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Rejecting Highly Cited Papers: The Views of Scientists Who Encounter Resistance to Their Discoveries From Other Scientists

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We studied the views of scientists who experience resistance to their new ideas by surveying a sample of 815 scientists who are authors of highly cited articles. The 132 responses (16.2%) received indicated that only 47 scientists (35.6%) had no problems with referees, editors, or other scientists. The most common causes of difficulty were rejection of the manuscript, and scepticism, ignorance, and incomprehension. The most common arguments given by referees against papers were that the findings were an insufficient advance to warrant publication, lacked practical impact, were based on a wrong hypothesis, or were based on a wrong concept. The strategies authors used to overcome resistance included obtaining help from someone to publish problematic papers, making changes in the text, and simple persistence. Despite difficulties, however, some respondents acknowledged the positive effect of peer review.

Introduction

The history of science includes some important discoveries that initially met resistance or were ignored by fellow scientists (Barber, 1961). Some important discoveries were "premature" in the sense that they did not fit the common paradigms, or their implications could not be connected by a series of simple logical steps to existing scientific knowledge. These discoveries were often rejected and deemed impractical for quite some time after their initial introduction (Stent, 1972, 2002). In other words, some theories or discoveries collided with the dominant paradigms in science, and were therefore slow to be accepted.

Almost all leading journals use a peer review system to evaluate and select contributions. Manuscripts are reviewed by members of the editorial staff or by one or more external referees who are experts in their fields. Stephen Lock, former editor of the *British Medical Journal*, also felt that peer review "favours unadventurous nibblings at the margin of truth rather than quantum leaps" (Lock, 1985, p. 1560). Medical editor David F. Horrobin also accused the peer review system of impeding the publication of innovative ideas, and illustrated his accusations with an amazing set of well-documented cases (Horrobin, 1990). Other scientific contributions and data can be suppressed for a variety of reasons (Martin, 1999a, 1999b).

Some common patterns of resistance to scientific discovery can be distinguished: papers are rejected, fellow scientists ignore discoveries, articles are not cited, or commentaries are written against the new finding or the new discovery. Sometimes it is very hard to convince the experts of the worthiness of an alternative idea (Martin, 1996, 1998). In other instances, authors of very innovative papers are openly criticised. Scientific contributions are effectively silenced (Sommer, 2001) and prevented from being published for years. For example, in June 1932, Dr. C.F. Koelsch submitted a paper to the *Journal of the American Chemical Society* (JACS) about the synthesis of a pentaphenyl allyl radical. The manuscript was rejected and it was not published until 25 years later (Koelsch, 1957). Other examples of discoveries that were initially resisted by scientists are now taught in basic science courses, as with Arrhenius's theory of electrolytic dissociation, Wegener's theory of continental drift, or Eyring's theory of the activated complex in chemical reactions (Nissani, 1995; Campanario, 2002a).

Having a paper published does not imply that it will automatically receive attention from the scientific community. Stent, a molecular biology researcher, coined the term "premature discovery" to describe a finding that is not fully appreciated at the moment of its announcement because it cannot be connected by a series of simple logical steps to canonical or generally accepted knowledge (Stent, 1972, p. 84). A similar kind of neglect has been also described by other authors. For example, Eugene Garfield, former

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president of the Institute for Scientific Information (ISI), has used citation analysis to identify some cases of "delayed recognition" (Garfield, 1989a, 1989b, 1990). According to the approach used by Garfield, a typical article that shows delayed recognition receives very few citations, and may not be cited at all for years. Later, the scientific community begins to recognise the value of the article and the number of citations starts to rise. The article can then be said to have been "discovered" by the scientific community (Garfield, 1990). It is interesting to note that the articles that show the phenomenon of delayed recognition are usually published in widely read journals, so this phenomenon cannot be attributed to difficulties with access to the relevant scientific information. In recent years, research on this type of delay has awakened increasing interest, and premature discovery was the subject of a recent monograph (Hook, 2002).

In previous articles we used a systematic approach to study resistance to new discoveries (Campanario, 1993, 1995, 1996). We have examined essays on highly cited papers in order to identify articles that were difficult to produce or to get published. A Citation Classic^R is a paper or book that, by definition, is extraordinary because of the large number of citations it has received in comparison to other publications. Citation Classics^R have been identified in the ISI database since 1977. Authors of Citation Classic^R candidates are invited to write a short essay on the highly cited publication, and some of these shorts essays have been published in *Current Contents* (Campanario, 1993). Some of these Citation Classic^R papers were first rejected by the editors and referees of the journals to which they were sent. Indeed, even some of the most frequently cited papers of all time and the most highly cited papers in some scientific journals were rejected at first (Campanario, 1995, 1996). Here, we use another approach to identify papers that were difficult to publish, and papers whose authors experienced other kinds of problems.

Objectives

We wished to expand the previous analysis of resistance to scientific discovery by scientists to a wider population of prestigious researchers who have encountered resistance from other scientists to their discoveries. To record the views of prestigious scientists who encountered resistance to new ideas or theories in different fields of science, we surveyed a sample of scientists who were authors of highly cited articles. We also wished to document the phenomenon of delayed recognition by examining scientists' own accounts of resistance from the rest of the research community, once their article had been published and subsequently highly cited. A further aim was to record the opinions of the scientists on the role of the peer review system in the resistance to new ideas in science, and obtain advice about strategies intended to overcome such resistance.

Method

We surveyed scientists who were authors of highly cited articles. Although we asked them about their highly cited papers, they also commented on other cases in which they had experienced difficulties. Authors of highly cited papers were identified from http://www.isihighlycited.com. These scientists are identified by the ISI as having published highly cited or very influential papers in their fields. A personalised letter containing three questions was sent to a sample of 815 scientists who were authors of highly cited articles (see Appendix). The letter contained two open questions and a questionnaire. This questionnaire has been used in previous research (Campanario & Martin, 2004) to identify the strategies used by dissident scientists who challenge current paradigms. We sent a follow-up survey to try to obtain the highest response rate possible.

For each reply obtained we constructed a record in which we classified the kind of work that encountered resistance (theoretical, experimental, etc.), the kind of problem, the solution, and the observations or comments from the scientists concerning their experience with resistance to their discoveries.

Results and Discussion

We obtained 132 replies in all, for a modest response rate of 16.2%. Four scientists sent additional material (mostly papers and copies of letters). We received six answers from Nobel Laureates. Other scientists suggested we contact colleagues who could offer additional information about the topic of our research. Thirteen scientists declined to participate in our survey, citing lack of time as the main reason (9.8% of respondents). This nonresponse rate may reflect the numbers of scholars who have had no problems with the current structure or practice of peer review. In addition, we believe that the scientists we surveyed are very busy because many of them are heads of large research teams who hold important positions at well-known institutions. Given the low response rate, we cannot generalize from the responses we obtained. Our analysis therefore focuses on an attempt to understand the views of scientists who encountered resistance to their highly cited discoveries.

Of the 132 scientists who responded, 47 reported no problems with referees, journal editors or other scientists (35.6%). Below we offer an analysis of some of the problematic cases described by the other respondents.

Resistance From Peers

The first point concerns the kind of work involved. In some cases we were able to identify the following categories: experimental (38 cases), theoretical (9 cases), and review (3 cases). In 48 cases the scientists who experienced resistance said that in their opinion, the work was innovative. In 19 instances our respondents indicated having problems with more than one paper. From the answers obtained in our survey we identified the kind of problem with the

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TABLE 1. Classification of the kind of difficulties encountered by scientists who participated in our survey.

	INSTANCES
Rejection of the manuscript	33
Scepticism, ignorance and incomprehension	8
Conflict with prevalent ideas	5
Ideas not accepted by journals and scientific community	6
Problems with referees	2
Lack of significance or boring	2
Attack from scientific groups	1
Disagreement in interpretation	1
Time to implement ideas	1

work, and classified the kind of difficulty into broad categories (Table 1). The most common causes of difficulty were rejection of the manuscript, and scepticism, ignorance, and incomprehension.

Sometimes difficulties arose even before the scientists tried to publish their results. In our opinion, some funding agencies rarely risk money on innovative projects, and department heads are not always aware of the importance of an original paper. In a previous study we found that sometimes, scientists who challenged dominant paradigms had to use private funds to carry out their research (Campanario & Martin, 2004). However, some grant-giving bodies are open to unorthodoxy, one such example being the Lifebridge Foundation (http://www.lifebridge.com). Money for some types of unorthodox projects is also available from the military, which does not wish to miss potential applications no matter how unorthodox the theory behind them (Campanario & Martin, 2004). In the present study we found 10 examples in which the scientists mentioned problems in obtaining funds. A common complaint concerning this problem was that funding agencies are often too conservative, and it is difficult to obtain funds for innovative projects. However, it should be pointed out that this is a different type of peer review. Below are some examples of scientists' comments:

> To get a proposal funded by the NSF [National Science Foundation] you have to describe in detail what you will accomplish and provide a timeline. This isn't research, this is development. To someone who worked in industry for 25 years, "research" is figuring out how to get to where you want to go. You start with a gut feeling that the problem can be solved, then you work until you find the solution. If you can see where you are going, you aren't going very far. Unfortunately NSF has never realized this, and so, instead of stimulating new ideas it tends to stifle them. I get my funding from the DoD, which takes a much longer-term view and funds people, not projects (D. Aspnes).

> I have had some difficulty in obtaining funds for "blue skies" ideas that later did result in significant results. Again this seems to reflect the ethos of the funding bodies in the UK who require evidence that the grant will be successful in meeting its objectives. Thus risky work sometimes is harder to get funded (V. Morris).

> I remember, still with some pain, having an application of mine to NIH [National Institutes of Health] rejected by the study section. I was told that I was only proposing this work to get more funds when I already had a surfeit of funding, that I knew very little about doing separations, and this technique would have limited utility. Today, it has become commercialised (R. Zare).

> Scientists should be given more freedom to develop and implement their own research ideas and programs without spending an undue amount of time and effort applying for grants. Funding decisions by grant-giving agencies are based on inadequate reviewers' panels expecting the applicants to provide unreasonably specific details of proposed research and to anticipate in advance the usefulness of the expected results (E. Frankel).

A possible solution to this type of problem might be to earmark part of the available funds for risky projects proposed by scientists with a solid reputation. It may even be desirable to give support to specific scientists rather than individual projects. Funds could be awarded to researchers with an outstanding record of professional achievement without obliging them to go through the usual application procedures.

In five instances, the scientists complained of unjustified delays in the review from the referees. Complaints concerning delays in peer review systems are common in the literature (Campanario, 1998a, 1998b). In 8 instances the scientists suspected ill-intentioned motives on the part of referees or competitors. For example:

> The reviewers of our papers asked harsh questions and in some cases made unreasonable demands that couldn't be satisfied (Participant 14, Nobel Laureate).

> There was a strong and articulate group of classical neuroscientists who missed no occasion to publicly discredit the findings. They either challenged the reproducibility of the phenomenon altogether or if they believed in the phenomenon they would claim that it was functionally irrelevant etc. (Participant 129).

A common complaint against peer review is that anonymity favors this kind of behavior (Campanario, 1998a, 1998b). In some research fields it can be hard to find a referee who is qualified to judge the science, yet is not a competitor. However, most editors seem reluctant to use "open" peer review, i.e., to make the reviewers' names known to the authors. Anonymity is assumed to protect referees against reactions or retaliation by authors who were unable to accept negative evaluations. It is nonetheless worth noting that this fact clashes with the widely held perception of science as an altruistic activity that aims to seek the truth above and beyond any other considerations.

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TABLE 2. Arguments given against the problematic articles.

	INSTANCES
Insufficient advance to warrant publication, and no practical impact	9
The work was based on a wrong hypothesis	8
The concept was wrong	6
The research was done before	3
Paper too long	2
The results go against the predominant concepts of that time	2
The work was unfamiliar to the scientific community	1
Threatening to some research fields	1
Reviewers didn't understand the concepts	1
The work was like an advertisement or recipe	1

Arguments Against the Findings and Responses From Authors

We also collected the arguments offered against papers with problems (Table 2). The most common criticisms were that the findings were an insufficient advance to warrant publication, lacked any practical impact, were based on a wrong hypothesis, or were based on a wrong concept.

Once the problem had been identified, we were interested in the solutions that scientists used to overcome resistance. The first option was to talk with the editor. This approach was used by 15 scientists, and according to some published surveys scientists sometimes resort to this strategy. For example, Simon, Bakanic, and McPhail (1986) followed the fate of rejected manuscripts and tracked authors' reactions when a complaint was lodged with the editor of the *American Sociological Review*. In 60% of the cases the editor sent the paper to a new set of referees; in 22% of the cases the original decision to reject the manuscript was maintained with no further refereeing. Thirteen percent of the authors who complained obtained a reversal of the initial decision, and their manuscripts were accepted for publication (Simon, Bakanic, & McPhail; 1986).

Ten scientists used the help of a friend or colleague to publish. For example:

> Our conclusions were highly controversial at the time of publication and the papers we submitted to the *European Journal of Biochemistry* would have been rejected had Hans Dieter Soling, the Managing Editor who handled them, not been an expert on this particular topic and overruled one of the referees! His view was that we should be given the benefit of the doubt. Subsequent research showed that his decision was a wise one (P. Cohen).
>
> Fortunately, we submitted our papers to journals that had strong, far-seeing editors who over-rode the recommendations of narrow-minded reviewers and allowed our papers to be published (Participant 14, Nobel Laureate).

To nonscientists this strategy may seem strange. Many active scientists believe that the evidence presented in manuscripts submitted to journals should be considered sufficient to convince editors and referees of the merits and relevance of their contributions. However, the comments reproduced above illustrate a different situation: some scientists who have authored highly cited articles have been obliged to seek help from other researchers to publish their results.

A frequent solution was to change something in the paper. As might be expected, another strategy was to submit the article to another journal. Some surveys have found that many rejected papers are submitted with only minor changes to different journals (Campanario, 1998a, 1998b). For example, a study of manuscripts rejected by the *Journal of Clinical Investigation* found that 85% of these papers were subsequently published elsewhere (Wilson, 1978). In our survey we found that this strategy was used by 16 scientists. It should be noted, however, that some changes probably improved the articles in question, so not all problems with referees result in negative outcomes in terms of publication.

Initially Negative Responses From the Scientific Community and Subsequent Citation

We were also interested in the initial response from the scientific community. Scientists told us that the responses were positive in 32 cases, negative in 22 and indifferent in 18. "Positive" responses indicated that the paper was very well received and cited in contemporaneous review and opinion journals, quickly obtained many citations, and was rapidly used and expanded upon by others. "Negative" responses indicated that the paper was considered highly controversial at the time of publication, or led to incomprehension, resulting in the work being ignored and rejected. In some cases opposition was due to competing ideas or interests, or simply to initial astonishment at the conclusions. Indifferent responses comprised those that expressed scepticism, neglect of the findings until they were published everywhere, or simply no response. In some instances, extreme reactions were encountered:

> I stopped being invited to conferences and people stopped talking to me (Participant 19).

For a scientist, the response from the rest of the community is of fundamental importance. The difference between success and failure of a new theory, a discovery, or a new application depends on whether other scientists make use of the information. If the rest of the research community considers the proposal or the advance to be useful, the article will have an impact that can often be measured by the number of times it is cited. Scientists whose contributions are valued by the rest of the scientific community will be asked to speak at conferences and other events, and will be invited to participate in joint research and new projects. These consequences may conflict with naïve conceptions about science: success, for a basic researcher, consists mainly of eliciting positive opinions from the rest of the research community.

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We also found two curious cases in which scientists reported problems with nonscientific activist groups:

> I have had problems with nonscientists misusing and misconstruing some of my research papers. The . . . paper in NEJM [New England Journal of Medicine] has served as the basis of an urban legend (Participant 72).

One scientist said he had problems with an industry representative, someone from the press, and a professor. The critics published a letter in the press in which they publicly declared that his theory was wrong.

Given that one of our goals was to identify new instances of delayed recognition, we asked how long it took for the research to be recognised by the scientific community. We found 12 instances in which the scientists needed 10 years or longer to be recognised by the scientific community, and 11 instances in which recognition was delayed less than 10 years.

> My work was accepted in some circles (Environmental Microbiology), and ignored for 20 years in other areas (Medical Diagnostic Microbiology) (Participant 24).

> Uneven. Some were hailed right away as important, others it took 15 years to accept I was right (or sometimes wrong!) (Participant 34).

> I and my colleague . . . (who also is in the highly cited list) published papers on and proposed since the beginning of the 1990s that neurofibrillary tangles (NFTs), formed by tau and Lewin bodies (LBs) formed by what we know to be alpha-synuclein, played a role in mechanisms of neurodegeneration in Alzheimer's (AD) and Parkinson's (PD) and related disorders, but this was met with disbelief until the discovery of tau and alpha-synuclein gene mutations pathogenic for neurodegenerative diseases in 1998 and 1997, respectively. (Participant 53).

> My fellow Physical Organic chemists thought I had gone mad by shifting my focus away from experimental work, and I suffered over a decade of peer rejection until my crusade (along with a few others) to convince chemists of the virtues and advantages of computational chemistry by personal example finally succeeded. (P. Schleyer).

> My answer: "less is more, and all the data fit the single receptor, but it did take 25 years for me to convince everyone that the D2 receptor was the common site for all antipsychotics." (P. Seeman).

> The manuscript was rewritten and submitted to the *European Journal of Pharmacology* where it was published in 1990. This, in my opinion, is a seminal piece of work because it opened an area of investigation that ultimately targeted the NMDA [N-methyl d-aspartate] receptor as a final common pathway of antidepressant action. As an aside, about a decade later, a report appeared (R. Berman, et al., Biological Psychiatry, 2000) demonstrating a rapid and robust antidepressant effect of an NMDA antagonist (Participant 100).

These findings lead us to consider another aspect of knowledge transfer that is poorly understood and has been little studied to date: the reception given to science publications. Once a researcher has completed a study and published the results, the success of his or her proposals is far from guaranteed. As noted earlier, the rest of the research community will decide—by applying the new ideas—whether a given study or proposal is successful or not. Occasionally, it is not enough to get published. In the cases summarized above it took considerable time before some articles began to be fully accepted by the rest of the research community.

As in previous studies, we found that some previously rejected papers were highly cited after being published elsewhere. In fact, as noted above, all the scientists surveyed for this study were identified because they are authors of highly cited articles:

> I have, during the past 40 years, submitted six articles to that journal (*Physical Review Letters*). Three of them got accepted and three rejected, the latter because of not offering enough news value. The three rejected manuscripts were all accepted by other respected journals and are all heavily cited. None of the three articles accepted in *Physical Review Letters* had a high impact. (P. Sigmund).

In at least one instance, success was complete: the 97th American Oil Chemists' Society (AOCS) Annual Meeting held in 2006 in St. Louis, Missouri, included a symposium in honor of Professor Edwin Frankel, celebrating 50 years of lipid oxidation research (http://www.aocs.org/meetings/annual_mtg/).

Answers From Nobel Laureates

Analysis of the answers from Nobel Laureates yields some interesting insights. In a previous study we identified instances in which 27 future Nobel Laureates encountered resistance on the part of the scientific community towards their discoveries. We also found instances in which 36 future Nobel Laureates encountered resistance on part of scientific journal editors or referees to manuscripts that dealt with discoveries that at a later date would earn them the Nobel Prize (Campanario, 2007).

Two Nobel Laureates indicated that they did not encounter resistance, but we found other cases in which their discoveries were met with at least some resistance. Table 3 shows the types of resistance encountered by four Nobel Laureates who responded to our survey.

Highly Cited Authors' Views of Peer Review

Some scientists complained about the current peer review system, and suggested alternatives and improvements. For example:

> The present refereeing process used by scientific journals is exceedingly slow, inefficient, and inadequate because it

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TABLE 3. Problems encountered by Nobel Laureates who took part in the survey.

Field of Nobel Laureate	Problem
Physics	Ignorance and incomprehension from the scientific community because they said his work was unfamiliar. This also led to a delay in publication by a referee who did not understand the work.
Physiology or Medicine	Scepticism towards his discoveries. The reviewers asked harsh questions and in some cases made unreasonable demands that could not be satisfied.
Physiology or Medicine	Scepticism, initially, from the scientific community. It took more than 15 years for his work to be recognized.
Chemistry	A paper was rejected by *Physical Review Letters* in the first instance although it was published after a considerable delay.

depends on volunteer editors and reviewers. Many editors and reviewers are busy professionals who cannot spend the time and effort necessary to properly evaluate scientific papers. We need to reevaluate this reviewing system, which slows down research, by using paid full-time professional editors. The system should select more efficient and competent reviewers who should be rewarded for their efforts (E. Frankel).

Very often two referees will take opposite views. Which one to placate? It's not an ideal system, but it is the best we have. Editors have an important part to play in moderating the conclusions of referees (Participant 18).

The best explanation I have of this phenomenon is the nature of the peer review system. One cannot expect that a random referee will have the same foresight as the author. (Sometimes even the author has no idea how important something will become over time.) A truly novel idea or revolutionary result will always be met with scepticism. In that respect I like the e-print system better. It allows room for new ideas, although it lacks checks for technical competence (Participant 45).

My view of the peer-review process is that often it is more of a lottery, than a reflection of good work. Yes, there is probably some correlation between a good paper and getting it through the review process, but the standard deviation can be quite large. In other words, good papers probably have a better chance of being accepted, but no guarantee. The same with a poor paper, which can also have a very large variance around it (i.e., it may not necessarily be rejected) (R. van Genuchten).

The rather limited openness to new ideas is the most important inherent weakness of the peer review system, both for scientific journals and for funding agencies, but I have not yet found a better system. The only way out is probably to become more independent from impact factors, citation index etc., when it comes to promotion, job opportunities etc. But who is the first step? According to my experience, today's researchers are less self-confident, less independent, less fighting for their own ideas etc. than two decades ago (Participant 78).

Interestingly, some scientists told us that they expect this kind of resistance and problems with work that is highly innovative or that challenges dominant paradigms. For example:

I have the impression that revolutionary discoveries in science always meet a combination of skepticism and enthusiasm. The skeptics are people who are committed to a certain world-view that is challenged by the discovery. The enthusiasts are the people who are more open to change. I can understand the reasoning of both groups. The skeptics are understandable because most discoveries billed as "revolutionary" turn out to be artifacts or misinterpretations of results that are actually consistent with prevailing theory. The skeptics are correct in the great majority of instances. On the other hand, the enthusiasts are also correct because they realize that all scientific belief is provisional, and is always susceptible to replacement by a more powerful theory (as stated by Popper, 1959). To strike a proper balance between these opposing viewpoints it is necessary to hold up all science to rigorous scrutiny, and to accept a revolutionary idea only when it has resisted all possible attempts to disprove it. By definition, the first announcement of a revolutionary discovery will never satisfy the second criterion, so skepticism is warranted. That skepticism should never prevent the publication, dissemination or funding of the new idea because these are the only means by which it can be tested. Premature choking-off of new ideas is fatal to the scientific process itself (Participant 14, Nobel Laureate).

I am under the impression that it continues to be true that there seems to be an Inverted U in the probability of fast acceptance of new scientific work, as a function of the work's quality and paradigm-shifting power (Participant 20).

The barriers to new ideas are part of the skeptical side of science. It's probably important that there is resistance to changing paradigms because it takes a lot of energy in the field to do this and it's terrible when this energy is wasted on an incorrect new idea. So it's probably protective for the field in general (M. Dustin).

Orthodox thinking has often replaced liberal thinking. Censorship has occasionally invaded the scene. Nowadays, it has become much more difficult to publish unorthodox results and views than to remain in the mainstream which is never as exciting as to be on the original side (U. Heber).

Most of the significant findings I have made were initially rejected by the journals they were submitted to, especially when they went against the dogma at that time (Participant 54).

Editors are people too. They can make errors like anybody else. This is my experience in the daily struggle for being competitive in research (Participant 123, Nobel Laureate).

Other comments expressed positive assessments of the current peer review system regarding its role in support of acceptable levels of quality of scientific work. However,

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the result will be some resistance to innovative work. For example:

> There was always some rationale behind the rejection, and I do understand these things. Mistakes are made by human enterprises, but the wonderful thing about science is that the truth inevitably comes out, and great papers generally get published (Participant 23).

> First, I wish to express gratitude to competent reviewers whose critical remarks have helped me to improve our work. The peer review system has for a long time been highly effective to increase the quality of published work. We have enormously profited from the altruism of unpaid first-class scientists who devoted their time to the improvement of the science of others. However, my impression is that during my career I have witnessed not only the rise, but now unfortunately also the decline of the peer review system. The flood of manuscripts has finally been too much for the limited number of competent colleagues to maintain the quality of judgment. It is admittedly difficult for editors to find reviewers who are willing to do unlimited work for others. In their predicament, they may feel forced to go for reduced quality. Whereas it is the task of reviewers to judge manuscript[s], it is not too difficult for an experienced author also to judge the quality of a reviewer. This quality has, in my opinion, declined (U. Heber).

> I have found that the most significant papers that I have written have generally been the most difficult to publish. However, I believe this actually vindicates the refereeing system. When I have challenged an accepted idea the referees have asked for far more details about the work and challenged the interpretation more than they seem to with work that just backs an accepted idea. In a way that is probably good because new ideas should be rigorously tested (V. Morris).

> On the opposite side of the coin, scientists are correct to be cautious and demanding of more data and proof. There is a lot of carelessness in research. Good critics (yes, and enemies) make us work harder. After all, science is about challenging hypotheses (Participant 110).

Although the peer review system may pose a barrier to innovation, it is clear that in many instances it helps improve articles that are submitted to journals. Occasionally reviewers detect errors that the authors have overlooked. For example, *Physical Review Letters* rejected the first account of the theory of the fractional quantum Hall effect by Nobel Prize winner Robert B. Laughlin because a referee discovered mistakes (Laughlin, 1998). In the present study, the positive opinions about peer review expressed by the authors of articles that were highly cited speak in favor of the need for journals to have a mechanism for quality control in place. The problem lies in distinguishing between useful contributions from referees and criticisms that can impede the development of new knowledge. Comments that can hinder publication of important new ideas are more likely when the article being critiqued goes against the dominant points of view in the field.

Strategies to Overcome Resistance

One of the aims of our survey was to identify strategies used by researchers to overcome initial resistance and lack of recognition from the rest of the academic community. Table 4 shows the distribution of answers to the third question in our survey, concerning the methods used to overcome resistance. The most common approaches were to persevere without being distracted or discouraged, obtain funding

TABLE 4. Methods used by scientists to overcome resistance.

3.1 Methods that have been used to obtain funds	N
A. Obtain funding from innovative agencies	6
B. Obtain funding from agencies not worried about innovative aspects (such as the military)	—
C. Obtain private or personal 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	3
3.2 Methods to promote new ideas	
A. Challenge the editor's rejection	8
B. Use friends or patrons to help get published	3
C. Submit to other journals	16
D. Publish in many different journals and conferences	—
E. Keep publishing after the initial breakthrough	12
F. Seek wider audiences beyond the key discipline	—
G. Send out preprints (to sidestep journal rejections)	3
H. Publish books (as an alternative to journal publication)	3
I. Publish directly on the Internet	4
J. Publish paid advertisements	—
K. Seek coverage in the mass media	—
L. Send to influential people in the field	3
M. Give conferences	21
N. Create tests, analogies, extensions of the idea to provide a consistent pattern	4
O. Set up a journal or a special section in an established journal	—
3.3 Methods to survive attacks	
A. Continue without being distracted or discouraged	15
B. Seek support from others who have come under attack	1
C. Expose the existence of attacks, especially their unscientific aspects	—
D. Expose the biases or vested interests of the attackers	—
E. Seek support from colleagues or a professional association	3
F. Counterattack using similar methods	—
G. Take legal action	—
H. Join with others who have come under attack	1
I. Work hard	2
J. Patience	4
K. Perseverance	11
L. Find out what the objection is	1
M Communicate directly with the editor	9
N. Send more detailed evidence and documentation	1
O. Give arguments supporting the case	6
P. Listen to constructive criticism	1
Q. Collaborate with those who criticised the work	2
R. Try to discuss the issues openly	1
S. Update the paper	1
T. Be confident in the work you do	5
U. Negotiate with or resubmit to the same agency when resubmittal was allowed	—
V. Change the research methods or materials slightly	—
W. Repeat a publication	—
X. Pay attention to reprint requests	1

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from innovative agencies, keep publishing after the initial breakthrough, submit to other journals, and give lectures. The excerpt below illustrates the strategies used by scientists to overcome resistance:

> I acted as a missionary! In the middle 1970s, I gave seminars at universities, got invited to education sessions at hematology meetings both national and a few international meetings (Participant 79).

From the list of recommendations suggested by authors of highly cited articles we can surmise that an active researcher needs to be an effective communicator. It is not enough to obtain relevant findings; the scientist must also be skilled in "selling" them. The first audience the researcher needs to convince consists of participants in the science communication system, i.e., journal editors and peer reviewers. Persistence is a useful virtue to have when a new proposal or theory challenges or raises questions about the dominant paradigm. Fortunately the opportunities to communicate new proposals and contributions are many, and include congresses, lectures, review articles, and the Internet.

In recent years there has been increasing awareness of the problems associated with peer review. For example, when the winners of the Nobel Prizes are announced, it is not uncommon for journals such as *Nature* or the Nobel Foundation itself to note the problems winners had to overcome in order to communicate research findings that, years later, merited science's highest honor. There has been a gradual realization that a successful scientist needs, in addition to skills in research, unflagging patience and persistence in the face of scepticism.

Conclusions

This study is limited to the personal accounts of scientists, so it is hard to draw generalizations. In addition, we used self-reports as data. We have to rely on the perceptions of the scientists based on some of their experiences.

The episodes reported above provide insights into some behaviors and patterns, and shed light on the processes of evaluation and validation of new discoveries. Because our subject was resistance to new discoveries reported in highly influential or highly cited articles, the results may be especially relevant to the understanding of the dynamics of change in scientific beliefs, theories, and paradigms. Some common views on the development and construction of scientific knowledge emphasize the process of discovery over the process of scientific communication. However, once scientists have obtained results they consider relevant, they may face an even tougher challenge: convincing their peers that their findings are valuable, relevant, and important.

Editors of academic journals argue that they are obliged to reject many papers because they contain mistakes, are not of sufficient quality or novelty, or simply because journal space is scarce. They also wish to protect readers from bad science (Campanario & Martin, 2003). However, sometimes problems arise because novel or interesting discoveries are rejected.

In this study we have classified the reasons that led to initial rejection by referees and editors of scientific articles later shown to be influential or highly relevant. This resistance occurred because some contributions challenged current views or theories. Scientists had to argue with editors of scientific journals or had to send their manuscripts to other journals—strategies that are common in science. Throughout the publication process, the scientists' perseverance was a fundamental factor in ensuring the new findings were communicated to their peers. This fact contrasts with some naïve views on the process of construction of scientific knowledge.

We documented new instances of delayed recognition, that is, discoveries that initially go unnoticed but some years later attract attention. In some instances it took about 10 years or more for the work to be fully appreciated by the research community.

To avoid the problems documented above, we need new channels for communicating alternative theories and views. In Economics, for example, some journals, such as *International Journal of Forecasting*, publish papers that challenge common practices and beliefs. Scientists who challenge dominant paradigms often use alternative journals that, curiously enough, use also the peer review system (Campanario & Martin, 2004). Among these alternative journals are the *Journal of Scientific Exploration*, *Medical Hypotheses* (Horrobin, 1990), and the *Electronic Journal of Mathematics and Physics* (Elitzur, 2002).

Elsewhere we have suggested new approaches to reduce the risk of rejecting important or innovative work (Campanario, 1997, 2002a, 2002b; Campanario & Martin, 2003; Campanario & Acedo, 2004). One alternative is the creation of a central facility (Metajournal) in each discipline. Authors of scientific articles would submit an abstract to this facility. Editors and referees ("journal scouts") would routinely scan the Metajournal to locate good manuscripts, and would then contact authors about publishing the manuscripts. Another possibility might be to devote a page of every issue of a journal to a list of authors and manuscript titles that have been recently rejected, and to provide a full URL address for the journal's website, from which an electronic version of the rejected manuscript can be downloaded. The rejected manuscripts would be labelled as "rejected but available." Readers interested in these manuscripts could download them and judge them on their own (Campanario & Martin, 2003). However, it is possible that many authors would not agree to this new approach. A further possibility is for editors of scientific journals to automatically submit rejected papers to the Metajournal (Campanario & Acedo, 2004). Then, only the more relevant or important ones would successfully undergo peer review.

Scientists' training usually focuses on aspects related to research work per se. In recent years other facets such as research ethics, communication skills, and the ability to convince colleagues of the worth of new contributions and proposals have received increasing attention. We believe

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knowledge of the personal accounts of scientists who authored relevant, highly cited articles can contribute to the training of young researchers, students, and the general public (Campanario, 2002a). Moreover, many young scientists may become discouraged and feel their contributions are worthless if their manuscripts are rejected by prestigious journals. They may find encouragement in the experiences of prestigious scientists who had to fight scepticism by reviewers, editors, and the rest of their scientific community.

As shown by the instances analysed here, resistance to new discoveries can affect even great scientists and very influential, highly cited papers. But not all initial reactions were negative, and some scientists experienced no problems with other scientists, journal editors or reviewers.

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References

Barber, B. (1961). Resistance by scientists to scientific discovery. Science, 134, 596–602.

Berman, R.M., Capiello, A., Anand, A., Oren, D.A., Heninger, G.R., Charney, D.S., et al. (2000). Antidepressant effects of ketamine in depressed patients. Biological Psychiatry, 47(4), 351–354.

Campanario, J.M. (1993). Consolation for the scientist: Sometimes it is hard to publish papers that are later highly-cited. Social Studies of Science, 23(2), 342–362.

Campanario, J.M. (1995). Commentary: On influential books and journal articles initially rejected because negative referees' evaluations. Science Communication, 16(3), 304–325.

Campanario, J.M. (1996). Have referees rejected some of the most-cited articles of all times? Journal of the American Society for Information Science, 47(4), 302–310.

Campanario, J.M. (1997). The 'journal scout'. The Scientist, 11(10), 9.

Campanario, J.M. (1998a). Peer review for journals as it stands today-Part 1. Science Communication, 19(3), 181–211.

Campanario, J.M. (1998b). Peer review for journals as it stands today-Part 2. Science Communication, 19(4), 277–306.

Campanario, J.M. (2002a). The parallelism between scientists' and students' resistance to new scientific ideas. International Journal of Science Education, 24(10), 1095–1110.

Campanario, J.M. (2002b). A new approach to make scientific journals actively compete for good manuscripts. European Science Editing, 28(3), 78–79.

Campanario, J.M. (2007). Rejecting Nobel Class Papers (in revision). Retrieved from http://www.uah.es/otrosweb/jmc

Campanario, J.M., & Acedo, E. (2004, June). Advantages of two new approaches for scientific e-publishing. Paper presented at the ICCC 8th International Conference on Electronic Publishing, Brasilia, Brasil.

Campanario, J.M., & Martin, B. (2003). Rejected but available. European Science Editing, 29(3), 73.

Campanario, J.M., & Martin, B. (2004). Challenging dominant Physics paradigms. Journal of Scientific Exploration, 18(3), 421–438.

Elitzur, A.C. (2002). Launching a new journal, Electronic Journal of Mathematics and Physics, 1(1), 8–11. Retrieved January 18, 2007, from http://www.emis.ams.org/journals/EJMAPS/volume_1_1/editorials/E4.htm

Garfield, E. (1989a). Delayed recognition in scientific discovery: Citation frequency analyses aids the search for case histories. Current Contents, 23, 3–9. Retrieved from http://www.garfield.library.upenn.edu

Garfield, E. (1989b). More delayed recognition. Part 1. Examples from the genetics of color blindness, the entropy of short-term memory, phosphoinositides, and polymer rheology. Current Contents, 38, 3–8. Retrieved from http://www.garfield.library.upenn.edu

Garfield, E. (1990). More delayed recognition. Part 2. From inhibin to scanning electron microscopy. Current Contents, 9, 3–9. Retrieved from http://www.garfield.library.upenn.edu

Horrobin, D.F. (1990). The philosophical basis of peer review and the suppression of innovation, Journal of the American Medical Association, 263(1010), 1438–1441.

Hook, E.B. (2002) Prematurity in Scientific Discovery: On resistance and neglect. Berkeley, CA: University of California Press.

Koelsch, C.F. (1957). Syntheses with triarylvinylmagnesium bromides, α,γ-bisdiphenylene-β-phenylallyl, a stable free radical, Journal of the American Chemical Society, 79(16), 4439–4441.

Laughlin, R.B. (1998). Autobiography. Retrieved January 18, 2007, from http://nobelprize.org/nobel_prizes/physics/laureates/1998/laughlin-autobio.html

Lock, S. (1985). Letter to P.B.S. Fowler, British Medical Journal, 290(6481), 1560.

Martin, B. (1996). Confronting the Experts. Albany, NY: State University of New York Press.

Martin, B. (1998). Strategies for dissenting scientists. Journal of Scientific Exploration, 12(4), 605–616.

Martin, B. (1999a). Suppression of dissent in science. Research in Social Problems and Public Policy, 7, 105–135.

Martin, B. (1999b). Suppressing research data: methods, context, accountability, and responses. Accountability in Research, 6, 333–372.

Nissani, M. (1995). The plight of the obscure innovator in science: A few reflections on Campanario's note. Social Studies of Science, 25(1), 165–183.

Popper, K. (1959). The logic of scientific discovery. London: Routledge.

Simon, R.J., Bakanic, V., & McPhail, C. 1986. Who complains to journal editors and what happens. Sociological Inquiry, 56, 259–271.

Sommer, T.J. (2001). Suppression of scientific research: Bahramdipity and Nulltiple Scientific Discoveries. Science and Engineering Ethics, 7(1), 77–104.

Stent, G.S. (1972). Prematurity and uniqueness in scientific discovery. Scientific American, 227, 84–93.

Stent, G.S. (2002). Prematurity in scientific discovery. In E.B. Hook (Ed.), Prematurity in Scientific Discovery. On resistance and neglect (pp. 22–36). Berkeley, CA: University of California Press.

Wilson, J.D. (1978). Peer review and publication. Journal of Clinical Investigation, 61(6), 1697–1701.

Appendix

Letter Sent to the Scientists Who Were Surveyed

Dear Professor . . .

I write to enquire about your experiences in overcoming resistance to new ideas in science. I learned about your work via http://www.isihighlycited.com.

As you may be aware, some important discoveries in science were initially resisted by peers who did not appreciate their significance. In other instances, referees and editors rejected articles that heralded new ideas or singular or unexpected discoveries.

In previous studies, I identified examples of influential and/or highly cited papers that were initially rejected by one or more scientific journals. In some instances, the work reported in such papers eventually earned the Nobel Prize for their authors. In collaboration with Dr. Brian Martin, I have also collected testimonies and insights from other scientists, especially those who challenge dominant paradigms. Citations to previous work are given below.

I am interested in your recollections of obstacles (if any) that you might have encountered from editors, referees, peers or others to the work you consider most important. I am also interested in cases of "delayed recognition," namely contributions not fully appreciated by the scientific community at the moment of their announcement or publication.

If you are willing to assist, please respond to the 3 questions at the bottom of this letter. If I anticipate using any part of your response in a publication, I will send you a draft for comment so that you can check the wording and context. I hope that the results of this study will assist scientists in the future to more effectively deal with resistance to important new work.

You are most welcome to forward this email to others who might be interested or to send me names of others to contact. I also welcome any suggestions you may have about this project. Please feel free to contact me if I can be of assistance.

Sincerely

Dr. Juan Miguel Campanario

Questionnaire

1. Have you ever encountered resistance on part of the editors, referees, peers or others to work that you consider your most significant?

(If yes, please elaborate briefly. Additional data on papers, journals, personal anecdotes, etc., will be of tremendous help.)

2. What was the initial response from the scientific community to your more important contributions?
3. What methods have you used to overcome resistance to your work? I would appreciate any account that you can send.

To jog your memory, you may find it useful to look through the following list of diverse methods for overcoming resistance to new scientific ideas, but please feel free to mention methods not listed here.

3.1. Funding. Innovators often have a hard time obtaining funding from conventional sources. Sometimes they may have funding withdrawn. Here are some methods that have been used to obtain funds.

A. Obtain funding from innovative agencies.
B. Obtain funding from agencies not worried about the innovative aspects (such as the military).
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.

3.2. Publishing and other ways of promoting ideas. Innovators often have a difficult time getting their work published. Submissions may be rejected or subject to significant delay. Major revisions may be required. Even when published, the work may be neglected. Here are some methods to promote new ideas.

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.
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 (to sidestep journal rejections).
I. Publish books (as an alternative to journal publication).
J. Publish directly on the web.
K. Publish paid advertisements.
L. Seek coverage in the mass media.

3.3. Surviving attack. Some innovators come under attack beyond normal criticism of their ideas. For example, their professional integrity may be challenged, malicious rumours may be spread about them, they may be threatened, their submissions or grant applications may be rejected without proper review, their grants may be removed, and their jobs may be put in jeopardy. Here are some methods to survive such attacks.

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

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