On the ?joys? of editing *Language*

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§1. Introduction.

Language, the flagship (and until recently the only) journal of the Linguistic Society of America, was founded in 1925, the year after the Linguistic Society itself came into being. The journal's first editor was George Melville Bolling. In 1940 the editorship passed on to Bernard Bloch, who edited it until his death in 1965; he was succeeded by William Bright, the editor from 1966 through 1987. I was the fourth editor, and with the transition from Bright to me the Society decided to change the editor's term from an indefinite-length appointment to a seven-year stint. Accordingly, I was in office 1988-1994. I knew two of my three predecessors – Bernard Bloch was my revered graduate-school teacher and Bill Bright was my mentor in the editing enterprise – and they both took an energetically hands-on approach to the editing process. This meant, to me, that in order not to disgrace myself I had to edit intensively too, fixing not only grammatical infelicities and stylistic howlers but also substantive errors before sending manuscripts to the printer. Like my predecessors, I made all decisions on all manuscripts, and in addition to articles I solicited and edited reviews and book notices.

My seven years as editor involved an immense amount of paper. Those were still the dark ages before submissions, referee reports, and other official journal communications were all handled electronically. All the routine correspondence went into the LSA archives long ago, but copies of most of the interesting non-routine correspondence remain in my personal files: complaints about editorial bias, correspondence with authors who tried to engage in duplicate publication, the three lawsuits threatened by disgruntled book authors who hated the published reviews of their books, correspondence with authors who wouldn't check their data properly, and miscellaneous complaints about this and that. (On very rare occasions I received complimentary correspondence, but that was of interest only to me and will not figure in this essay.) The journal received about 150 article submissions each

year during my editorship; the acceptance rate ranged between 9% and 11%. Most authors accepted negative decisions gracefully, or at least silently, but some didn't. Those were the days of heated intellectual controversies over functionalism vs. formalism, a debate that exacerbated the frayed nerves of some rejected authors. Bob King was a distant observer of some of the agitation in the editor's office during those years and a closer observer of the most dramatic of the threatened lawsuits. This paper is dedicated to him in grateful acknowledgment of his encouraging words then and his friendship then and now.

I will report on some of the lessons I learned in my editing days, focusing on five main areas: referees and referee reports (§2); how to interact effectively with journal editors (§3); how to handle data responsibly (§4); the sin of attempted duplicate publication (§5); and book reviews, book reviewers, and threatened lawsuits (§6). I'll close with a few final remarks (§7).

The main lesson I learned from those seven years was that I am not a born editor. I did my best to keep up with the work and to do a good job, and I'm proud of my average submission-to-decision time of about three and a half months. But the constant pressure of the work was wearing. So was the need to be polite in the face of invective: I used up my entire lifetime stock of tact during my editing years. I also learned, rather to my dismay (because they say that what you're best at is what you love most), that I am very good at writing politely nasty letters.

In the rest of this paper I will occasionally need to mention specific people, but I won't actually identify anyone who appears in a negative light. When I do refer to a specific person, I'll use the name X; but note that in different passages X refers to different people.

§2. On referees and referee reports.

The ideal journal referee is broadly knowledgeable, courteous, prompt, and possessed of excellent judgment. Some referees are ideal; many others have all the qualities except promptness (see Thomason 1990b for detailed discussion and Thomason 1991 for further comments). Requests for referee reports during my editorship went out with the following guidelines:

1. Promptness is crucial. A month is a reasonable length of time to take in writing a report....

2. Please send a report that the editor can use as is. This means making a specific recommendation (e.g. Publish without revision; or, Publish with revision; or, Don't publish). It also means providing reasons for your judgment, so that the editor need not go to the text and become a referee before making a decision. This is as important for positive recommendations as it is for negative ones.

3. Here are the crucial points to consider in reviewing a ms.: originality; cogency of argumentation; clarity (will readers outside its subfield understand it?); appropriateness for *Language*, as opposed to a more specialized journal; content-to-length ratio...; and – last but not least! – what priority should be given to this ms.? Do you recommend publication with enthusiasm, or only if there happens to be a bit of left-over space in an issue?

4. Type your report on plain paper (no letterhead!) and don't sign it; otherwise the editor will have to cut and paste in order to keep you anonymous before sending the report to the author....

5. Remember that your report, in addition to helping [the editor] decide whether or not to publish the ms., should also help the author improve the ms. through revision. Try to criticize without denouncing...

6. Submitted manuscripts are privileged documents. Please do NOT make them available to colleagues or students, or duplicate them for your future reference, unless the author's permission is obtained. Such permission should be requested from the editor's office...

Some referees were so far from the ideal that I kept a list of people never to be consulted again. I remember one report that was so nasty that I had to apologize to the author for sending it to him. Another referee said to publish the paper without revision but gave no reasons, and then complained to me later ("I told you to accept that paper!")

when I rejected it on the basis of a negative and more substantive report from a different referee. One referee sent a reasonable report but then gave the paper to one of her graduate students (without asking permission from me, much less from the author) because the student's dissertation project was on a similar topic. And more than once referees wrote a very kind favorable report but then told me in a cover letter that they actually thought the paper should be rejected but didn't want to be mean by saying so in the report that the author would see.

The vast majority of referees, however, were conscientious and honorable, and they tried hard to help me make the right decision and to give authors helpful advice for revision. Sometimes referees would express concern about the tone of their reports, as in this instance:

> 'This long delayed review is probably unnecessarily grumpy, but I can't spend any more time to introduce mollifacients. I don't know whether it will count as unfair; but I am anything but hostile to the desire to understand developments of these kinds better. The empirical and conceptual background has to be considerably more secure than it is here, though.'

I very rarely saw a report that seemed to me to reflect inappropriate theoretical bias; that happened mainly with submissions in relatively unfashionable theoretical frameworks, where referees sometimes felt that to be negative about a weak ms. that adopted their favored framework would hurt the framework's prospects for future success. Of course I may well have failed to spot instances where a clever and ethically-challenged referee tried to suppress a good paper written in a rival theoretical framework. But I hope, and believe, that there weren't many of those. I tried to avoid being fooled by biased referees by sending submissions to people working in frameworks different from the author's as well as to referees working in the author's framework.

Unsurprisingly, even ethical refereeing sometimes drew complaints from disappointed and disgruntled authors. Often the complaints revolved around the functionalist/formalist divide. (In those days, generative syntax was Government and Binding – GB.) In one memorable week in late 1994 I received two such complaints, from opposite sides of the

divide: one author was aggrieved to find that, in her opinion, the refereeing process for non-generative papers was less careful (by implication, more hostile) than the refereeing process for generative submissions; the other author charged that '*Language* is a strictly non-generative journal'. I resisted the temptation to send each letter to the other author. Earlier, in 1993, the author of a non-generative paper that I had rejected wrote, 'I had hoped that my contribution might help *Language* defend itself against people's oft-voiced disillusionment with the journal on the grounds that it adheres too rigidly to a particular party line'. I responded: 'I do thank you for the thought, but I can assure you that no defense would be effective, or even noticed. One of the first things I learned in this job is that people who think in terms of party lines and biased editors will continue to do so without regard to the facts....since even the most casual inspection of the contents of *Language* should disabuse anyone of the idea that *Language* is a GB journal, it seems very unlikely to me that publication of yet another non-GB paper on morphosyntax would change anyone's mind.'

Not all the complaints about referee reports reflected suspicion of theoretical bias. One author suspected (correctly, but I only found that out too late) that he and a referee for his paper were competitors for the same prestigious job, claiming (incorrectly, I hope) that bias was responsible for the referee's negative evaluation of his submission. Another author was indignant that a referee report referred him to a large area of research without listing specific works that the author should have consulted: "If the reviewer knows of a work that bears on the issues discussed in the paper, but which I have not taken sufficiently into account, his/her job is to tell me what that work is and how it bears on those issues." I responded: 'a referee's sole responsbility is to advise the editor what to do about the ms., and to give enough reasons so that the editor can tell what the referee's judgment is based on. Good referees – including this one – go further and make a conscientious effort to provide more specific criticisms that might help the author with revising.'

But the most common complaint was that referees failed to understand the paper. As one author put it, generalizing from his interpretation of the negative referee reports I had sent him, 'some referees, for whatever reasons, grossly misrepresent the manuscripts that

they are asked to evaluate.' He added that for *Language* there is also a prestige factor, 'which exacerbates professional jealousy....this is added to the problem of the big egos, who are opposed to anything that does not at least mention them in the References, and theoretical bigotry.' My routine response to such complaints is permanently embedded in my memory cells: If the referees have misunderstood your paper, other readers will surely also misunderstand it, and you therefore need to explain yourself more clearly. I refrained from responding to the charge of ego-driven bias in the referees' reactions, since in this case, as in most others, referees did not in fact object to authors' failure to cite the referees' own writings.

The most surprising complaint I received was from a prominent European scholar who objected strenuously to the fact that I had had his paper refereed at all. Here is part of the indignant letter he wrote me after I rejected his paper:

> ' I can still remember (and can read) Bernard Bloch's reaction to an earlier offer: "I am truly delighted to have your paper for LANGUAGE. I have read it with pleasure and have prepared the MS for the printer." Whether your recourse to a referee (or several) is due to your uncertainty or just youth, is your problem. I – having been elected an honorary member of the LSA not very far back – feel too old for such games, and am not prepared to be messed about in this way.'

On second thought, perhaps this vicious postcard that I received in 1993, denouncing referee X, was even more surprising:

> 'Described by his University of Y Medical Center psychiatric clinician as a "manic-depressive who engages consistently in manipulation and deceit, when, that is, he isn't simply confusing his speculations about reality with reality itself," described by his own mother as "evil", X has in the past accused his departmental colleagues of being in conspiracy against him and used this to explain away his history of child abuse, neglect, and abandonment. These are

things that need to be taken into consideration whenever evaluating anything Mr. X says or does.'

The postcard was anonymous, of course. I ignored it.

Referees who took months to send reports were for me (as for many other editors) the most frustrating part of the editing job. Nagging had to be done delicately, because if I annoyed a referee, s/he might take out the annoyance on the innocent author. Occasionally I had to decide on a submission on the basis of a single referee report because a promised and hoped-for second report never arrived, and occasionally, if the manuscript was anywhere near my own areas of expertise, I became an anonymous referee myself to provide a referee report to base my decision on. Once, after waiting more than four months for a report, I called the non-performing referee and asked if he had read the paper; when he admitted that he had read it, I interviewed him at some length to extract comments that I then turned into a report. And on one memorable occasion a referee who was very late getting a report to me was also waiting to hear news about a paper he himself had submitted to the journal three months earlier. When I emailed him to ask when he could send me his report, he responded, 'I have finished my review of the paper...and will send it off as soon as I hear about my paper.' I asked him why he was punishing an innocent author for the fact that one referee for his own paper was slow to do the work; he did not respond (and he also didn't send his report immediately).

Some people were prompt in declining to review a submission because they had no time for the task. That was fine, because it didn't result in major delays in processing the manuscript. Or rather, it was fine if the referee was suffering from a temporary time crunch and was willing to referee manuscripts at other times; my determination to be tactful was most challenged by senior scholars who told me, with no hint of embarrassment, that they were too busy to do professional chores like refereeing. I did not tell them what I thought of people who had benefited from referees' efforts early in their careers but felt no responsibility to perform refereeing duties for the benefit of younger scholars once they were no longer in great need of such advice.

§3. How to deal effectively with journal editors.

This section concerns the author's role in the process of evaluating manuscripts submitted to the journal (see Thomason 1991 for discussion). Of course an author's paper will only be accepted if the editor and especially the referees are enthusiastic about it, and in the ideal world editors and referees will be infallible so that all and only absolutely first-rate and journal-appropriate papers are accepted. Still, an author can do more besides submitting an excellent paper to help make the process run smoothly. Suggestions for appropriate referees in the cover letter sent with a submission are welcome; often I was able to send a ms. to at least one of the suggested referees, and even when I couldn't, the suggestions gave me pointers to suitable referees. Referee suggestions were unhelpful when authors recommended only their friends, colleagues, and former professors, but that too was useful information for the editor. I also regularly honored authors' requests not to send their paper to particular referees – except on one notable occasion when an author anti-recommended an entire disciplinary subfield full of specialists, apparently with the hope of excluding all referees who had expert knowledge of the subject area of her paper.

Authors occasionally sent me reprints of their earlier publications and asked me to send the reprints to the referees along with the submitted ms., informing me that these would help referees understand the author's as-yet-little-known theoretical framework. I had to break the news to them that asking a referee to read one submitted manuscript was reasonable, but asking a referee to read one submitted manuscript plus a set of previously published papers was not.

When I decided on a submission, I wrote a short letter if the decision was Reject (the largest category by far), a longer letter with formatting requirements if the decision was Accept without revision – the rarest of all decisions – and a still longer letter if the decision was Revise and Resubmit. I didn't encourage resubmission unless the prospects for acceptance after revision were good, and I almost never encouraged a second round of revision if the paper still wasn't acceptable after a first revision. (I would of course consider a revision of a paper that got a straight Reject decision, but re-refereeing was always necessary in those cases.)

Where an author could have a major impact on the process was in the cover letter

submitted with a revision after a Revise-and-Resubmit decision, or even after a Reject decision. A detailed explanation of how and where the author responded to the referees' comments and (if any) mine, and of why the author did not make changes in response to other comments, was important in helping me decide whether I needed to consult one or more referees again.

Most authors, however, did not get good news about their submissions. This was painful to me as well as to them, though for the most part I became accustomed to it soon after I began handling manuscripts. I never quite lost the feeling that I would myself be depressed to get negative referee reports and a rejection letter: it took me back to the grim days early in my career, when I had no stable job and my first journal submission was flatly rejected. During my editing years, some authors reacted to rejections with furious denunciations of me, the referees, and the entire process, but most authors didn't. Sometimes an author would appeal the decision, arguing in a lengthy letter that the referees' criticisms were not as damaging as they had appeared to me. Referees do certainly make mistakes; so do editors. My advice in such cases, if it seemed to me that the author might well be right, was to revise the paper to improve the exposition and respond to any useful referee comments and then resubmit it, with a cover letter that detailed the changes and also explained how the original referees were mistaken. If the author requested that the paper be sent to different referees, I did so. In one case where I had rejected a paper with no encouragement to resubmit, the author wrote such an excellent and convincing cover letter when he submitted a revised version that I was able to accept the revision immediately, without re-refereeing.

What doesn't help an author's case is a move made by a nontrivial number of authors whose papers I rejected: the argument that I had misjudged the referees' comments, that they weren't really all that negative; or that, since one of two or three referees had recommended accepting the paper, I should have paid more attention to that one than to the referee(s) who recommended rejection. These complaints were squarely in the area of judgment calls, and my job was to make judgment calls. My response was always the same: I made the best decision I could on the basis of all the evidence I had – the referees'

reports plus my interpretation of those reports with reference to the manuscript itself – and I hoped that my decision was correct. I realized that the referees and I could all be mistaken, and I was sorry if we were, but I hoped that we were not. If a referee made a demonstrable error that I had failed to detect, I was willing to reread the file to see if the error was serious enough to affect the outcome. Otherwise, if a paper seemed to have a reasonable chance of acceptance after revision, I advised the author to revise first and explain referees' mistakes in the cover letter accompanying the resubmission.

Needless to say, denouncing an editor for bias or stupidity or sheer incompetence is also unlikely to help an author's case, though such denunciations sometimes make entertaining reading (as with the prominent European scholar quoted above). One research area that attracted repeated denunciations was a topic I had published on myself, so that my critical view of some of the research in the area was known. I received a total of three submissions in this area. I rejected two of them on the basis of referees' advice; the third paper criticized my own research, so I turned the file over to an associate editor for processing: he chose the referees and made the eventual decision, which was Reject. The authors complained to me, and they complained of my perceived bias and general unfairness to the LSA and in informal public venues (which had limited distribution in those pre-social-media days). Their friends also complained to me. To one complaining friend-of-author I wrote,

> 'As a matter of fact, your impression is dead wrong. I would be delighted to publish some first-rate [research in this area]. It would be my best defense against the more or less continuous attacks by X et al. Meanwhile, keep in mind that *Language* has a rejection rate that hovers around 90%; statistically, it is hardly astonishing if a total of three papers submitted by a particular set of people get rejected. Breadth of coverage is never a valid reason for accepting a paper; scholarly excellence, judged by me to the best of my ability on the basis of referee reports (by referees judged by me to be knowledgeable and objective, again to the best of my ability), is the **only** valid criterion. The idea that people "should be given a hearing" merely because they haven't yet been

given a hearing...is very strange—and it's a view that you are not likely to encounter among editors of good journals.'

It was not only rejected manuscript submissions that drew fire. On one occasion I realized that I was spending so much time editing a prolific reviewer's Book Notices (short 500-word reviews) for style and clarity that it didn't make sense to keep sending him books. So when he next sent me a lengthy list of books he wanted to review, I wrote to him explaining that I would no longer be able to give him high priority for Book Notice books. I tried to be tactful, but clearly I failed. (He wasn't stupid; he understood that 'not high priority' meant no more review books.) He was outraged. He complained to the LSA's Editorial Advisory Committee and, when their response gave him no joy, he wrote to the LSA Executive Committee and then to the LSA Secretariat (addressing the all-female staff of the Secretariat as 'Gentlemen'), requesting that they begin a 'thorough investigation' of 'certain irregularities perpetrated by the Editor of *Language*' and demanding that they 'exert a change in the editorship'. He added, 'Surely it must be as obvious to you as it is to many of us that the membership sees right through all of the Editor's improprieties.' His effort to have me 'exerted' from the editorship was doomed because the Editor of *Language* is elected by the membership, not appointed by the LSA Executive Committee or the staff of the Secretariat. He did not receive any more books for review. (Neither did the other 77 people who appeared on my list of inept reviewers.)

Most reviewers sent acceptable review manuscripts that weren't so hard to edit, and most were also less ready to assume that they had a right to review as many books as they wanted. One frequent reviewer based in the UK sent me this email message in October 1993: 'If you ignore my offer to review more books for you I will either kill myself in despair, turn to alcohol, join the Conservative Party, feed myself to a crocodile, or go on living cheerfully as I do now. YOU MAY NEVER KNOW WHICH OF THESE I CHOOSE: if the guilt doesn't get to you, not knowing certainly will. I'm just crazy enough to choose the last one, but I make no promises. On your head be it.' He got more books to review.

§4. On handling data responsibly.

The editorial headache that motivated my most ambitious 'Editor's Department' column (in *Language* 70/2, 1994) was the problem of data accuracy. Editors of linguistics journals cannot possibly check all the data in all the languages that authors write about, and even the most dedicated referees can't detect errors in data cited from languages they don't know. Whenever possible, an editor will send a submitted manuscript to at least one referee who does know the language that is being analyzed in a paper, but that is all too often impossible: the paper's author might be the only linguist who has analyzed data from the language; there might be several linguists who have have investigated the language but the author thanks all of them for advice in an acknowledgment note, which tends to rule them out as referees; the editor might have no confidence in the judgment of the only eligible referee for a paper on that language; or the paper might cite data from several or many languages, so that the editor would have to send the ms. to five or seven or twelve referees in order to have a chance of having all the data checked (and no editor will do that – it's hard enough to get reports from two or three referees). I was not encouraged by the occasions on which I spotted mistakes in data cited from languages I know something about; those occasions only served to make me aware of all the data I couldn't check for accuracy in countless examples in other manuscripts.

In my column on the topic of responsible data handling, I listed three rules for authors to follow: 'never rely solely on a secondary source if a primary source is available' (1994:411); 'be very thorough and careful' in citing examples (p. 412); and 'say exactly how the data you are citing was collected' (p. 412). My column expanded on these rules to illustrate them and the importance of paying attention to them. I admitted that following the rules is not easy. It is time-consuming to track down primary sources, even though 'primary' in this context means the original published data from a particular language, not native speakers of the language. Being careful and thorough means not cherry-picking two or three pieces of data from a grammar book and generalizing from those to the language as a whole. Sometimes it also means doing the necessary philological work to interpret the unconventional transcriptions in many sources; here I'm thinking especially about the time a phonologist based a theory in part on data from Boas 1911, apparently without realizing that Boas's transcriptions were not phonemic. And of course there were other ramifications.

Referees did often catch errors, and their reactions were not always gentle. Here, for instance, is a comment from one referee's report:

> 'much of the argument...rests on [Language A] data extracted from a problem set in [Linguistics Textbook B]. Here the author has committed the cardinal sin of not going back to the primary sources. In the case of [Language A], the primary sources show the analysis to be hopelessly flawed...all the generalizations...on which the author's analysis is based are false.'

The greatest indignation was expressed by referees, and sometimes by the linguists who had published the sources the author had cited, when an author made up data. Shortly after my column was published one such linguist sent me a letter that contained this passage:

> 'Phonological and morphological data have been taken many times from my dissertation on [Language A]; most of the data extracted from it has been misunderstood or misintepreted and in a few cases examples were invented to fit the writer's theory of this or that. The most flagrant example was a recent article by [X]...published in [prominent journal Y]. She actually corresponded with me while she was writing this article but even so seven of the 15 examples of [Language A] in the article contain errors; some of the errors are irrelevant to the theoretical issues but others are not.'

Of course my column didn't solve the problem of inaccurate data making its way into journal articles. The problem persists, and I doubt that editors are spending much more time now than I did then trying to ensure the accuracy of data they publish. At least there is one partial remedy now that was not available in my editing days: journals can and should make it possible and perhaps even mandatory for authors to post complete datasets on the journal website so that readers can check the data for themselves. This might help make authors more careful about data handling.

§5. The sin of attempted duplicate publication.

Duplicate publication, or at least attempted duplicate publication, is something that editors have to deal with surprisingly often. It comes in two forms: an author submits a paper that has already been published, entirely or in large part, elsewhere; or an author submits the same manuscript to several journals at once. Authors submitting to *Language* had no real excuse for doing either of these things, because the acknowledgment form that my office sent to every submitter contained the following passage:

> 'In accepting your work for refereeing, we assume that you have entered into no other arrangements for publication of the paper or significant parts of it, and that neither the paper nor a significant part of it is currently being considered for publication elsewhere. If this assumption is incorrect, we expect you to notify us at once.'

Language was not unusual in this respect; other journal editors felt, and no doubt still feel, very strongly indeed about duplicate submission. At least one editor I discussed this problem with in the 1990s said that she would not consider any other submission from an author who had violated this expectation. My own reaction varied. If the author seemed to me to be young and merely ignorant (and too careless to read the acknowledgment form I had sent), I did nothing worse than write a letter saying that I had learned that s/he had submitted the same paper to another journal, and that I had therefore withdrawn it from consideration for publication in *Language*.

But some very senior authors who surely knew better submitted papers to *Language* that had already appeared elsewhere. On one occasion I discovered the duplication when a review book arrived in my office and, looking through it, I noticed a chapter that closely resembled a paper currently being refereed for the journal. When I compared the chapter to the article submission, I found that they were virtually identical. More often I discovered duplicate submission when referees to whom I had sent a manuscript wrote to say that they had just received the same manuscript from another journal.

Not all authors suffered in silence my stern letter about withdrawing the ms. from consideration. One prominent author insisted that the *Language* submission was substantively very different from the earlier publication that my referee had alerted me to;

but when I compared the submission with the published publication, I couldn't see any significant difference. I learned later, from a linguist who knew that author well, that that particular author sincerely believed that minor tweaks to a previous theoretical position made a paper totally different, in spite of the fact that the differences in wording were few and (to everyone but the author) relatively trivial. That author was therefore almost certainly innocent of intending to commit duplicate publication, but I did not regret withdrawing the paper.

On one notable occasion an author submitted a paper that I rejected after getting two very negative referee reports. The author submitted a revised version a few months later, and this time the referees were more enthusiastic, so I decided on Revise and resubmit. Almost a year later the author submitted a second revision, and I was about to accept it when one of the referees alerted me to the fact that the **first** version of the paper, the original *Language* submission, had just appeared in another journal. So I rejected the revised version. In content, the published version and the second revision were essentially the same paper except for the conclusions: the author had, as I recall, simply reversed his conclusion in the last version he sent me. I was very puzzled as well as very annoyed: why would anyone want to publish twice on the same topic, with the same data and analysis except for the very different and incompatible conclusions? The author, however, apparently did not find this odd at all, and he filed an outraged complaint with the LSA about my unreasonable behavior in rejecting his paper.

I should add that I did, and do, understand authors' frustration with editorial processes being glacially slow, especially when an author is untenured and in a high-pressure 'publish or perish' situation where multiple publications are required for a favorable hiring or tenure decision. But journal editors have no choice but to stand firm on duplicate submission and duplicate publication. One reason is that if the journal holds the copyright on the articles that appear in it (as the Linguistic Society did in my editing years), publishing an article that has been published elsewhere risks violating another publisher's legal rights to the article.

Another reason, specifically for duplicate submission to more than one journal at a

time, is that editors rely absolutely on the good will and diligence of referees, who do their work without any kind of tangible reward, simply because they want to be good citizens and contribute to the health of the discipline. Referees have many demands on their time, and – understandably – they won't sacrifice precious time to do often thankless and usually uninspiring refereeing chores unless they believe that their work will be useful to the profession. But if a referee spends an entire day or more working on a manuscript for a journal, and later discovers that all that work was wasted because the author had also submitted the paper elsewhere and had decided to publish elsewhere, that referee is likely to be reluctant to agree to referee anything for that journal again (and maybe for any other journal as well). Scholarly journals can operate successfully only if good scholars continue to be willing to give up some of their time to refereeing; anything that undermines that willingness is very bad indeed for the whole profession. If there were an unlimited supply of good referees, maybe it wouldn't matter so much if some of them became disaffected and stopped performing this vital service. But good, reliable referees are all too rare. No editor can risk alienating them. In other words, authors' convenience in the matter of submitting papers to journals has to be sacrificed for the sake of the profession at large. Recognition of the author's dilemma was the main reason I tried so hard to keep authors' waiting time to a minimum.

§6. Book reviews, book reviewers. and threatened lawsuits.

The most unexpected part of my editing job turned out to be the wild drama of the book review section. Nothing else came close to the passions aroused by reviews – testy reviewers, testy would-be reviewers, enraged book authors and editors, and even, on occasion, furious publishers. Nobody objected to positive reviews; it was the negative reviews that caused all the uproar. Those, and also the people who objected strenuously about the fact that some books they cared about didn't get reviewed at all. I wasn't surprised to be criticized when a review was less than excellent, but I definitely did not expect to be threatened with lawsuits over negative book reviews. I also didn't expect to have to do battle with offended authors, publishers, and miscellaneous readers when *Language* failed to publish a review of some particular book.

And yet the process would seem at first glance to be straightforward: a publisher sends a review book to the editorial office; I look through the book to get an idea of what linguists would be suitable reviewers; I send off a request to one of them for a review, repeating as necessary until someone agrees to write the review. I send the book to the reviewer and eventually, if I'm lucky, a review will appear on my desk, get edited and sent off to the printer, and appear in the journal. Most book reviews followed this path to publication, though a shocking number of people agreed to review a book but never produced a review – and didn't return the book either. It was the exceptions to the normal handling of books and book reviews that caused all the trouble.

There was the publisher who, inspired (I suspect) by an unreviewed author, expressed skepticism when I told her that apparently two of that author's books had not reached my office during the editorial transition from Bill Bright to me. It was strange, she wrote, that it always seemed to be the best books that were "lost" in transit (the shudder quotes were hers). I responded by assuring her that no one in my office was stealing books, and I was confident that no one in Bill Bright's office had been stealing books either. I resisted the urge to say that if we were going to start stealing books, we would not begin with that particular author's books.

There was an author's friend who wrote to express his suspicion that, given my own contrary theoretical stance on a specific issue, I was deliberately suppressing his friend's book on a closely-related issue by pretending that I had never received a review copy. In that case the review copy had in fact arrived in my office and had been sent to a reviewer; but that reviewer, like too many others, didn't send a review and didn't return the book. Luckily for me, by the time I received the protest letter I had already found someone else (who had her own copy of the book) who agreed to review it, and I had actually received the review, and it was in press in the next issue of the journal.

There was the indignant LSA member who wrote to complain that a book he had asked to review had been assigned to another reviewer, although he was the most qualified person in the world to review it, with multiple NSF grants on the topic of the book. But his request had arrived on my desk after I'd sent the book to the other person, and I

hadn't know about those NSF grants.

And then there was the LSA member who was very unhappy about the fact that *Language* had published no review of a three-volume Russian work that he considered enormously important. He urged me to commission a review; I pointed out that I had no review copies of the books; he offered to send photocopies of the books to the reviewer. I wasn't enthusiastic – it had been several years since the last volume was published, and it seemed late to be publishing a review. Reviewer expertise was a potential problem because the three volumes covered a huge number of languages as well as raising methodological issues, and of course the reviewer would have to be able to read Russian easily. The complaining LSA member offered to co-author a review with a distinguished scholar who had most of the necessary expertise; but that scholar never responded to my request, so I dropped the issue. The review proponent disliked my decision to drop it so much that he complained to the LSA and then, when that didn't lead to a review, he circulated an open letter, by email and on LINGUIST list, asking linguists to join him in demanding that the books be reviewed. By this time 1994 and my term in office were ending, so he transferred his demands to the incoming editor, Mark Aronoff. At first Mark was willing to consider a review, but while discussions were under way, the open letter, which charged both me and Mark with bad behavior in the matter, had appeared on LINGUIST List. Mark was not pleased to be denounced even as he was trying to arrange with the review proponent to have the books reviewed, and he ended the discussion. The books were not reviewed in *Language*.

But all these things paled by comparison to the threatened lawsuits. There were three of those: one by the author of a monograph (who threatened to sue the reviewer), one by the author of a textbook (who threatened to sue me and the reviewer), and one by the editor of a collection of papers (who threatened to sue me).

The author whose lawsuit threat targeted the reviewer rather than me had written a monograph on a topic specialized enough that I initially put it on the Book Notice list, which I circulated regularly to LSA members, inviting them to volunteer to write Book Notices. The person who volunteered to write on this monograph was a graduate student.

After looking at the book more closely, I thought it might deserve a full review rather than just a brief Book Notice, so I upgraded it after the volunteer reviewer agreed and told him that he should have a faculty advisor supervise his work on the review. (A few other graduate students had written full book reviews, so although this was somewhat unusual, it was not a departure from the journal's norms.) When the review arrived in my office I saw that it was quite negative; but it was well within the bounds of normal critical reviews, so, although there were some problems with the writing style, I published it. The book author's reaction was swift and sharp. A student should not have been allowed to review his book, no faculty advisor had supervised the work (this was false), there were mistakes in the review, and the reviewer had misunderstood the author's main arguments. At this point I went and got the book from the university's library and read it. Indeed, the reviewer had made some errors, and I could understand the author's frustration about those. But my main reaction to the book was that the author was lucky that I had not reviewed it: I would have been at least as critical, and I probably wouldn't have made mistakes. So I didn't change my opinion that, though flawed, the review was a reasonable assessment of the book. Eventually the author subsided without suing the reviewer, which was good, but I did not forget the bullying legal threat (made by a tenured professor against a graduate student, over a negative review!) that marked the author in my mind as a shabby sort of person.

The textbook review that generated a lawsuit threat – this time against me as well as the reviewer – was written by a well-known expert in the relevant subfield. This review too was very critical (oddly enough, no one threatened any lawsuits over favorable reviews). But that's not what infuriated the book's author, or at least it wasn't the main thing. The reviewer had stopped just short of accusing the author of plagiarism, referring to 'scholarly improprieties' because of overuse of secondary sources. The reviewer was concerned about this assessment when he submitted the review, so he had sent me examples to support the charge and he also offered further documentation if I needed it. It wasn't as if the entire book, or even an entire chapter, was lifted from secondary sources without proper citation; instead, most of the examples I checked were less than a page long and scattered here and there. It looked to me as if the textbook author had been careless about citing sources

rather than deliberately quoting the sources without attribution. Nevertheless, I found the reviewer's charge justified, and I published the review. The author reacted with outrage and threatened to sue over the thinly-veiled accusation. A more interesting response came from the head of the publisher's advisory committee, who assured me that the committee was not planning to sue me but that, since the charge was obviously unfounded, perhaps I should do something to correct the unfortunate situation. This annoyed me enough to send me to the library to get the textbook and one of the books the textbook author had made extensive use of. I compared the two, spot-checking passages rather than reading both books straight through, and easily found several other places in which the textbook author had used the other author's exact words without citing the source. My reply to the advisory committee head included two of the examples, and I assured him that I understood the advisory committee's concern, since what was at issue was material to which other publishers held the copyright. He had ended his letter with an avuncular anecdote from early in his career, about a kind and tactful editor who had advised him to tone down the harshness in a book review. In my reply I agreed with him about maintaining a courteous tone in reviews and added, 'Like other editors who try to be responsible, I am in the habit of urging moderation on reviewers whose criticisms seem to me to be unjustified, ill-supported, or excessive.' He then wrote to say that as far as he was concerned, the matter was closed.

The third lawsuit threat was the most serious, because it escalated to numerous letters and phone calls from the aggrieved book editor and several of his associates and, eventually, from a lawyer he had consulted. It also came close to ending in my resignation from the editorship. The issue was not so much that the book was reviewed negatively, although presumably the book's editor would not have complained if the review had been favorable. The real issue was that the editor and his associates and his lawyer claimed that the review was written under a pseudonym by X, a professional enemy of the editor's. This charge seemed quite bizarre, and it took some time – too much time – before I realized, to my dismay, that it was in fact justified. By that time I had learned that emotions ran high in the subfield of linguistics reflected in the edited volume, with long-standing controversies and personal resentments. I thought (and still think) that that subfield was the most

contentious one I had ever encountered in our discipline. When I made the mistake of mentioning that opinion to the editor, he was so enraged that he threatened to sue me for saying it (in addition to threatening to sue me for publishing the offending review and then refusing to denounce X for writing it under a pseudonym).

The review was unsolicited, and the purported reviewer had no institutional affiliation, so I had exercised what I thought was sufficient caution by having the review refereed by a prominent expert in the subfield. The refereeing process was double-blind, as was usual: that is, the reviewer didn't know who the referee was and the referee did not see the reviewer's name. The referee said that it was a fine review and should be published. He did not tell me until much later – much too late to help me avoid all the subsequent trouble – that he had assumed that the review was written by X. As I also discovered too late, at least one and probably all other experts in the small subfield also assumed that the real author was X, not the person whose name was on the review.

Once the charge of fraudulent authorship was brought to my attention I reread the review and noticed that only one paper in the book (a conference proceedings volume) escaped criticism entirely – namely, the paper written by X. Bob King was drawn into some of the many conversations about the whole issue because he too had a paper in the book (and his paper was hardly criticized at all); he offered a sympathetic virtual shoulder for me to virtually cry on when the noise surrounding these events got too loud. The other authors in the book, including the editor himself, were criticized sharply.

My next step was to try to discover whether the charge was justified. In particular, I wanted to find out whether the purported reviewer actually existed. I made a serious effort to do this, eventually sending a relative who lived near the address given by the putative reviewer to go to the address and try to meet him. She did not see him, but she did meet his supposed landlady, who turned out to be X's mother-in-law and who insisted that the supposed reviewer was a real person. That was my final effort: I never did find any evidence that the purported reviewer was a real person. Much later, when the excitement had died down and a reporter from the now-defunct magazine *Lingua Franca* interviewed me about the whole kerfuffle, I explained that I had not had much experience with people

who don't exist, and that I wished I had less.

Meanwhile, the book's editor was still very angry. He sent me a very long unfocused reply to the review, demanding that I publish it in *Language* – the implication was that if I published it he would not sue me. As editor of the Linguistic Society's journal I was insured by the Society; but the Secretariat informed me that insurance for scholarly societies was so precarious that if the LSA had to defend me against a lawsuit, they would lose their insurance. There was gentle, but still evident, pressure to publish the editor's submission. I was prepared to resign from the editorship rather than do that, since I could hardly continue to run a highly selective journal if I published something that could never have passed a peer review successfully. Fortunately, the crisis never quite reached that point. Meanwhile, I tried and failed to get X, the culprit (as I came to think of the person who actually wrote that review), to admit what he had done. Without an admission, I couldn't denounce his action in the journal, because if I did he could sue me, and I had already learned the hard way that some people in this subfield are quick to resort to legal remedies for scholarly grievances. I was very unhappy with everyone involved in this affair, myself included. I was naive, slow to acknowledge that I had unwittingly foisted a fraudulent review on the readers of *Language*, and furious with the book's editor (yes, in spite of his initial justified grievance; for reasons that might occur to the reader, I'm suppressing some details here) and his friends as well as with X. Nevertheless, when I later wrote an Editor's Department column on how to review edited collections of papers (Thomason 1990a), I refrained from warning would-be reviewers not to publish denunciations of their enemies under pseudonyms.

§7. Some final remarks.

Reading about so many complaints, accusations, and threats of lawsuits might make some people wonder why any sane person would take on a job as stressful as editing *Language*. The answer is that there were highlights as well as lowlights, and the expected benefit of doing all that work – meeting interesting linguists and learning about research ideas and results that were entirely new to me – materialized in full. I didn't enjoy the vehement complaints or the attempts by at least five different people to have me fired or at

least censured, but I very much enjoyed encountering good ideas and data analyses. In particular, reading referee reports with the submission in hand was one of the most productive educational experiences of my life. Some of my Editor's Department columns received favorable comment, especially the one about handling data responsibly. (There was also one sentence in another column that enraged a large segment of the community of linguists, but I won't go into that: curious readers can read the offending column in Thomason 1989a, and they can read my apologetic follow-up comments in Thomason 1989b.) The Editor's Department column that I most enjoyed writing was the one debunking the recurrent claim that Noam Chomsky's early works were rejected by the then-editor of *Language*, Bernard Bloch (Thomason 1991). Some of the miscellaneous letters I received were amusing, as were some of the odder article submissions (for instance the 'emanent' translation of the Phaistos Disk); other correspondence was edifying, and I was grateful for those letters.

But the work load was huge and was a constant source of stress. It gave me nightmares; one of them is described in a note I found in my files recently. When I became editor, I was told sternly to keep the page count per volume to no more than 900 pages, i.e. an average of 225 pages per issue. My note, dated May 2, 1992, reads: 'Last night I dreamed that the March issue of *Language* just came out and was 657 pages long. This made me panic: how was I going to get three more issues out this year without going over a volume total of 900 pages?' All in all, then, I was ready and eager to end my stint as editor. In early 1995 I wrote a long email message to my successor, Mark Aronoff, about editor-transition matters such as sending him current submission files, review books, and the William Dwight Whitney desk (which resides in the editor's office). I added a comment about my reaction to the end of my journal-editing career:

> 'I am still waiting to experience some sort of reaction of loss (loss of being in the middle of things, loss of knowledge about what's going on, etc.), but all I'm experiencing is a continuing immense pleasure at the thought of not having to reject any more manuscripts or edit any more papers. People ask if I'm sorry I did the job for seven years: no, but I sure am glad I'm not doing it any more.

Been there, done that.'

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