

The Martial Art of Scientific Publication

E. N. Parker*

Publication of scientific results in refereed journals is an essential part of the scientific process. It is the final payoff for the obscure labors that compose scientific research. Unfortunately, effective operation of the publication procedure requires simultaneous rational scientific judgement on the part of the author, the referee, and the editor, and the coincidence of all of these cannot be taken for granted on every given occasion. There are times when the working definition of truth is taken to be the consensus of one's scientific intimates: the "good old boys." Anything outside that limited horizon is discomfiting and improper and is to be barred from consideration.

Navigation through these turbid waters can be different and hazardous for one or more of the three parties involved with each paper. Those authors venturing forth for the first time are especially vulnerable, particularly if they carry an unconventional message. They should take heart that they are not the first to set out upon these stormy seas. Those who went before them have faced similar threats and have survived for the most part with little more than temporary bruises.

There are navigational procedures to optimize the chances of a successful conclusion of the voyage if the navigator keeps his head and his courage. That is to say, a scientific career need not be made up of the continuing calamities that beset the voyages of Sinbad the Sailor or Odysseus. Let us see what we can learn from the anecdotal experiences through which my own career has passed.

An Author's Experience

I started my professional scientific voyage a little more than 45 years ago when I submitted my Ph.D. thesis research for publication. The work was in two parts. One was a treatment of the statistical mechanics of an ensemble of self-gravitating interstellar gas clouds, assuming that the individual cloud is a Hamiltonian system—in retrospect, a dubious approximation. The other part pointed out that the fine curved dust striations in the Pleiades can be understood only in terms of photoelectrically charged dust grains tied to an interstellar magnetic field of at least a microgauss—still a sound inference.

As the cynical reader may guess, the first part was published, and the second part was rejected on the grounds that such considerations had no relevance to

astrophysics. So I was interested to see that S. Chandrasekhar and E. Fermi teamed up in 1953 to publish an estimate of the interstellar magnetic field of several microgauss. I included the second part of my thesis in a summary article for *Review of Modern Physics* in 1956. There are often such alternative if one can afford to wait.

In 1958 I submitted for publication my paper showing the simple fact that the tenuous million-degree outer atmosphere of the Sun has no stationary state except slow expansion from a strongly bound quasi-static state near the Sun to supersonic outflow at large distance. This resolved the conflict between the mutually exclusive ideas of Bierman and Chapman, that the Sun emits solar corpuscular radiation in all directions at all times and that the static million-degree corona extends beyond the orbit of Earth, respectively. The two-stream plasma instability, if nothing else, would prevent the penetration of corpuscular radiation through a static corona. The solar wind incorporated both concepts, placing each in its proper perspective. The spiral form of the interplanetary magnetic field followed immediately, along with the modulation of the cosmic ray intensity by the outward sweep of the magnetized wind.

I was gratified to see how things fell together in a natural way. The paper was submitted to the *Astrophysical Journal*, for which S. Chandrasekhar was editor. Chandra sent the paper to two referees. Both pronounced the paper to be absurd. So one day, Chandra came to my office and said something to the effect, "Now see here, Parker. Do you really want to publish this paper? Both referees are authorities in the field and they both say the paper is wrong." I replied that neither referee had offered any substantive criticism and I wanted to publish the paper. After a moment's thought, Chandra said, "Alright, I will publish it." I learned later that Chandra was skeptical about the ideas at that time but, in the absence of any obvious errors, felt that publication was the proper thing.

A more amusing incidence occurred a year later, in 1959. The collisionless shock front based on small-scale plasma oscillations was a new topic, and I worked out a model for the structure of a longitudinal shock transition based on the obvious two-stream plasma instability. The paper was submitted to a well-known journal for publication. Hearing nothing from the journal after 2 months, I phoned the editorial office and was assured that I would soon get a response from the referee, who was an "important and busy man."

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Further inquiries over the next few months received the same brush-off. However, by that time I had realized that my simple shock model did not properly include Landau damping of the plasma waves. Serious modification of the model would be required, if indeed it worked at all. I tossed the paper in the wastebasket. After about 8 months I received a peculiarly worded referee report saying that publication would be acceptable in a suitably brief form, whatever that was supposed to mean.

A couple of months later, a paper by a well-known plasma physicist appeared in the journal with the sole purpose of pointing out the error in my unpublished paper. My paper was referenced as if it were an in-house report. I was flattered that even my unpublished work merited attention in a national journal. The referee, and subsequent author, had 1) done me no significant harm, 2) done himself no good, and 3) demonstrated that he acted in the best interest of neither the author, the journal for which he acted as referee, nor himself. Others were not so fortunate as I, and were significantly burned by the contemporary editorial hanky-panky of that journal. Fortunately, such things are relatively rare.

In 1966 the interstellar, or galactic, magnetic field was an active topic, even if it had been irrelevant in 1951, and some authors were speculating on general galactic field strengths of 5-10 microgauss. I was stimulated to write a paper pointing out that the galactic magnetic field must be forcibly confined to the gaseous disk of the galaxy by the weight of the interstellar gas in the gravitational field of the disk. Given the standard interstellar mean gas density of 1-2 H atoms cm^{-3} and the half thickness of the gaseous disk as about 100 pc, there is an upper limit of about 2 microgauss for the mean magnetic field in the disk. I went on to show that the interstellar gas sitting on the field is unstable to clumping on scales of the order of 1kpc along the field. The instability arises from the interaction of the magnetic field pressure and the cosmic ray pressure with the weight of the interstellar gas. The instability is opposed by the tension in the magnetic field. I submitted the paper to the *Astrophysical Journal*, and I should explain at this point that the editorial office for the *Astrophysical Journal* was about 10 m along the hall from my office.

One day a couple of months later the secretary for the editorial office handed me the referee's report, which had just arrived in the mail. I put down my pencil and read the referee's report, which began with the statement, "I had always thought that Parker was competent, but ...". There followed an entirely negative opinion of my paper, without the slightest substantive criticism. In fact, I was reassured by the inability of so hostile a referee to find anything wrong in the paper, and I was amused by the passion that the topic had aroused. I considered the magnetic field of the galaxy to be an interesting topic, but hardly one to arouse fury. It was obvious I was trespassing on someone's private preserve.

Now Chandra had always made the point that even the worst referee's report contains useful information. Whatever the author's opinion of the intelligence of the referee, the referee's response represents the reaction of an informed reader. Consequently, the author may judge from the report where the paper needs to clarify, expand, or emphasize certain essential points. It was clear to me that several things needed rewording and rearranging, so I spent the next couple of hours penciling in some alterations that I thought would improve the exposition.

I was nearly finished when Chandra came to my office and asked if the secretary had given me the referee's report. I replied in the affirmative. Chandra said that the secretary should not have given me the report because he always read over the reports first and did any editing that he felt necessary before the report went to the author. He felt that the existing report was inexcusably rude and upsetting. I responded that I was too conceited to be upset, and I reminded him of his advice that the worst report is useful, and said I had already made several minor revisions on the basis of the report, which I would put in his hands as soon as the revisions were typed into the paper.

Chandra felt that the affront was not so lightly dismissed and apologized again for the "blunder" on the part of his office. I realized that he did not grasp my view of the incident, so I pointed out that I had published many papers in the *Astrophysical Journal* over the years, and frankly, in retrospect most of them were of minor stature, some even trivial. One has an idea, one explores the idea with calculations, one writes a paper and submits it for publication. Those papers had passed without effort through the refereeing process. I said I also like to believe that three or four of those many papers were decidedly nontrivial. Those papers, without exception, had been blasted by the referees. I then confessed to him that the present paper involved such elementary considerations—the ideas could be explained to a sophomore physics student—that I had begun to feel that the paper bordered on the trivial: the obvious. So it was encouraging to me to see that the referee, who Chandra assured me was an expert on magnetic fields in general and the galactic magnetic field in particular, did not grasp basic concepts put forth in the paper. The report made my day. Chandra asked me if I really meant what I said. I assured him that I was serious. We both had a good laugh, and he published the paper.

However, there are circumstances when there is no easy solution to irrational condemnation by an anonymous referee. The relatively unknown young person starting out on a career may not be acquainted with an editor or any other senior person willing to give advice and assistance.

An Author's Rights

The first thing to remember is that submission of a serious scientific paper to a scientific journal establishes

the author as a citizen of the scientific community with all the rights and privileges thereof. So hold your head high and refused to be executed without a fair trial, in which the evidence against you is clearly presented and you have the opportunity to state your case on an equal level with the referee's indictment. If the referee thinks that your paper is wrong, the referee is obligated to spell out precisely in what way it is wrong. Then read the referee's indictment and think about it for a time. Is there a misunderstanding? If so, you need to rewrite some of your exposition. Or, as sometimes happens, is the paper really in error? If it is, then get off it. Life is too short to waste it pursuing bad science. I have made errors and have withdrawn two or three papers over the years.

Did I tell you about the hot idea I had many years ago about the theory of turbulence? It got as far as submission to journal. Fortunately, I realized before it was too late that the construction I had made was not the whole theory but rather the leading term in a slowly converging infinite series. The world does not need another slowly converging series in which the first term is the only one to be defined. I withdrew the paper and tossed it in the wastebasket, which should be considered the best friend of every practicing scientist, sparing any number of embarrassing situations if properly used.

Not infrequently, the case against your paper consists of a long list of things that your paper does not achieve. This is a standard gimmick for destructive referees, and both authors and editors should spot it immediately. Remember that no paper has to be the final paper written on a subject. Papers are published for whatever progress they contain; they are not to be rejected for what they do not contain. Insist upon that point.

Experiences of Others

There are other circumstances. Some years ago a very capable young theoretician of my acquaintance explored the possible effect of a burst of star formation on the galactic dynamo and generation of the galactic magnetic field. The activity of the massive stars formed in this way can be expected to stir up the gaseous disk of the galaxy, increasing both the turbulent diffusion and the cyclonic effect. So the problem was to solve the linear-dynamo equations with time dependent coefficients. Solution of the equations was not elementary but, after some hard thinking, he devised an elegant analytical scheme that did the job very nicely so that one could see the response of the galactic magnetic field over a wide range of the parameters. He submitted the paper to an appropriate journal and soon received the referee's report. The report noted that the partial differential equations could be solved numerically, in which case the paper would have been only a fraction of the length submitted. The editor added a note stating that in view of the "devastating" criticism of the referee the paper was

rejected for publication. It was obvious that someone felt the paper was trespassing on their turf.

The young man wrote to me and asked what he should do. I advised him to send the paper the *Astrophysical Journal* and to suggest three or four individuals, whose opinions he could respect, as referees. I told him that I would be happy to be included in the list. Suggesting possible expert referees is a proper procedure, pointed out to me by Chandra many years ago. The paper was submitted and soon published. It is a nice piece of work.

In a more recent case a brilliant young man worked out a comprehensive picture of the principal physical effects that make up the geomagnetic substorm, supporting the physics with illustrative calculations and detailed observational information. He also wrote a brief paper outlining the basic ideas and calculations for rapid publication. His paper submitted for rapid publication was stalled for over half a year and finally rejected on the ground that "the mathematics was not sufficiently original." Fortunately, the comprehensive paper was submitted to an editor with more catholic views and appeared in publication soon after the rejection of the brief paper.

The moral is not to be surprised or upset by anything that may come your way. Grit your teeth and begin thinking about alternatives. It is never too early to begin compiling your own collection of anecdotes from the point of view of the author.

The Referee's Task

Now the view from the referee's desk is somewhat different, of course, and I should begin by emphasizing that the conscientious referee plays an essential role in the publication of scientific papers. As an expert, often with long experience, the referee can provide both scientific and historical perspective for the author if needed. The author is well advised to listen carefully. Proper perspective greatly strengthens the presentation of the author's ideas.

When a referee is called upon to review a paper that seems to be in significant error, the referee faces the often difficult and time-consuming obligation of spelling out precisely what the error may be. It is here that the effort required of the referee may vary inversely with the value of the paper. It is necessary for the referee to study the paper carefully to be sure that he understands the viewpoint of the author. It is not sufficient to reject a paper simply because it does not put forth the view of the referees and his cronies. If the referee feels that the results of the paper are already well known, it is essential to state in the report exactly where the results can be found in earlier papers. It is not sufficient to make the common assertion that "the author is unfamiliar with the literature," or that the referee "is not persuaded by the author's arguments."

If there is an incorrect assertion in the physics or a mathematical blunder, or a misinterpretation of obser-

vational or experimental data, it should be spelled out precisely. That is to say, the referee may have a lot of homework to do. The referee also has an obligation to do his homework and submit his report in a timely fashion. The old trick of delaying an unwelcome paper on the excuse that the referee is a very busy and important person is not to be tolerated, and the astute editor should be suspicious that unnecessary delay or lack of precision in criticism may indicate prejudice on the part of the referee.

Refereeing is often no fun at all, but any responsible scientist should be willing to do his share without using the refereeing process as a weapon against intruders. The referee should be sensitive to how he himself would respond to his report if he were in the authors' shoes, remembering that eminence is not a substitute for science. Remembering too, that the author has the right to demand a clear explanation if the report is negative. Needless to say, the seriously flawed paper is relatively rare, and it should be a real pleasure to serve as referee for an interesting and perhaps important advance in scientific knowledge and understanding.

The Referee's Dilemma

Unfortunately, we authors are generally in love with our research results, and there are occasionally situations in which the author cannot handle the wrenching emotions that go with correcting or abandoning a serious error. In such case, what are we referees to go? The editor is well advised to seek a second opinion, of course. But suppose that the error in the paper is of a simple form, something that can be spelled out concisely. If the referee feels that the error may have important consequences, and if the editor is threatening to go ahead with publication in spite of the referee's most eloquent condemnation, I suggest that the referee write a concise note—one paragraph if possible—and submit it for publication immediately following the published paper. That is fair scientific exchange. Both points of view are out in the open with the appropriate authors identified. In fact, it is not unreasonable for an editor to suggest that possibility to an adamant referee for a paper with an equally adamant author.

As with authors, the referees should never be surprised by anything, and it is never too late to compile some of the more amusing things that come out of the refereeing process.

The Editor's Task

It should be recognized by all that the editor faces serious problems, no matter how lofty and remote his position may seem to the author. Most fields of science have developed so broadly that an editor usually cannot hope to do more than be advised by the referee and author. The experienced editor should of course be sensitive to the blocking tactics employed by the occasional referee, enumerated above, as well as appealing expressions of paranoia from the occasional author. The editor

should not assume a priori that the eminent referee is giving better scientific advice than the relatively unknown author, or vice versa. Again, it is my own opinion that when the question of publication occasionally degenerates into a standoff between the author and referee, the best decision for the editor is to publish the paper, with an invitation to the referee to publish a rebuttal. It is the exchange of ideas and articulate controversy that are at the heart of scientific publication and progress.

Francis Johnson once remarked that the worst paper received by an editor is the paper that is "not even wrong": the observation, experimental result, or mathematical calculation to which the author attributes no physical significance, but which is submitted nonetheless for publication. Unfortunately, such papers sometimes slide through the refereeing simply because they give no offense to anyone. The referee should always insist upon some discussion of the implications of the data or calculations presented in the paper. Some authors can be indignant that the referee should be so rude as to ask what it means. On the other hand, many authors subsequently do some hard thinking and come up with interesting conclusions.

It goes without saying that no one should assume the responsibilities of an editor if they feel they cannot maintain an impersonal attitude toward the scientists and scientific issues with which they will deal. In recent times there have been at least two editors of important scientific journals that have felt they were blessed with divine insights on such diverse subjects as cosmology and global warming. They have developed a list of like-minded referees and, I greatly fear, will be viewed badly by the hard, cold eye of history, in spite of the positive things they may have accomplished, as succinctly noted by Mark Anthony in his eulogy of Julius Caesar.

Editors, even more than authors and referees, accumulate a wealth of experience from the volume of publication during their tenure. I suppose an editor is morally obliged to be a little more restrained in recounting anecdotal experiences than referees and authors, and I suppose that editors soon learn not to be surprised or upset by anything. However, I have often wondered if a retired editor of a scientific journal many years later could not write a lengthy and witty autobiographical account of his editorial experiences. We authors and referees must appear a jolly lot.

It is clear that no one should be an editor whose professional position is in any way insecure, for the responsible discharge of editorial duties invariably infuriates the occasional eminent referee or eminent author, or both, yielding angry letters to the editor: "If you do/not publish this paper, I will never referee/submit another paper to your journal." "If you have no respect for my expert opinion...." "Under what rock do you find your referees?" etc. Like the police in the Pirates of Pensance, an editor's lot is not always a happy one. On the other hand, the editor plays the central role in the publication

of scientific results; and a dedicated and courageous editor is absolutely essential to the success of the process.

So, the writing and submission of scientific papers goes on and the refereeing goes on and the editorial decisions go on and the petty squabbles go on, and old-timers recollect anecdotes with which to amuse and edify their young colleagues. Somehow, we manage to carry on even though fragile young careers and firmly established older careers sometimes collide in the fog of human nature. It is better that the collisions be endured than that anyone reduce speed. The fog is here to stay and we should peer through it as best as we can.