way, some of it by members of the Society for Scientific Exploration, is already producing observations that challenge the status of these assertions as "facts." I predict that at least some of them will cease to be regarded as "facts" by the middle of the next century.

Theories, like living organisms, have life histories. They are born, mature, grow old, and die. Like humans, they are often repaired, in their case with subsidiary explanations; but eventually unfitting anomalies lead to their demise. In saying this I adhere to Kuhn's idea of scientific revolutions (Kuhn, 1962). (I do, however, believe — contrary to Kuhn — that much scientific knowledge is cumulative.) Even before Kuhn, Polanyi had exploded the myths of detached observation and impersonal knowledge (Polanyi, 1958). Both of them, however, had a predecessor, Ludwik Fleck, who in 1935 published a book (in German) with the remarkable title (in English) of "Genesis and Development of a Scientific Fact: Introduction to the Concept of Thoughtstyle and Thought-collective" (Fleck, 1935/1979). By "thoughtcollective" Fleck meant the beliefs held by the majority of scientists at any one time. In order for new observations to achieve the status of fact they must become sufficiently persuasive to convince a majority of scientists that they deserve that promotion. The thoughtcollective, however, does not change easily. Lakatos (1970) put this more bluntly when he wrote that "the history of science cannot be fully understood without mob-psychology" (p. 140).

*I believe it is more difficult now than formerly to introduce new ideas and concepts and have them accepted by scientists.* I attribute this to the larger number of practicing scientists compared with former times. This larger number of scientists increases the likelihood that one or more of them will make important discoveries. Unfortunately, it also has the disadvantage of presenting a larger mass of scientists resistant to change. "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, 1950, pp. 33–34). To this we may add that death is often not a sufficient facilitator of the acceptance of new ideas.

How did science arrive at this condition? It sometimes seems that little has changed since Francis Bacon, who, surveying the world of learning in his time, remarked that "the last thing anyone would be likely to entertain is an unfamiliar thought" (1607/1964, p. 79).

Nevertheless, scientists have not been deprived of exhortations to open their minds to new ideas. Heraclitus (c. 500 B.C.) warned: "If you do not expect the unexpected, you will not find it" (Kahn, 1979, p. 31). Other notable scientists have urged their colleagues to examine every observation without prejudice. Bacon — justly regarded as the instigator, if not the practical founder of modern science—wrote "that rarities and reports that seem incredible are not to be suppressed or denied to the memory of men" (Bacon, 1605/1962, p. 29). Herschel, one of the first writers on the philosophy of science, told his readers "A perfect observer... will have his eyes opened, as it were, that they may be struck at once with any occurrence which, according to received theories, ought not to happen; for these are the facts which serve as clews to new discoveries" (Herschel, 1830/1840, p. 132). A little later, Claude Bernard, one of the founders of modern physiology, warned that "if one sees only facts congruent with one's formed ideas, one will often miss making discoveries" (Bernard, 1865/1952, p. 215; my translation).

One of the most subtle writers of advice for scientists was Laplace, who added to his admonition to be unprejudiced a further, neutralizing, phrase. He first wrote: "We are so far from knowing all the forces of nature and their processes that it would show little wisdom to deny phenomena just because we cannot explain them in the present state of our knowledge" (Laplace, 1814/1840, p. 133; my translation). To this, however, he added: "...we ought to examine them [inexplicable phenomena] as much the more scrupulously as it appears the more difficult to admit them." The Swiss psychologist Theodore Flournoy called this aphorism "Laplace's Principle," and stated it as: "The strength of the evidence must be proportional to the strangeness of the facts" (Flournoy, 1899, p. 345; my translation). Flournoy then goes on to ask who is to be the judge of the "strangeness of the facts" and reaches the obvious conclusion that such judgments are subjective. The credulous ignore Laplace's principle, but the skeptic can use it to demand evidence stronger than anything offered to him or her. Then, when given some stronger evidence, he or she may demand something yet stronger. Such a person can always protect a prior belief from erosion.

Scientists who think for themselves have few defenses against the thought-collective. In principle, peer review of research grant applications and articles submitted to scientific journals should boost their chances of escaping the vigilance of thwarting conservatives. That it does not do so is not news. In 1793 the Royal Society rejected Jenner's paper on vaccination (Magner, 1992); he published it privately five years later (Jenner, 1798). In the next century anonymous reviewers for the *Annalen der Physik* refused to publish Helmholtz's paper on the conservation of energy (Graneau & Graneau, 1993). Readers who wish experimental evidence of the imperfections of peer review can find it in Mahoney's (1977) study of the influence of personal bias on referee's judgments of the quality (and hence suitability for publication) of manuscripts submitted to a journal. Horrobin (1990) has gone so far as to stigmatize peer review as a suppressor of innovation.

Remedies have been proposed. I suggested that a portion of funds granted for research be trickled down though administrators who, at each level would be obliged to divide most of what they receive to those at a lower level; thus even a beginner in research would receive some funding with which he could do research as he wished (Stevenson, 1966). Wessely (1998) has reviewed other suggestions for improvement that have been suggested. Perhaps Horrobin (1996) has made the boldest recommendation. He proposed that the entire sum available for, say, medical research in the United Kingdom, should be divided among all (British) researchers with academic medical positions. By his calculations each investigator would thus receive about U.S. $85,000 per year. There would be no research grant applications, no reviewing committees, no required reports. Research administrators would disappear. The grants would be good for ten years. Renewal for another decade of support would require evidence of productivity in research. Horrobin predicted that his plan would lead immediately to increased productivity — and more innovation — on the part of scientists.

The Society for Scientific Exploration was founded to combat the problem presented by the resistance to new ideas on the part of scientists. The founders of the Society hoped that it would prize open the half-closed minds of other scientists so that deviant ideas could find more receptive audiences among their colleagues. A friend, whom I exhorted to join the Society when it was newly founded, declined and warned me that the new society could have an effect opposite to that intended. Editors of conventional journals, he believed, could say "We do not have to publish this wild paper. Let it go to the *Journal of Scientific Exploration*." Thus the isolation of dissidents would continue without more than trivial discomfort to the authorities who were keeping them in exile. The Society and its members, he predicted, would become isolated and considered harmless, like an encysted foreign body in a living organism. I have to say that he may have been right. In this respect the Society has not succeeded. It has not, however, failed; and it can yet succeed. I am sure it will.

Some readers may reproach me for wandering away from the methodology of science into its sociology. I agree that persuading one's colleagues to take one's research seriously is not part of the research itself, but it definitely has to be part of the scientific enterprise. No scientist has completed his or her work until other scientists have been persuaded to continue the research he or she has initiated.

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