Against the Tide

A Critical Review by Scientists of How Physics and Astronomy Get Done

Martín López Corredoira
Carlos Castro Perelman
(Editors)

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FOREWORD
MARTÍN LÓPEZ CORREDOIRA AND CARLOS CASTRO PERELMAN

It is always necessary to take a critical look at the way in which scientific research actually gets done. While Philosophy and Sociology have long established themselves as full-blown academic disciplines with an ever-increasing literature, there is a dearth of such literature written by practising scientists. The aim of this book is to gather the views of some working physicists and astronomers on the influence of the social structures of science within which scientists are obliged to carry out their research, and examine the ways in which they are sometimes used in negative ways to destroy careers and hinder innovative research. At the present time there are no widely known academic outlets where scientists can express their opinions about the scientific establishment. Not so long ago there were astronomical journals where one could raise these issues but these journals have either ceased to exist or have been revamped into pure research journals. Physicists have no outlet for expressing their views—especially unorthodox views—on the nature of the scientific method and/or social structures affecting their research because journals for physicists are solely dedicated to research. This book aims to fill this current gap in the literature with a sample of critical papers.

The essay "Challenging dominant physics paradigms" by Campanario and Martin is a general analysis of the difficulties found by well-qualified scientists to challenge scientific orthodoxy. Particular cases of dissidence are reflected in the autobiographical odysseys narrated by Kundt, Arp or Castro Perelman. Castro Perelman tells us about the illicit, shameful censorship and blacklisting of scientists taking place in the electronic e-archives web-site <http://arXiv.org> and which is the most important internet site for preprints in Physics, Astronomy and Mathematics. Scientific and political elites in Western democracies control the system, according to Arp. Anonymity in the peer-review system is susceptible of corruption—says Marvin Herndon—and interferes with the objective examination of extraordinary ideas on their merits—says Van Flandern. These problems of science are worldwide and present in rich countries like the United States, as pointed out by Marvin Herndon, as well as in developing countries. Apostol talks about the corruption, decadence and mafias in Romania hidden behind the use of politically correct terms: "technological transfer", "international cooperation", "scientometrics", etc. Like the example of Romania, many other countries have similar problems. The same problems in Physics and Astronomy are widespread in all fields of science and in all areas of research performed by humans. One representative of the text outside the fields of Physics and Astronomy is the essay by Bauer, where a critical study of analytical chemistry and the conflict of interests in science is presented. We considered it interesting because the context of his essay is applicable to all the sciences in general. The situation in Astrophysics is widely described in López-Corredoira's essay on the oldest profession. Kundt tackles some aspects of astrophysics too, Arp focuses in the research of cosmology, and Van Flandern's article focuses on the solar system.

Whether the alternative ideas in these fields are correct is something at least doubtful because they are risky and speculative proposals. The vision of complexity in knowledge versus the mechanistic interpretation of Nature, as explained by Kirilyuk, is also challenging and highly philosophical. In any case, suppression of these ideas does not look the best way to do and filter science or promote progress in human knowledge. The possibility of removing good ideas from the stage is something harmful for the search of truth. Moreover, the Gold effect, mentioned by Kundt, by which a mere unqualified belief can occasionally be converted into a generally accepted scientific theory through the screening action of refereed literature, of meetings planned by scientific organizing committees, and through the distribution of funds controlled by "club opinions", is another disturbing element for the search of truth. It leads to unitary paradigms in the sense expressed by Kirilyuk, unitary thinking not necessarily associated to the unique truth.

There are two main attitudes one could take towards the present scenario in science. On the one hand, in López-Corredoira's chapter "What is research?", or in Apostol's chapter "Where is the Science?", one finds a pessimistic view without offering any solution, in which the state of science nowadays is decaying and waiting for its death. Science had an important role in the history of Western societies, but it is now too eroded to serve the ideal of a "science for the sake of science". On the other hand, Rabounski's declaration of academic and scientific freedom, as well as the conclusions of many other authors along the book guided by the same *leitmotiv* of "freedom of research", envision some hope to improve the current system of how science is done in order to avoid many of the problems described in this book. The two last contributions by Bauer and Castro Perelman reveal both a pessimistic view of the miseries of the actual system with a glimmer of hope in promoting ethics in science.

Of course, there is a third position which is not included in this book, which consists in saying that science has no major problems at all and the dissidents or scientists with critical ideas about the system are simply bad scientists with wrong points of view. Are the authors of this book mere charlatans who do not deserve any attention whatsoever about their complaints about the current system? There may be many strange, inconsistent or exaggerated points of view in the scientific contributions of some authors, and some points of view expressed here about the sociology of science might be misleading as well, but this is not a reason to avoid presenting and discussing them. All the authors elaborated this book with the honest hope to improve, to help, to analyse the system, with the sincere purpose to search the truth, both in nature and science as a human activity.

Many scientists were asked to contribute to this book but many feared all sorts of retaliations, like loss of funding and jeopardy to future tenure positions; many feared shame and ridicule from their peers; some very prominent ones would have contributed if we had found a front-line publisher; while others thought it was an utter waste of time for they believe it is hopeless, by definition, to try to change the way science is done nowadays because it involves humans. By offering this book to the readers we hope to find a middle ground between what may be perceived as the lesser of two evils: a young pessimist versus an old optimist.

The purpose was to collect miscellaneous contributions of diverse authors in order to avoid sectarianism and to promote criticism from different points of view. A minimum level of quality was ensured because all of us are scientists with experience in professional research and/or University departments rather than amateurs. Many of the coauthors are well-known and highly reputed dissident scientists with a very long experience in battles against dominant paradigms. We have not tried to separate the different kinds of contributors, although we are aware that most of the challenging heterodox ideas have high probabilities to be incorrect. Even if somebody is wrong, there should be some outlet where one could express his/her complaints about the current state of science and the system which breeds it. The question here is not whether somebody has correct or wrong ideas about Physics and Astronomy (history will tell and not necessarily the present orthodox publications) but to analyze the problems of the official mechanisms under which current science is being administered and filtered. Nobody should have a monopoly of the truth. Whether the present ideas are part of a revolutionary movement in science or not is something which cannot be judged from an absolute point of view. Judge for yourself!

Martín López Corredoira, Editor.
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CHALLENGING DOMINANT PHYSICS PARADIGMS[1]

J. M. CAMPANARIO AND B. MARTIN

Juan Miguel Campanario
Departamento de Física,
Universidad de Alcalá,
28871 Alcalá de Henares, Madrid, España (Spain)
Email: juan.campanario@uah.es
Web: http://www.uah.es/otrosweb/jmc

Brian Martin
Science, Technology and Society,
University of Wollongong, NSW 2522, Australia
Email: bmartin@uow.edu.au
Web: http://www.uow.edu.au/arts/sts/bmartin/

Abstract: There are many well-qualified scientists who question long-established physics theories even when paradigms are not in crisis. Challenging scientific orthodoxy is difficult because most scientists are educated and work within current paradigms and have little career incentive to examine unconventional ideas. Dissidence is a strategic site for learning about the dynamics of science. Dozens of well-qualified scientists who challenge dominant physics paradigms were contacted to determine how they try to overcome resistance to their ideas. Some such challengers obtain funding in the usual ways; others tap unconventional sources or use their own funds. For publishing, many challengers use alternative journals and attend conferences dedicated to alternative viewpoints; publishing on the web is of special importance. Only a few physics dissidents come under attack, probably because they have not achieved enough prominence to be seen as a threat. Physics could benefit from greater openness to challenges; one way to promote this is to expose students to unconventional views.

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Physics has a reputation as one of the most highly developed and well established fields of science. Although there are many exotic-sounding theories at the research frontier involving strings, black holes and charm, the basic postulates of classic theories such as electrodynamics, relativity and quantum theory are seen as solidly established.

It is surprising to find, therefore, that there are many challengers to orthodox physics who offer critiques of conventional theories and present their own alternative formulations. Furthermore, many of these challengers are well qualified, with degrees, mainstream publications, positions at well-known universities and prizes including the Nobel Prize. Table 1 gives a few examples, listing only a selection of these particular challengers' achievements. This is not a ranking of dissidents; there are others with just as many accomplishments.

Table 1. A sample of well-qualified challengers to orthodox physics

Halton Arp is a professional astronomer who has worked at the Mt. Palomar and Mt. Wilson observatories. He has received the Helen B. Warner prize, the Newcomb Cleveland award and the Alexander von Humboldt Senior Scientist Award. He has published a large amount of evidence that contradicts the big bang (Arp 1987, 1998).

Andre Assis is professor of physics at the University of Campinas, Brazil, is the author of several books and over 50 scholarly articles and is a leading authority on Weber's electrodynamics. He is a critic of relativity (Assis 1994, 1999).

Robert G. Jahn is professor of aerospace science and dean emeritus of the School of Engineering and Applied Science at Princeton University and has received the Curtis W. McGraw Research Award of the American Society of Engineering Education. He researches mind-matter interactions.

Paul Marmet was professor of physics at Laval University, Québec, for over 20 years, is author of over 100 papers in electron microscopy, was president of the Canadian Association of Physicists and has received the Order of Canada. He is a critic of relativity.

Table 1. (cont.) A sample of well-qualified challengers to orthodox physics

Domina Eberle Spencer is professor of mathematics at the University of Connecticut and has published several books and over 200 scholarly articles. She supports an alternative theory of electrodynamics, in the Gaussian-Weberian-Ritzian tradition.

Tom Van Flandern has a PhD in astronomy from Yale University, became chief of the Celestial Mechanics Branch of the US Naval Observatory and received a prize from the Gravity Research Foundation. He is critic of theories of the big bang, gravity and the solar system (Van Flandern 1993).

If you decide to question a widely accepted theory, or to present data that is anomalous in terms of current understandings, it can be difficult to gain a hearing. Although the essence of scientific advance is going beyond current knowledge or offering a new way of understanding data, questioning fundamentals is seldom welcome. Some types of challenge, such as perpetual motion machines or causality violation, are automatically rejected. Others, such as cold fusion, are openly considered and tested but then, if they do not measure up, henceforth rejected by mainstream science.

It is easy to dismiss challengers as "cranks," but this risks rejecting fresh ideas from those who are well placed to achieve radical breakthroughs. There are instances where the official expert view is later revealed as unproductively dogmatic, as when the French Academy rejected observations by common people of stones falling from the sky. It may be that "the kinship of the scientific crank with the scientific creator is more than a superficial one" (Watson 1938, 41) but few scientists embrace this connection.

A proponent of an unorthodox idea is likely to encounter several types of difficulties. First, it is difficult to obtain funding: very few research grants are awarded for proposals to re-examine long accepted theories. Most funding agencies expect that proposals will build on existing science rather than challenge basic postulates. Second, it is difficult to publish in mainstream journals. Third, proponents of unorthodoxy may come under attack: their colleagues may shun them, they may be blocked from jobs or promotions, lab space may be withdrawn and malicious rumors spread about them. Even if they can overcome these problems, they have a hard time gaining attention.

Our focus here is on strategies used by challengers to overcome such obstacles. In the next section we outline ideas from the social studies of science that help to explain the way science responds to challenges. Then, drawing on responses to questions we submitted to dozens of physics dissidents, we look at methods used by challengers to current paradigms to obtain funds for research, publish their work and survive attacks. We conclude with some observations about how challenges to orthodoxy, even though most of them are judged wrong, can be used constructively.

Understanding challenges

Of the large body of research in the history, philosophy, psychology and sociology of science, we here pick out a few key ideas that are helpful for understanding why challenges to orthodoxy are likely to be given a cold reception. We have found that some earlier ideas, now superseded in the eyes of many, remain useful for explaining challenges and responses, though for other purposes these same ideas have important limitations.

The most common view about how science works is that new ideas are judged on the basis of evidence and logic: if a new idea explains more data or provides more precise agreement with experiment, this counts strongly in its favor.

Karl Popper claimed that science advances by falsification (Popper 1963). In his view, it is the duty of scientists to attempt to disprove theories, confronting them with experimental data and rejecting them if they do not explain the data. Theories that cannot be falsified are, according to Popper, not scientific. Many scientists believe in falsificationism.

These conventional views were challenged by Thomas Kuhn (1970). Kuhn argued that scientists — and physicists in particular, since most of his historical examples were from physics — adhere to a paradigm, which is a set of assumptions and standard practices for undertaking research. If an experiment gives results contradictory to theory, then instead of rejecting the theory altogether, alternative responses include rejecting the experiment as untrustworthy and modifying the theory to account for the new results (Chia 1998; Chinn and Brewer 1993).

When anomalies accumulate, the paradigm can enter a state of crisis and be ripe for overthrow by a new paradigm. This process of scientific revolution does not proceed solely according to a rational procedure but involves social factors such as belief systems and political arrangements. Kuhn's successors have modified his model of paradigms and revolutions, for example showing that paradigms are not as well defined and incommensurable as Kuhn imagined, but they have extended his insight that the process of scientific change involves social factors and is not just a rational matter (Barnes 1977, 1982; Collins 1985; Fuller 2000; Mulkay 1979; Pinch 1986).

In any case, the idea of paradigms puts a different spin on the problem of new ideas in science. Rather than being dealt with according to logic and evidence, challenging ideas may be ignored or rejected out of hand because they conflict with current models. In effect, the logic and evidence used to establish the paradigm are treated as definitive and are unquestioningly preferred over any *new* logic and evidence offered that challenge the paradigm. During periods of "normal science," the ideas developed by mainstream scientists originate *from* current paradigms: they add more and more pieces to standard puzzles. Given that the paradigm is the source of ideas, it is not surprising that challenges to the paradigm — the framework that allowed mainstream scientists to contribute to the development of science — are seldom greeted with open arms. If a theory is not considered physically plausible, it may be rejected even though it makes successful predictions (Brush 1990).

Eminent philosopher of science Imre Lakatos says that research programs have a hard core set of fundamental principles surrounded by a set of subsidiary, less significant assumptions, called the protective belt. For the research program to advance, lesser assumptions can be tested and possibly modified, protecting the hard core from being falsified (Chalmers 1999, 130-136; Lakatos 1970).

Conventional science education helps to perpetuate current orthodoxy. Students are introduced to physics through textbooks that typically present current ideas as "the truth" and either ignore alternative ideas altogether or portray them as convincingly disproved by experiment. Students learn by solving problems, and the concepts and magnitudes used in these problems assume the validity of current theories. Only rarely are students presented with theories that don't work, and even in those cases, such as Bohr's model of the hydrogen atom, the intent is to show how researchers overcame problems. By and large, students are confronted only with success in science. Acceptance of received wisdom is deeper because orthodoxy is never discussed as orthodoxy: it is simply the truth. Students are also taught about the "scientific method" — observation, hypothesis formulation, testing, etc. — and hence come to believe that theories that have been tested by experiments are true, because the textbook scientific method is thought to be the way science actually operates. Views that science actually proceeds in a different fashion are seldom mentioned (Barnes 1974; Bauer 1992; Feyerabend 1975). Relevant here is a famous quote from Max Planck (1949, 33-34): "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."

The system of examinations and degrees is a sorting process; the physics PhD screens out most of those who question orthodoxy (Schmidt 2000). Once students are committed to the basic principles of the field, then it is possible to begin research and to question, within implicit limits, prevailing ideas.

There are definite advantages to training and researching within a standard framework. Rather than spending lots of time getting bogged down in the basics, researchers instead can move more rapidly and confidently to the cutting edge, pushing out to unexplored areas of knowledge and, thus, reinforcing and developing the paradigm. As long as the basic principles of the field are sound, it makes sense to simply learn them and build on them. Traditional education seldom tells students how to go about challenging current paradigms.

There is another obstacle facing challengers: the psychological commitment of scientists to current ideas, especially their own ideas and the dominant ideas. The usual image of the scientist is of a cool, calm, detached, objective observer, but the reality is quite different (Mahoney 1976; Mitroff 1974), as anyone who knows scientists is aware. The classic study of the psychology of scientists is Ian Mitroff's book *The Subjective Side of Science,* in which he revealed that Apollo moon scientists were strikingly committed to their ideas, so much so that contrary evidence seemed to have little influence on their views. As well, scientists express strong views, often quite derogatory, about other scientists. To expect every scientist to react coolly and objectively to a competitor's idea is wishful thinking, though there are some scientists who approach the ideal. Intriguingly, Mitroff found that it was often the top scientists who were the most strongly committed to their ideas.

Tom Van Flandern commented to us:

> *I have taken aside several colleagues whose pet theories are now mainstream doctrine, and asked quizzically what it would mean to them personally if an alternative idea ultimately prevailed. To my initial shock (I was naïve enough that I did not see this coming), to a person, the individuals I asked said they would leave the field and do something else for a living. Their egos, the adulation they enjoy, and the satisfaction that they were doing something important with their lives, would be threatened by such a development. As I pondered this, it struck me that their vested interests ran even deeper than if they just had a financial stake in the outcome (which, of course, they do because of grants and promotions). So a challenger with a replacement idea would be naïve to see the process as anything less than threatening the careers of some now-very-important people, who cannot be expected to welcome that development regardless of its merit.* (1 August 2002)

Though it is easy to criticize dogmatism, a certain amount of it can be valuable for scientific progress. That was certainly Kuhn's view: unless the current paradigm was in crisis, dogmatism in science education and practice has a functional value (Kuhn 1963). Michael Polanyi, a chemist and eminent commentator on science, argued "that the scientific method is, and must be, disciplined by an orthodoxy which can permit only a limited degree of dissent, and that such dissent is fraught with grave risks to the dissenter" (Polanyi 1963, 1013) Similarly, Mitroff concluded that the classic norms of science, such as universalism, disinterestedness, communism and organized skepticism, did not adequately explain the operation of science, and instead proposed that counternorms were equally important, including "organized dogmatism."

Another problem facing challengers stems from the intense pressures under which most scientists work. Many scientists, especially those who are ambitious, work extremely hard. They may spend long hours in the lab or in problem-solving on top of other duties such as teaching, supervision and administration, not to mention life outside of work. Science is highly competitive and even the most talented scientists need to work hard to stay ahead of the game.

What happens when some challengers, who have spent years or decades developing their ideas, show up and ask a busy career scientist for an assessment? Even for an open-minded or sympathetic scientist, it is a real sacrifice to spend days or even just hours examining alternative ideas, since that means correspondingly less time available for their own pressing work. The more eminent scientists serve as editors and referees for prestigious journals where they typically are focused on rejecting work that *fails* to meet the standards of orthodox science, making it even more difficult for them to accept work that *challenges* those standards.

Most challengers believe their ideas have value, otherwise they would not bother with them. What they desire from mainstream scientists is not acceptance (though that would be nice!) but a fair-minded examination of their ideas. There is a certain irony here: challengers confront academic power and what some of them see as corruption, but what they really desire is the attention of mainstream scientists. The practical problem facing challengers is a scarcity of attention: there are not enough scientists who have both the time and inclination to scrutinize their unorthodox ideas.

The way that science is organized exacerbates the problem of shortage of attention for paradigm challengers. Most scientists work as part of a small network, local and/or international, members of which address the same topic, share common interests and goals, exchange information and reprints, and attend the same conferences (Crane 1972). Scientists are more likely to devote attention to work by others in their network than they are to the work of outsiders. Dissidents who go to the roots of a paradigm do not specialize sufficiently to be part of such a network: they are outsiders in the field in the sense that they do not focus on a small portion of a paradigm. As a result, few scientists will be willing to give them any attention.

In summary, perspectives and evidence from the history, philosophy, psychology, and sociology of science, and from science education, suggest that the obstacles facing challengers are formidable. Most scientists, due to their education and day-to-day interactions, work within the prevailing paradigm. Most scientists develop a strong commitment to their own ideas, a psychological process that is reinforced by the large career investment in a particular line of work. Finally, the competitive struggle for success means that most scientists are extremely busy, with little time available to examine unconventional ideas.

There is, though, a contrary force: the rewards available for significant innovation. The founders of quantum theory and, above all, Einstein as the founder of relativity theory are heroes in physics for inaugurating new paradigms. Even short of these epic feats, physicists may aspire to be known for their contributions, which often means questioning the received wisdom.

Choosing research problems can be likened to an investment process (Bourdieu 1975, 1988). Scientists have available a certain amount of "capital" — knowledge, experience, time and effort — that they can invest in different ways. A conservative investment strategy is to pursue small, incremental innovations, with a high likelihood of success and a modest return on investment, following Peter Medawar's dictum that science is "the art of the soluble" (Medawar 1967). A risky strategy is to pursue a speculative idea: the likelihood of success may be low but the returns, if the idea pans out, can be huge. For example, astrophysicist Fred Hoyle could be said to have originally invested in the steady-state theory of the universe, which had decent prospects but turned out to be a bad bet. He later made a riskier investment in the more speculative "life-in-space" hypothesis (Hoyle and Wickramasinghe 1978) which, if validated, would have more dramatic returns (though now too late for Hoyle). In a sense, paradigm challengers are ambitious investors, in that they commonly criticize entire theories, such as relativity and quantum theory, rather than just a part of such theories. They seek to change theories at the level of university textbooks.

A different investment calculation comes into play, though, when it comes to someone else's ideas. To examine or even promote someone else's challenge to orthodoxy requires significant time and energy, yet the major returns go to the other person, if they are recognized as the innovator. If the idea is a promising one, the temptation is to grab credit, for example by domesticating the radical idea and publishing in orthodox journals. It is no surprise that many innovators are afraid of having their ideas stolen.

So although there is an incentive to pursue unorthodox ideas, only some researchers will be tempted to do so. The obstacles remain daunting, especially given that paradigm-challenging ideas are seldom taken seriously. Furthermore, few will have the eminence of a Hoyle to attract attention to their ideas.

Investigating dissent in physics

Our aim is to gain insight into how challengers can overcome the obstacles facing them. We began our empirical investigation by examining a range of work — including our own — on resistance to scientific innovation (Barber 1961; Bauer 1984; Campanario 1993a, 1993b, 1995, 1996, 1997, 2003; Mauskopf 1979; Nissani 1995; Sommer 2001) and suppression of dissent (Hess 1992; Horrobin 1990; Martin 1981, 1996, 1998, 1999a, 1999b, 2004; Moran 1998). From the large array of obstacles facing challengers, we concluded that three areas are of crucial importance: obtaining funding, getting published, and dealing with attacks. Though there are other types of obstacles, we focus on these three since our interest is less in obstacles than on ways of overcoming them.

By examining a diverse set of challenges, we came up with a list of ways of overcoming these obstacles. (See Table 2.)

Table 2. Some methods that challengers can use to overcome barriers to their work

1. Funding

A. Obtain funding from innovative agencies.

B. Obtain funding from agencies not worried about the innovative aspects.

C. Obtain private funding.

D. Fund the research through personal resources.

E. Apply political pressure to obtain funding.

F. Use conventional funding but disguise the nature of the research.

2. Publishing

A. Challenge the editor's rejection.

B. Use friends or patrons to help get published.

C. Submit to other journals.

D. Publish in many different journals and conferences.

E. Keep publishing after the initial breakthrough.

Table 2. (cont.) Some methods that challengers can use to overcome barriers to their work

F. Seek wider audiences beyond the key discipline.
G. Set up a journal or a special section in an established journal; attend alternative conferences.
H. Send out preprints.
I. Publish books.
J. Publish paid advertisements.
K. Seek coverage in the mass media.

3. Surviving attack
A. Continue without being distracted or discouraged.
B. Seek support from others who have come under attack.
C. Expose the existence of attacks, especially their unscientific aspects.
D. Expose the bias or vested interests of the attackers.
E. Seek support from colleagues or a professional association.
F. Counterattack using similar methods.
G. Take legal action.
H. Join with others who have come under attack.

To determine which of these methods are actually used in physics, we obtained the addresses of a sample of dissidents by means of webpages of journals such as the *Journal of Scientific Exploration* and meetings and societies of dissidents. To exclude most of the many uninformed and unsophisticated critics, we restricted our attention to those who have scientific degrees or are affiliated with reputable universities or have publications in mainstream journals, though no doubt this restriction excludes some worthy challengers. Given our aim of finding a diverse group satisfying our criteria — namely that they are challengers to dominant physics paradigms who have scientific degrees or research positions or publications — our search was extensive but not exhaustive.

We did not attempt ourselves to judge the quality of the dissidents' work. Whatever our own ideas about some research work's rigor, agreement between theory and experiment, quality of expression and the like, others would be likely to differ in their assessments, especially because judgments about quality are commonly mixed with views about whether conclusions are right or wrong.

Hence, rather than use personal assessments in our selection process, we relied on the surrogate measures of degrees, affiliations and publications, which encapsulate the collective judgments of other scientists.

We wrote to a total of 41 well-qualified dissident scientists, mostly in physics, inviting them to describe their experiences in overcoming resistance to new ideas in science. We drew their attention to our list of methods (Table 2) but invited them to tell about any other methods that they had used. We obtained many fascinating responses, some of which are mentioned below. We did not seek to collect statistical data on use of different dissident strategies, because our aim was exploratory; responses to our letters were wide-ranging, suggesting the limitations of imposing neat classifications at this stage. Not enough is yet known about dissident strategies to make it worthwhile pursuing quantitative categorization, especially given that self-reports may reflect different judgments about matters such as success and failure of a strategy.

Because our aim was to find out how contemporary challengers try to overcome obstacles, we ruled out those who were once dissidents but subsequently succeeded in obtaining recognition. There are a number of these who could be cited (Hook 2002; Hunt 1983), of which one of the most prominent is S. Chandresekar (1969), whose ideas on stellar evolution were initially rejected by Sir Arthur Eddington and others. Examination of such cases suggests that most dissidents encounter the same sorts of obstacles whether they are ultimately vindicated or not.

How to mount a challenge

Although we asked about experiences in *overcoming* resistance, many respondents focussed more on the resistance itself, commenting critically on the nature of the scientific establishment. For example, Paul Marmet said that "Scientists prefer to stick to old theories even if they do not make sense. I was at first very surprised by that reaction but, after a few years, I had to admit that it is a normal human reaction" (28 July 2002). Ruggero Maria Santilli, president of the Institute for Basic Research, said "There simply is no way of correcting academic-scientific corruption and I consider futile any attempt at that" (4 August 2002). According to Bruce Harvey, a dissident physicist, "To say that the established scientific world is prejudiced against new ideas is an understatement. It is paranoid about them." (13 August 2002).

On the other hand, some respondents believed that, despite resistance, in the long run their ideas would be recognized. David Bergman said that "Nevertheless, I am confident that the truth will come out and Common Sense Science will prevail as valid science. I have no ideas how long it will take, or how many will come to accept the scientific truth that modern physics must be replaced (not reformed)." (30 July 2002).

Funding

Innovators often have a hard time obtaining funding from conventional sources. Sometimes their funding is withdrawn. One option is to obtain a job in science — often by doing conventional research — and use it as a base to do unorthodox research. Those who are more successful using this strategy can even create a lab or institute, such as Princeton Engineering Anomalies Research, a laboratory for research on the mind-matter relationship. This option is more common for scientists who become interested in unorthodox ideas after establishing an orthodox career, as in the case of Brian Josephson, who won the Nobel Prize in physics for the discovery of the effect named after him and who is now working in parapsychology.

Sociologist Ron Westrum, who has studied the scientific community's response to anomalous phenomena (Westrum 1977, 1978), thinks that the most comfortable basis for mounting a challenge to orthodoxy is as an older or retired professor (23 October 2002). Historian-of-science Stephen G. Brush offers similar advice: obtain a secure job and do conventional research to establish a reputation, thus laying a foundation for proposing radical ideas (31 October 2002). However, these options are available to only some individuals.

Some pursue unorthodox ideas by using conventional funding but disguising the nature of the research. We are aware of a case in which astronomers, while using a major telescope for observations on a conventional research topic, used a bit of spare time at the end of their observing run to look for something different, relevant to an unorthodox theory. Richard A. Muller (1980) revealed how he circumvented the funding system for innovative (though orthodox) research. According to David Horrobin (1989), editor of *Medical Hypotheses,* scientists know that to obtain funding they must misrepresent their motivations in grant proposals, otherwise "All innovative scientists know that they would rarely get funded, such is the nature of the review system."

Parapsychology researcher Helmut Schmidt — a physicist by training — was employed by Boeing Scientific Research Laboratories when he carried out some of his early work using quantum random number generators (Schmidt 1969). (Later he worked in a private research institute.) Corporate funding has sustained cold fusion research after it was rejected by mainstream science (Simon 2002).

There are a few grant-giving bodies open to unorthodoxy, such as the Lifebridge Foundation. Money for some types of unorthodox projects is available from the military, which does not want to miss potential applications no matter how unorthodox the theory behind them.

Other challengers do not ever get started in a conventional career. They are more likely to support their work through their own funds. This helps explain why so many challengers focus on theories; personal funds are seldom sufficient to sustain a significant laboratory. Cynthia Kolb Whitney, editor of *Galilean Electrodynamics,* says that "Personal resources have worked best for me. Though resources are modest, there are no discontinuities, uncertainties, interferences, or other annoyances" (17 August 2002).

Publishing

Innovators often have a difficult time getting published. Submissions may be rejected or subjected to significant delay. Major revisions may be required. Even when published, the work may be neglected. Challengers have used a variety of methods to promote their ideas.

Sometimes unconventional papers are rejected without refereeing or any critical comment, in which case the author can request a formal assessment. Apparently *Nature* previously returned all submissions from private addresses without looking at them; one of those so treated was atmospheric scientist James Lovelock, later best known for his Gaia hypothesis (Bond 2000). Authors also can contest the comments made by journals, asking for re-evaluation. Of course, challenging the editor's rejection is a technique available to all scientists, but it is especially important when ideas may be rejected out of hand.

After a rejection, it is a standard technique to scout around to find somewhere else to publish. Challengers often have to search more widely in doing this. However, there is a down side, as indicated by Paul Marmet: "Spending too much time in an effort to publish our ideas in conventional journals leads to serious frustration. That is a trap, which destroys the delicate ability that can lead later to new ideas." (28 July 2002).

Stephen G. Brush recommends writing balanced review articles, with plenty of citations of other authors, for publication in a journal such as *Reviews of Modern Physics,* allowing the possibility of some self-citation (31 October 2002). However, we are unaware of any dissidents who have adopted this approach.

Even when challenging ideas are published, they may be ignored (Collins 1999). Therefore, publishing in a range of journals and presenting papers in a variety of conferences maximizes the chance that someone will take the ideas seriously.

Parapsychologists set up their own journals, rigorously refereed, such as *Journal of Parapsychology* — reputable enough to be included in the Science Citation Index database — to get around the low acceptance rate in mainstream journals. Other examples are *Journal of Scientific Exploration, Galilean Electrodynamics, Frontier Perspectives, Infinite Energy, Cold Fusion* and *Apeiron.* Several of our respondents reported favorably on alternative journals. Vladimir Ginzburg said he "published five papers in the journals that are receptive to speculative ideas, *Speculations in Science and Technology* and *Journal of New Energy.*" Caroline Thompson, who has challenged standard views on quantum entanglement, commented: "I attempted to publish my next important paper in *Physical Review Letters.* The story of its rejection is the subject of my Tangled Methods paper [Thompson 1999]. The paper in question has now been accepted by *Galilean Electrodynamics.* I have had other papers in *Infinite Energy* and the *Journal of New Energy,* and contributed chapters to a few books." (16 August 2002).

Conferences dedicated to alternative viewpoints, and conference proceedings, provide a venue for challengers. Domina Eberle Spencer, who has worked since the 1940s on reformulating electromagnetic theory, reports that as well as new journals open to discussing fundamental questions, "International meetings which wel-