

Principles of Editing

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Abstract

During my 29 years (1971-1999) as Editor-in-Chief of *The Astrophysical Journal*, I learned or developed various principles and policies for editing that seem scientifically sound, fair, and effective.

1. The History Since 1970

At his home in Chicago on December 7, 1970, Dr. Subrahmanyan Chandrasekhar invited me to succeed him as Managing Editor (later retitled Editor-in-Chief) of *The Astrophysical Journal*. My immediate reaction was, "But Chandra, I do not have the breadth of knowledge of astronomy that you do." His reply was that although the goal of the past 18 years had been to improve the scientific quality of the Journal, it was then growing so rapidly in size that the next challenge was to organize its production in an efficient way.

At that time Dr. Chandrasekhar was helped by an Associate Editor (Dimitri Mihalas), one Assistant (Jeanette R. Burnett), a Production Manager (Elmars Bilsens) at the University of Chicago Press, and one copy editor (Jeanne Hopkins), for a total of five people. By the end of my editorship, I had a Letters Editor (A. Dalgarno), Deputy Letters Editor (Eugene H. Avrett), 15 Scientific Editors, five assistants in Tucson, and in Chicago a Production Manager, a production staff of seven, and 13 Copy Editors, for a total of 44 people. *The Journal* size grew from 7058 pages in 1970 in a 12.7×18.4 cm text size to 28,084 pages in a 18.5×24.4 cm text size in 1999. The latter is equivalent to 54,293 pages in the smaller size for a factor of 7.69 increase. Thus the pages-to-staff size went from 1412 pages per person in 1970 to 1234 in 1999 (in the smaller page size). However, the material became much more complex.

Why did Chandrasekhar select me to succeed him? It is a little known story. In the 1960s I depended upon *The Journal's* General Indexes to find incompletely remembered papers. They came out after every 25 volumes and were more prompt than the *Mitteilungen der Astronomischen Gesellschaft*. When no General Index appeared after volumes 125, 126, or 127, I phoned Chandrasekhar about them. He said that he did not have the time to produce the manuscript. While the Press staff could generate the Author Index, it took an astronomer to generate the Subject Index. I was prepared for his response and replied "If I produced the manuscript Index, would the Press print it?" (I had already arranged for a half-time secretary, namely Eleanor Biggs, to help me). His reply was affirmative. So a General Index for Vol. 136-145 was produced. Later we produced a General Index for Vol. 146-165. [We also produced the manuscript for *The Astronomical Journal* General Index for 1944-1975.] So Chandrasekhar felt that I was a person with a conscience regarding *The Journal*.

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2. The First Year

I decided that during the first year I would make no major changes until I understood why things were done as they had been. The sole exception was a minor change. In the 1940s when *The Journal* published about 15 papers every six weeks, we tended to read nearly all of them. But by 1970s when *The Journal* published about 35 papers per issue, we scanned the abstracts to determine which papers we would read in full. So we read many more abstracts than papers, but the abstracts were printed in a smaller type font. I questioned the Press about that and learned that it was merely conventional. At my request they willingly started to print the abstracts in the same type size as the text.

3. Selecting Referees

The only time I differed with Chandrasekhar was regarding the selection of some referees. He conscientiously selected an opponent of an author of a controversial manuscript because he was more concerned with the accuracy of published papers than losing good papers. I found that if an opponent was selected, the reviewing never stopped or the paper was never published. Instead, I preferred neutral experts as referees.

I also developed a method in which if an author openly contested another author, I would ask the contested author for his/her comments but not to act as a referee, i.e. a judge regarding publication. Then those signed comments were sent to the new author for a written reply. The two sets of comments were then sent to the neutral referee for his/her use in evaluating the manuscript. Both the contested author and referee were kept informed about the progress of the manuscript.

I remember one author who was (rightfully) upset when a paper appeared in print (not when I was the Editor) that contested his work and he had no prior knowledge of it. Such a procedure wastes time, journal space, the patience of scientists, and leaves the readers confused. The Editor must always scan manuscripts enough to detect criticisms of others – either by name or subtly by content. Then the Editor should apply the above procedure for contesting papers.

Surprisingly, I do not recall rejecting any papers. I believed that the reviewing process should continue until the authors and referees came to an agreement. Either the paper was accepted for publication or the authors saw that there were problems with the paper.

I did not think that a referee should be asked for more than two reviews of a manuscript. If the referee was asked for more reviews, he/she was likely to get tired of it and perhaps allow publication when he/she still had lingering doubts. If an agreement was not reached after two reviews, I consulted a second referee. In that case the second referee received anonymous copies of the previous reviews and the authors' replies. In other words, the second referee was acting as an arbitrator. It had happened in rare cases that three referees were involved. But science in a controversial area can be difficult. I always admired a referee who said, in effect, "I do not believe the authors' results but I cannot prove them wrong so I think that they should have the right to publish their conclusions."

In an American court, the judge would never decide on a case after listening only to the Prosecutor's evidence. The judge would insist on hearing both sides of the case and then letting

the defendant and Prosecutor question each other. In the same way, an Editor should never decide for or against publication of a serious paper after reading only the referee's report. (Editors receive "crank" papers, ones written by authors not well educated and making claims without substantiation. Those are politely rejected without reviews). The Editor should allow the authors to respond to the report. Also, further discussion and interaction may be needed. But I am aware that recently some journal Editors have ruled against publication of serious papers after reading only one referee report without allowing the authors to respond. I am also aware of a paper that was published without a review and that paper proved to be incorrect.

In recent years I have always identified myself as a referee, but I do not recommend that all referees do that because some authors are vindictive. If a referee needs to submit a negative report and, independently then or later, is seeking a grant, promotion, or new position, the rejected author may seek revenge.

By the 1970s it was easy to write letters with an IBM Selectric typewriter and early computer networks. I felt that if a referee spent typically 5-10 hours, and sometimes several days, in reviewing a manuscript, I could spend a few minutes sending him/her a personal short letter showing that I had read the report and give my thanks. Also, I always kept the referee informed of later progress with the paper and whether and how the referee's requests were answered. If a referee or contested author saw a paper published that ignored their requests, they rightfully would be indignant.

We acknowledged every letter, manuscript, or e-mail received. I strongly believed in an open and prompt distribution of information.

4. Journal Ownership

The Journal was started and owned by the University of Chicago Press, a not-for-profit division of the University. But Chandrasekhar thought that it should belong to astronomers and favored a transfer of ownership to the American Astronomical Society (AAS). That was more pertinent when I became its Editor; I was no longer a member of the University of Chicago faculty so the University had no control over what I did. So while Martin Schwarzschild was President of the AAS (1970-1972), the two of them negotiated a transfer of ownership. A difficult issue was what fraction of the reserve fund was appropriate for the Press to give to the Society. I believe that the amount was \$100,000. At a later time a new Press administration sought to make a profit from its 75 journals, but the AAS then chose a different publisher, as Chandrasekhar's fear was well taken.

Another condition of the gift to the Society was that the finances of *The Journal* and the Society should be kept separate. After all, the annual budget of the Society was about \$100,000 and that of *The Journal* was \$500,000 at that time. Chandrasekhar wanted to be sure that the Society did not look upon *The Journal* as a source of income to support the Society. The AAS and Chandrasekhar agreed to that condition.

That led to my only conflict with the AAS. Rightfully, the Society pointed out that having *The Journal* caused it extra expenses for bookkeeping, funding meetings of the Publication Board, etc. The AAS Council proposed that a fixed percentage (6%) of *The Journal's* income be given to the Society for those extra expenses. I worried that in several years that percentage might, by the actions of a later Council, be raised to 10% or 20%, in violation of the ownership agreement. The Council assured me that that would not happen, but

the current Council could not guarantee what future Councils would do. I appealed to Drs. Chandrasekhar and Schwarzschild for help. They decided that a modest amount was reasonable and that should be allowed.

Again because *The Journal* budget was much larger than that of the Society, the AAS Treasurer (Harold Weaver) worried that it might happen that *The Journal* might suffer a sudden financial loss (e.g, if the U.S. government suddenly decided against honoring page charges in NSF grants). In that case the Society would not have the funds to cover a large journal deficit. *The Journal* had always maintained a reserve fund. Dr. Weaver favored 50% of the annual budget and we finally agreed upon 33%. That meant that *The Journal* had to raise ~ \$200,000, whereas I always tried to keep costs for subscriptions and page charges as low as possible to break even.

A main reason why journals have nearly replaced observatory publications for the publication of most papers is that with journals the publisher and readers share in their costs while for observatory publications and other in-house publication the publisher pays for all of the production and distribution costs and the readers receive them free. That does not seem fair. An additional reason is that journal papers are peer-reviewed for accuracy by outside referees while observatory publications had internal reviews, at most, which may not be unbiased or knowledgeable.

5. Technical Improvements

After the first year I initiated changes, partly in response to author requests and partly to reduce costs. One of the first improvements concerned half-tone illustrations. *The Journal* used the usual somewhat-coarse paper stock (partly recycled paper) for paper texts and high-quality glossy stock for half-tones. To include the half-tones at the appropriate places within the papers meant cutting signatures (large sheets are printed with eight or 16 pages and then are folded to form a signature) by hand and hand-inserting them into the signatures for each copy of *The Journal*. That was very expensive. So to save that cost, all the half-tones had been published together at the end of an issue for many years. One observer sarcastically asked “Why do they put the half-tones at the end of the issues? Why don’t they put the equations there?”

But all half-tones do not require high resolution, so starting in 1972 we offered authors the option of half-tones on paper stock within the papers or on glossy paper at the end of the issue. At a later time the Press found a text stock with a much smaller grain but at a lower cost, so good quality half-tones could again be put into the papers.

Before we had the Internet and sources such as Google and Bing to find information, we depended on indexing. Therefore *The Journal* started having volume, annual, and five-year indexes. But where in a Subject Index should one look for similar papers in a narrow topic? Starting in 1973 we added the appropriate subject headings on each paper to guide the readers on where to look for similar papers. The Editors made the first selections of headings but allowed the authors to change those choices.

Also in 1973 we allowed color half-tones for the first time. They were very expensive because the authors had to provide the color-separation negatives, but colors are needed to show certain features in astronomical pictures. For instance, a color picture of the Crab Nebula

clearly shows the red hydrogen-glowing filaments and the blue synchrotron radiation. Now color prints are reasonable in cost, even within papers.

As *The Journal* grew at 13% a year, the space for *The Journal* strained many bookshelves. We did two things. Starting in 1974 we went to larger format pages (see Sec. 1, second paragraph). We also offered subscribers a microfiche edition. That was a precursor to an online format that lasted for a few years.

In most of the 20th century research in astrophysics was limited mostly to Europe, the Americas, and South Africa. As astronomers in developing countries became involved, they wanted access to American and European journals, which were expensive, partly due to their rapidly-growing sizes. Starting in 1987 we allowed libraries in developing countries, which were specified, the opportunity to subscribe at the foreign AAS-member rates, rather than the library rates. Library rates were always more than member rates because their copies served many people.

Before we had Astro-ph, astronomers wanted to know about submitted manuscripts, even before they were reviewed and accepted for publication. Therefore in 1980 I started the “yellow pages” which gave the titles, authors, journals, and dates of submission for manuscripts submitted to the *AJ*, *ApJ* Part 1, Letters, Supplements, etc.). We invited Editors of other astronomical journals to send us their lists of submitted manuscripts for inclusion. The yellow pages lasted until the end of 1999, by which time Cornell University’s Astro-ph was operating (since 1992).

By the 1980s it was obvious that many authors and their teams were publishing their papers in a variety of journals. It was confusing and time-consuming for them to remember that one journal used boldface for section headings while others used regular type or all caps. Similarly for the treatments of indentations, monograph titles and especially the forms of references. Therefore I arranged a meeting in Paris in 1988 of the Editors of *A&A*, *ApJ*, and *MNRAS* to seek more uniformity of styles. The Editor of *AJ* did not attend but agreed in advance to abide by the final format. We easily compromised on style requirements that soon after appeared in all the main astronomical journals.

In the July 10, 1992 issue we added the option of publishing videotapes, which were sent to all subscribers (by airmail abroad). In January 1, 1993 we started publishing CD-ROMs for many data with the promise that when later a better technology (i.e., on-line) became available, the material so published would be transferred.

Starting with the November 10, 1995 issue the Press used a paper stock that was 22% thinner but with the same see-through, making issues that much thinner. Later (1998) the quality of the paper was such that all half-tones could be printed on text stock within the papers.

6. Online Publication

Peter B. Boyce was the Executive Officer of the AAS during 1975-1995. Being well versed in computer technology, he realized that it was possible to publish a journal, such as *The Astrophysical Journal*, online. He obtained a grant from the NSF for \$500,000 to program that. Starting in 1996 *The Astrophysical Journal* became one of the first scientific journals to be published online, as well as in print. By 2015 the printed edition was terminated and all issues were online only.