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Double Effort, Not Double Blind

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DOUBLE EFFORT, NOT DOUBLE BLIND!

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SUMMARY.   Three arguments against double-blind refereeing are added to the list from the recent report of the Ad Hoc Committee on Double-Blind Refereeing. They are

- The expansion of our communication networks and shrinkage of our peer refereeing groups, especially when combined with the lengthy refereeing process, often makes double-blind refereeing practically infeasible;
- Inferential and (timely) applied works are likely to be handled inappropriately in the absence of authors' backgrounds;
- Reviewers' detailed assistance on language and presentation often depends on identifying whether an author is a non-native English speaker.

As a junior author, referee, and associate editor, I also testify and argue that the alleged unfairness to junior authors in the current refereeing system is largely a misperception due to the lack of involvement of junior researchers in the editorial process. Most senior reviewers have made great efforts to help junior authors; we should appreciate such efforts. The real unfairness to authors, especially to junior ones, is the excessive and unnecessary length of the current review process. Double-blind refereeing does not address this problem and could possibly make it even worse. We should redouble our effort to reduce the length of the review process, and one possible way is to combine junior researchers' time and energy with senior (associate) editors' wisdom and experience. I thus suggest that we

- create a rank of Assistant Editor for junior researchers. An Assistant Editor will work with an (senior) Associate Editor as a team. This will also create a formal mechanism for linking junior researchers to senior ones beyond graduate education, a collaboration that will greatly benefit junior researchers' professional careers.

I provide detailed suggestions on how this team system, by invoking a "censoring" mechanism for referees' delays, ensures a 3 to 4 months turn-around time for authors. I further suggest a back-up system for the editor, which guarantees the journal's responses on submissions within 6 months under the worst circumstances. The feasibility of these suggestions is more worthy of investigation than that of double-blind refereeing, because the former at least are targeted at the most serious problem in our current publication system.

Key words and phrases: Assistant editor, Editorial team, Editorial board, peer review, publication bias, refereeing, turn-around time.

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WHAT MOTIVATES THIS ARTICLE

This article contains my reaction to the recent debate on double-blind refereeing for statistical journals, as discussed in the report by the Ad Hoc Committee (AHC) on Double-Blind Refereeing (Cox *et. al.*, 1993) and initiated by the report of the New Researchers' Committee (NRC) of IMS (Altman *et. al.*, 1991). It also collects some thoughts and experiences I have had during the past few years as a junior author, referee, and associate editor. These thoughts and experiences helped me make a specific suggestion that possibly will help to solve the number one problem in our current publication system — the lengthy period of the refereeing process.

Like many, my reaction is strong, as expressed here. The cautiousness of the AHC Report leads me to believe that a more direct exchange is necessary, especially among new researchers. For example, in summarizing different opinions AHC wrote

> Broadly speaking, the majority opinion among those senior enough to have had experience of the editorial process does not favor double-blind refereeing, and the majority opinion among those in possibly disadvantaged groups does favor double-blind refereeing.

This is a good summary, but could be made more explicit — typically, only those who are *unfamiliar* with the editorial process and who *think* they might be discriminated against believe double-blind refereeing can help them. I surely understand the AHC's cautiousness, because discrimination is a sensitive issue, particularly considering the current society's emphases, as noted in Professor van Zwet's (1993) comments to the AHC Report. Incidentally, I found Professor van Zwet's comments highly informative, frank, relevant, and humorous. I wish we could all write that way, and I recommend every junior researcher read it, especially if one is curious about the "inside" of the editorial process. A few points in this article echo some of his comments, from a junior's perspective.

As a junior researcher, from a country where millions of people have sacrificed their lives simply for expressing their views openly, I feel more freedom in expressing my viewpoints, especially when communicating with my fellow junior colleagues, on this sensitive issue. The AHC Report speculated that the NRC's recommendation on double-blind refereeing represented the majority opinion among junior researchers. I would argue that it only represented a common misperception (as NRC admitted; see below). I am confident that few junior researchers would support double-blind refereeing, if only we had more exposure to the editorial process and understood how much help we obtain simply because we are new. I am more confident that most junior researchers would at least like to *first* solve the most serious problem in our current publication system – the excessive length of the review process. My confidence comes from experience; I was fortunate to be exposed to the editorial process shortly after my graduation in 1990, thanks to the trust of the editorial board of *Statistica Sinica*.

I will be as direct as necessary, and hope I do not offend anyone. I also hope my view adds variety to the seniors' collection of juniors' thoughts on double-blind refereeing specifically, and on the editorial process in general.

THE NEED FOR DOUBLE-BLIND REFEREEING IS A MISPERCEPTION

The NRC Report discussed almost all problems faced today by new researchers in statistics, and I believe all new researchers applaud them for such a great effort. I also believe, however, that some of us think NRC's recommendation on a general effort investigating double-blind refereeing was pushed a bit too far by a stereotypical perception, leading to inappropriate allocation of our current attention and resources. In its Rejoinder (Altman *et. al.*, 1992) to Professor Herzberg's (1992) comments on the NRC's recommendation (it is noteworthy how NRC contrasted Professor Herzberg's comments with Dr. Chernick's (1992), which in fact fit in perfectly with my arguments), NRC stated:

> The NRC has not suggested that new or minority researchers have been prejudiced against (sic), but we do note that in other fields such prejudice has been shown to exist. In addition, NRs would be more comfortable if they were sure that their work were (sic) being judged only upon its merit. We encourage anyone who wants to undertake a study of double-blind refereeing in statistics to do so; however, it is unlikely that the outcome would change our recommendations. Much of the value of double-blind refereeing lies in the community perception of fairness.

Reading these statements, several questions naturally come to mind. Why did NRC suggest establishing a committee for a problem that was only implied by studies from other fields, while not suggesting a committee for a widely acknowledged and clearly evident problem in our own field, i.e., the lengthy refereeing process? If the committee's investigation is not going to change NRC's recommendations, why establish a committee at all? Why should our policies be changed to fit a community perception of fairness, if such a perception is based on ignorance? College professors have been accused of being grossly overpaid because they only teach 10 hours a week (e.g., Maeroff, 1993). Should we also reduce our pay to fit this "community perception of fairness" (I only teach 3 hours a week)? As Professor van Zwet (1993) rightly pointed out, one becomes a lifetime slave of society's perceptions if one always tries to fit them.

The NRC Report has also argued that double-blind refereeing is at least not harmful. This is also not true. If not harmful, most senior researchers would have been in favor of it, not only because that would largely render them immune from accusations of discrimination, but also because they would be the likely beneficiaries! While there is little evidence for discrimination against junior researchers, there is abundant evidence that senior researchers have received very harsh treatment simply because of their seniority. Let's examine a few examples.

◇ In his comment on the AHC Report, Professor Carroll (1993) wrote, of his two-year

editorship at *JASA*, "If I had \$1,000 for every report that said something like 'the (senior) author is capable of much better work', I would have a down payment for 100 acres bordering a trout stream in Montana."

◇ A former head of a large statistical institute recently told me during a casual conversation (after a couple of beers), "Don't think we old guys get special treatment! We still have to fight with you guys, and you guys have so many advantages. If we are a bit careless, bang! rejection!" (He then went on to detail a couple of examples).

◇ A senior author, who is a native English writer, used a phrase "a infinite number of imputations ......", and that was viewed by an editor as a sign that the author didn't care too much about the paper (I was a referee for the paper, and I have altered the actual phrase to preserve anonymity). Had the author been a new researcher or non-native English speaker, I am quite sure that it would be viewed simply as a typographical or grammatical error.

◇ I myself have been quite critical in refereeing senior authors' articles, especially from those who are well-respected, because I (and many others) believe that their works are more likely to be influential, and thus should be held to a higher standard (and that they are more capable of achieving such a high standard).

Given so much discrimination against senior authors, why do most of them still not favor double-blind refereeing? An apparent answer is that they believe that it does not work and will only hurt new researchers not readily identifiable as such (e.g., Carroll, 1993). There is also abundant evidence that junior researchers are helped by referees and (associate) editors simply because they are new, as clearly documented in Professor Carroll's (1993) comments. As a member of the editorial board of *Statistica Sinica*, I am aware of the great care *Statistica Sinica* provides to young authors. The editorial board has repeatedly emphasized the importance of such care, and some senior referees for *Statistica Sinica* have provided extraordinary help to young authors.

I urge my fellow junior colleagues to think about who we should listen to: those who really understand the system and care about us, or ourselves, based on stereotypical thinking. In the NRC Report, it stated "...... after extensive discussion, the consensus of the NRC is that the advantages of the double-blind system outweigh the costs, and we recommend that IMS journals evaluate the benefits of adopting such a system." I don't know what was the empirical evidence used during the "extensive discussion", but I have no doubt that NRC's recommendation would be much more forceful had it been based on empirical studies rather than on a seductive and fashionable argument of possible unfairness towards new and minority researchers.

The real unfairness in our current publication system, particularly towards junior researchers, is the widely acknowledged problem of the lengthy refereeing process. As junior researchers waiting for promotion, we all understand the possible consequences of delayed publication on our career developments. The NRC Report did discuss this problem, but

gave it much less space than the issue of double-blind refereeing. The report also made no concrete suggestion for addressing that problem; I surmise that this was one reason NRC was not able to recommend a committee to act on the problem. I also believe double-blind refereeing can make the turn-around time longer; at least no one has argued (and has no reason to believe) that it can reduce the refereeing time. In fact, as I will argue below, the problem of lengthy refereeing time itself will often make double-blind refereeing practically impossible.

FURTHER ARGUMENTS AGAINST DOUBLE-BLIND REFEREEING

The AHC Report listed several arguments for and against double-blind refereeing. A couple of these seem minor or not very relevant (e.g., the second and fifth arguments in the list of "Disadvantages"), as pointed out by some commentators (e.g., Genest, 1993). I would like to add a few more specific arguments on the infeasibility and possible harm of double-blind refereeing.

A. *The large and prompt circulation of technical reports in one's peer group and the increased number of oral presentations, together with the problem of lengthy refereeing time, often make it practically impossible to preserve authors' anonymity.*

Partly due to the slow appearance of journal articles, researchers in statistics now rely heavily on technical reports as well as oral presentations in exchanging ideas, obtaining feedback, and announcing priority (I am puzzled by Professor Genest's (1993) implication on this point). Thanks to the rapid development of computer communication*, we now even send electronic copies of our papers to our peers right after finishing our manuscripts. On the other hand, our possible refereeing groups often become smaller and smaller, as the branches of research become more and more specialized (which is often unfortunate, as Professor van Zwet (1993) portrayed). In addition, associate editors, especially when not very familiar with the topic, often find referees from authors' citations, which further increases the chance that the referees are aware of the authors' papers. As a result, I have often found myself refereeing articles that I had heard of from other sources, at least by the time I sent in my reports. This is particularly true for papers from young researchers, especially fresh Ph.D.s, because they usually try their best to publicize their work by giving many (job) talks and conference presentations. They are also more likely to send their manuscripts to the best known and most active researchers in the field, who are also the core of referee groups.

I urge AHC to take this confounding issue into account when it conducts the experiment on double-blind refereeing. The planned pilot study using *Annals of Statistics* (Benedetti *et. al.*, 1993) seems to be particularly vulnerable to this problem. The turn-around periods

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* Related to this point, I have suggested to *Statistica Sinica* that journal articles, whenever possible, should list authors' email addresses with their affiliations to facilitate post-publication correspondence.

of *Annals of Statistics* are usually long, and its topics are often too specialized to have a large group from which to select referees. In any case, given the expansion of our communication networks and the shrinkage of possible referee groups, I see less sense in investigating double-blind refereeing without first addressing the problem of excessive turn-around time.

B. *Authors' identities typically reveal the authors' perspectives for conducting statistical inferences. Inferential and (timely) applied work are likely to be handled inappropriately in the absence of authors' backgrounds.*

Some have argued that a paper should be judged only according to its content, a criterion that fits common perceptions. It may be so for mathematical and methodological work. But judging an inferential work (that is what we care about most!) is much more complex. We often see different perspectives leading to different methods for analyzing the same set of data and thus it is useful to know the authors' perspectives when making evaluations. As a not-too-unrelated analogy, it helps a great deal in evaluating a philosophical paper to know from which school the author comes, what he has expressed previously, and how his thoughts were developed. Philosophers would surely think we were joking if we were to tell them that their articles should be refereed double-blindly because of the "community perception of fairness". Knowing the authors' perspectives (or absence of perspectives) also helps the referees to make sensible and effective suggestions. It certainly will not be effective for a referee to suggest a Bayesian method to a known hard-line frequentist, even if the Bayesian approach is a better one for the referee.

Another case, in practice, where suggestions should be made according to authors' backgrounds, is when dealing with timely applied work (almost all important applied works are timely), such as a study on the current prevalence of AIDS. Real-life statistical analyses are never perfect, especially under time pressure. Consequently, referees almost always find room for improvement. The referees' suggestions should be made sensibly according to the authors' backgrounds, as long as a minimum standard is upheld. Journals surely like to bring out the best in every paper, but demands exceeding the authors' capabilities (it is easy to think of what should be done when not doing it!) will only turn away timely applied papers or delay them until their analyses become scientifically less relevant — as my (former) advisor Donald Rubin often says, "Science never waits on us." This problem is all the more likely to occur when the authors' backgrounds are unknown to the referees. In some cases, knowing authors' affiliations also helps referees to see the appropriateness of asking authors to bring in new data (I have used authors' affiliations in such a manner in my refereeing work). Some (e.g., Genest, 1993) have said that making recommendations according to authors' backgrounds is exactly what advocates of double-blind refereeing argue against. But I think that this is the only sensible way in this context, unless we want to have our journals full of elegant analysis and reanalyses of data sets that are as old as our text books.

C. *An author's identity often helps to judge whether he is a native English speaker, and how much exposure he has had in English writing. The degree of editorial assistance on*

writing often depends on such a judgment.

Those of us whose native language is not English, especially those who reside in non-English speaking countries (almost all journals accept submissions worldwide), understand our struggles in writing papers in English and our appreciation to reviewers who provide detailed help. I doubt the reviewers would provide the same, kind, detailed assistance, when the authors' identities are uncertain. Some commentators of the AHC Report (e.g., Billard, 1993; Genest, 1993) have argued that aid to presentation could be preserved with double-blind refereeing by adding a second round, or by providing such aid to all authors. I have to think that these suggestions were made purely for the sake of argument; if we have so many complaints about the lengthy refereeing process, the last thing we want to do is to introduce mechanisms that will further increase it! Furthermore, reviewers (especially non-native English speakers) are often reluctant or do not bother to make corrections for native English writers, for that may irritate some of these writers.

The issue of editorial assistance in writing is an increasingly important one, as more and more non-native English speakers are becoming statisticians. We have all seen the recent rapid growth in the number of Chinese students in statistics; of eleven of my classmates from Fudan University who are now in U.S., nine are in statistics. Examining a recent issue of *Annals of Statistics*, or *JASA*, or *Biometrika*, one would be surprised to find how many authors are young non-native English speakers. It is precisely those young non-native English speakers, like myself, who need more help. There is also an issue of how to help authors from undeveloped and developing countries (the editorial board of *Statistica Sinica* has recognized such a need). To blindly give the same treatment would be really unfair to these authors, as we are living in an unequal world. If we really want to be fair, then everything we do should only increase (not in perception, but in action) the amount of help we provide to disadvantaged groups.

MY PERSONAL DATA

A disadvantage for junior people making arguments is that their views are often regarded as based on second-hand experiences. I would thus like to set forth some personal experiences to further express and substantiate my view. During the past three years (through 1993), I have refereed or have been involved in the editorial process for more than 90 manuscripts, so I believe my sample size is not inadequately small. I have also been dealing quite often with reviewers' reports for my own submissions. Of course, as an individual, I am still of sample size one, since this is only my experience and my viewpoint. I do believe, however, that my personal data are more relevant than some "community perceptions".

On This Side

As an author and coauthor, I can report that, both fortunately and unfortunately, I have experienced the whole spectrum of referees' reactions: from swift acceptance without any revision, to complete rejection with "This is BS" (Could it be "Bootstrap"?) marked

6

inside the returned manuscript. The two extreme cases perhaps are very rare, but I mention them to indicate that I am aware of the variety of treatments the authors (not only junior ones) receive. I will return to the "BS" incident, but I first want to detail a more common case, which shows how I was helped greatly by referees.

More than a year ago, I wrote an article on a quite controversial topic, and submitted it to a prestigious journal. I was a bit worried about the fate of my submission, because it was on a topic many people had strong opinions on. Surprisingly, I not only got a quick response, but also a positive and unusually helpful one. Two referees' reports first pointed out some merits of my work that I hadn't pay much attention to, and one even offered his congratulations (how many new researchers would like that!). Then they detailed many comments, from suggestions on notation to questions on philosophical perspectives. I was particularly thankful to the referee who congratulated me (not just because of that!), because he clearly seemed to be an authority on the topic with strong opinions. But he communicated his strong opinions so effectively that they only stimulated my thinking, and did not put me in an uncomfortable defensive position. From his general tone, it was clear that the referee had taken into account that I was a new researcher. Some absence of materials in my original version was attributed to my unfamiliarity of the literature. The same absence, however, could also have been viewed as deliberate suppression, had the paper been judged as having been written by a senior author. I am not sure, in the absence of my identity as a new researcher, that the referees would have provided as kind, detailed, and sensible comments.

Some have argued that giving special care to new researchers is providing avenues for less competent work. I believe again that this is due to their unfamiliarity with the editorial process (a reading of Professor van Zwet's comments could be helpful). Special care only brings out the best of the junior researchers' work and helps them in the long run. Or as Professor Carroll put it, the special care "was in helping the new researcher turn a publishable idea into a publishable paper." In my example mentioned above, referees' sensible comments have enabled me to carry out a successful revision that is more substantial and interesting than the original, even though the original was of publishable quality in my own estimation.

On The Other Side

As a referee, I have already encountered several occasions in which I would have been aware of the authors identities even if the authors' names had been removed from their manuscripts. I would like to report one incident, which indicates how small our peer refereeing groups often are. Once I read a technical report which included a discussion of several unpublished works by a group of authors. In the following several months, I received from three journals requests to referee three papers that were among those mentioned in that report. What amazed me was that these three papers were not closely related, and no work of mine was cited in any of them, so there was no natural link to me as a referee. I could only

attribute that to coincidence, but then I have had other "coincidences". I am interested in knowing how often such "coincidences" occur to others. I heard one of my senior colleagues mention that he had repeatedly rejected the same paper for several journals.

As an associate editor for *Statistica Sinica*, I have had opportunities, particularly because authors often submit previously rejected papers to a new journal, to see many low quality manuscripts, both in terms of research and presentation. When we get a paper rejected, it is convenient and psychologically comfortable for us to blame the rejection on the unfairness of referees and editors, especially if we can classify ourselves into some disadvantaged groups. But have we really examined the quality of our papers before we blame others? What about the quality of our writing and presentation?

Even if we have done breakthrough research, I and my fellow junior researchers are often too inexperienced to present it clearly and attractively. We often either present too much, trying to tell the world everything we have done; or we hope too much, assuming every reader can understand instantly what we have spent years to develop. For those of my fellow junior colleagues who have the opportunity to referee papers, simply ask yourselves how many times you were frustrated because you couldn't follow the papers being refereed, and thus began to react negatively? In fact, the problem of presentation is not only limited to junior researchers, as encountered in my editorial experiences. Shouldn't we all make efforts to improve the quality and readability of our papers before we attribute the reviewers' negative reactions to editorial discriminations?

Cases of discrimination against young authors or any other group obviously can be found (we have found many cases of discrimination against seniors!). Unpleasant and unethical incidents always exist in any large system involving many individuals, and we should prevent them whenever possible. But, as we all know, there is simply no reason to throw away the baby with the bath water! If we are really so concerned about community perception of fairness, then the only safe way is to publish every article submitted to the journal. But few people would think me serious if I were to make such a suggestion, because most of us are not ready to implement a self-discriminating mechanism yet!

Reacting To (Extremely) Negative Review

To minimize the damage that an unpleasant referee's report can cause, we should, as a society, educate authors, especially the junior ones, to read the referee's report effectively. Once again, as Professor van Zwet rightly pointed out, experienced authors know how to turn negative reports to their advantage. I would like to share some of my own experiences in acquiring such skills. My initial reaction for the "BS" case mentioned earlier was frustration and even anger, as were my junior coauthors', and I did think that it was a case of discrimination because we were juniors. After reading more carefully the referee's report, it became clear to us that the referee had the extreme reaction for two reasons: (i) the referee largely didn't get the picture of what we intended, and (ii) the referee clearly had a very strong and different philosophical perspective.

8

When (i) occurs, what we should do as authors is to try our best to clarify what we want to present. Blaming referees is the least useful reaction. Referees are supposedly more capable of reading a paper than most readers, and no matter how little time they have spent, they perhaps still have paid more attention to the paper than most potential readers will. We also have to remember that nearly every referee tries to be insightful, almost as a psychological pre-conditioning when performing a judgmental work. Even if a referee completely misunderstood a paper, it is still an important fact that the paper can be interpreted in such a completely unexpected way! When we publish a paper, it is almost like a manufacturer producing a product — " the customer is always right". Some will strongly resent this notion, and argue that they only write papers for those who can appreciate them. I have no trouble with that argument (some breakthrough works do often proceed much ahead of their time), but I am sure very few journals would generally encourage such an attitude.

When (ii) arises, that is, when a referee has strong reactions because of a different perspective, intellectually, it is almost the best thing that can happen to us as authors. It is not an overstatement that statistics was largely, and still is, advanced by controversies from competing perspectives. When being criticized from a different perspective, it is the best time for us to reevaluate our formulations, reexamine our arguments, and rethink what we are really trying to accomplish. This type of criticism is particularly stimulating to junior researchers, because it often forces us to think about our work more broadly. A revised paper that deals directly with the issues involved in such a referee's comments will be a stronger and more useful one.

There are also two other scenarios that could trigger a referee's strong negative reactions, namely, (iii) stupidity and incompetence on our part, and (iv) making false statements or not acknowledging others' work, intentionally or unintentionally. I guess there is little need to spell out how we authors should respond in these two cases.

Having reacted sensibly to the referee's extreme comments in the "BS" case, we were able to cut the paper by more than 15% and to make it more clear and precise. This is the experience I have had with every single paper I have had authored or coauthored — the reviewers' comments always increase, directly or indirectly and often substantially, the quality of our papers. This is also the reason that I oppose the idea that some referee reports should be withheld or at least edited before sending to authors, as suggested by NRC and others (e.g., Herzberg, 1992). Positive, negative, or even harsh, when read effectively, referees' reports are always helpful and stimulating, especially the negative ones. Besides, referees reports are the true "responses". As statisticians, we almost always try to avoid masked data, why not here? To let authors to see only the smoothed "responses" doesn't mean that the original rough versions no longer exist. It only gives authors a false impression, especially because a referee usually represents a group and a perspective.

When an (associate) editor thinks a report is too harsh or could hurt the authors, what

he needs to do is to help the authors to deal with it rather than withholding or editing it. For example, the editor can indicate which suggestions should be followed and which should be read simply as a piece of information and even encourage the authors to rebut (some current editorial reports do contain such instructions). Writing rebuttals is not easy and often frustrating, especially for new researchers, but I find it is simply my most rewarding professional writing experience. The need for withholding or editing a report, in my view, only arises when the report contains irrelevant personal attacks. I believe such incidents are rare because a referee accomplishes nothing by including such attacks, but only succeeds in appearing foolish and unethical.

ASSISTANT EDITOR AND TEAM WORK

Having said so much about double-blind refereeing, I would now like to turn to what we really should have been addressing — how we can solve the most serious problem in our current publication system, the lengthy review process. Since almost every one of us complains about this problem, we can proceed directly to discussion without first debating why we should make such efforts.

When we are asked to referee or handle a paper, I believe almost every one of us has the intention to finish it as soon as possible. There is simply no (ethical) incentive to delay such a process. What happens next, however, often departs substantially from what we initially hoped. We constantly find ourselves replacing old deadlines with new ones and watching our file piles growing in an (dis)orderly fashion. An apparent reason for such an unfortunate situation is that we always find that other demands, professional and personal, request higher priority than those silent manuscripts. Sometimes, manuscripts are simply forgotten for a time when our minds are being occupied by so many other demands. I do not want get into the issue of how we should assign our priorities, as such a complex issue often results in fruitless debate; Gleser (1986) sheds some light on this issue. What I do want to discuss is how we can find more fellow colleagues to share the editorial work, so each of us can have more flexibility in allocating our time and energy, thus eventually helping to reduce the length of the review process.

While active and senior researchers are "burned out" by various demands, including heavy editorial work, some less active and new researchers are constantly concerned about the lack of involvement in our professional activities, both in terms of research contributions and community services. The first few years after graduation is particularly a period during which many young researchers feel a sense of isolation, when they are largely detached from their former advisors but are still searching for an appropriate mentor to work with at their new positions. This led me to think why couldn't we involve those of us that have more time, energy, and most importantly, more willingness, in our editorial work? Journals understandably ask active and senior researchers to handle submissions. Typically, they are easier to identify and are more aware of the current state-of-art of their areas. But that

does not mean that junior researchers are incapable of editorial work, especially if they can work *with* the more active and senior ones. In fact, many new researchers have not been asked simply because they haven't been given enough time and opportunity to demonstrate their abilities, both in terms of research and community service.

Many people, especially the seniors, have recognized such an unfortunate unbalance, and have been making efforts to change it. For example, as NRC Report noticed that, senior researchers are usually delighted to share referee work with junior researchers and give feedback to juniors' referee reports. The mutual benefits, especially to junior researchers, are too obvious to discuss. Since such team work is so advantageous, why don't we introduce it formally to our editorial system, which will also give these new editorial contributors formal name recognitions they need?

To formally introduce a referee team seems implausible and unnecessary. The NRC and many others have been advocating using new researchers as referees (e.g., Altman *et. al*, 1991; Tweedie, 1992), and I believe many journals are doing that, judging from the referee requests I have received. What I think is needed and entirely feasible is to have new researchers on the editorial board. I think it is advantageous and almost effortless for journals to adopt the following editorial system. The system was partially motivated by my own successful experiences of working with senior (associate) editors.

> ⋆ After appointing associate editors, allow (but not require) each of them to appoint one assistant editor. Each assistant editor will work with the associate editor as a team. Assistant editors are formal members of the editorial board, and should be listed in the journals along with the associate editors and the editor(s).

DETAILS OF IMPLEMENTING THE TEAM SYSTEM

Now I would like to elaborate on what I think is an effective implementation of such a team system. The goals of such a system are, first, to reduce the length of the review process, and second, to involve more new and less active researchers in our editorial system.

How To Select An Assistant Editor

An Associate Editor selects, from his own institution, a junior colleague that does not have any formal editorial position and is not overloaded by other duties (e.g., in charge of a consulting program). Preference should especially be given to those in the *first* year of their *first* professional positions. The associate editor should not be concerned if his appointee has no experience, precisely because the appointee will work with him and learn from the process. He does need to ensure the willingness of the appointee as well as the prospects for constructing a good and efficient working relationship with him. It is also desirable that the appointee's research interest be close to the associate editor's. This will not only ensure an efficient review process, but also will make it more likely that the team will develop research

collaborations. It is, however, not absolutely necessary because many new researchers are eager and able to explore new territories beyond those they have already conquered.

The length of the appointment should be based on a mutual agreement between the associate editor and the appointee, with the understanding that the associate editor can reappoint and the appointee can resign at any stage (a little cooperation is needed at that point to ensure requisite smoothness of the transition). This is completely a voluntary team-work system; if either party feels the team is not functioning properly, it is the time for change. The associate editor can always choose to work alone, just like under the current system. On the other hand, if a senior researcher serves as an associate editor for several different journals, he can appoint a different assistant editor for each journal, if desirable and feasible.

How The Team Should Function

The team should aim for a ten-week turn-around time, from the day of receiving the paper to the day when the team sends the review package back to the journal editor. A ten-week period is still long when compared with many other fields, but may be more realistic at the current stage. With the two to three extra weeks needed by the editor and mailing, this proposal aims for a three-month turn-around time for authors. Those who prefer an even shorter period can easily modify my following suggestions proportionally; individuals should always use whatever is effective for them to implement the team system. Efficiency is what we intend! My suggestions are specific, because I often find general suggestions are not as helpful in practice. A graphic summary of my suggestions is in Figure 1.

During the first week, both editors should screen the paper. The assistant editor should spend more time, since he has and generally needs more time to evaluate the paper. He should also prepare a single sheet checklist for handling the paper. By the end of the week, the two should set up a brief meeting to determine whether further review is warranted. Having two people to discuss the evaluation is also more objective and efficient. The associate editor should always encourage the assistant editor to express his opinion. The associate editor, however, should make the final decision in case of a serious dispute (which I believe will be rare).

If a decision for immediate rejection is reached, then during the second week the assistant editor should draft, based on the meeting discussion, a sensibly worded rejection letter that will be used by the journal editor. The letter should be given to the associate editor by the end of the second week. The associate editor will usually need to modify and polish the letter, and should also indicate to the assistant editor why these changes are necessary. Such feedback will greatly help the assistant editor both in terms of language and writing skills for (unpleasant) editorial letters. After further modifications, the letter should be sent to the editor no later than the end of the third week. The letter should be signed by the associate whenever possible (in the editorial system, letters are often written by somebody

other than the signatory).

If further review is necessary, then it should be decided during the same meeting who the referees will be. Here the associate editor's experience is greatly needed. The current standard norm is to send the paper to one to two referees. I suggest sending to three to four, if feasible, to anticipate some referees' nonresponse by the deadline (the current standard practice is to find alternative referees only when the initial ones do not respond). This is particularly because the system I am suggesting will not wait until all referee's reports come in before making a decision; such waiting is a primary reason for the lengthy review processes. Delayed referee reports can always be sent to authors in a later stage, if the paper was not rejected based on the reports received on time (on several occasions, I received such late reports during revision of my manuscripts).

Once the referees are selected, the assistant editor should take care of all mailing business. The standard letter of referee request should state that the referee is expected to reply within four to six weeks upon receipt, as do many current request letters. It should also state that a reminder will be sent to the referee by the end of the fifth week if no report is received by then. The letter should be signed by the associate editor, with "cc" to the assistant editor. The letter should be sent early in the second week so that it will generally reach the referees by the end of that week (international mailing may take longer).

During the next five weeks (the third to seventh weeks) while waiting for referee reports, the assistant editor should take some time to evaluate the paper more carefully. This is a necessary preparation for writing the summary report after receiving referee reports. It also prepares the assistant editor to serve as a referee himself, if necessary. I would suggest the assistant editor, whenever he has time, write a referee report during that period. Even if it is not used later (it can always be a part of the summary report), practice at writing such a report is important in developing one's writing and refereeing skills. The associate editor should always comment on the assistant editor's writing, to help him to improve. The associate editor should also pass any referee report received during that period on to the assistant editor.

During the seventh week, the assistant editor should briefly report to the associate editor what the team has received by then, and remind the associate editor to read the report(s) and the paper. The assistant editor should send, early in the week, a reminder via email to any referee that has not responded to remind him that his report is due in a week, including a thank-you note to the referee in case his report is already in the mail. Ask the referee to acknowledge the email; this will not only ensure the communication is effective, but also will help to increase the possibility that the referee will send in his report on time. If the referee does not acknowledge after two days, send a follow-up email. The current practice is to remind a referee only after his is far past the deadline; that is too late. In the reminder, the assistant editor should act only as a messenger, reminding the referee on behalf of the associate editor. Such a style is more polite and more effective. The reminder

should also ask the referee to send his report by email or fax. If the referee wants to and is able to respond, one week is almost always enough time for him to prepare his report, so his report will be received by the end of the eighth week. I suggest using email (or fax in case email is impossible) instead of regular mail because email is prompt, easier for the referee to acknowledge, and shows a sense of urgency (it is urgent!). A phone call can also be effective, but it can be regarded as too pushy and some junior researchers (especially non-native English speakers) may be reluctant to call people they don't personally know well. The assistant editor can, however, ask the associate editor to call a referee who does not respond to both emails. Such a phone call can determine whether the referee is reachable and if his report will arrive on time.

At the beginning of the ninth week, the assistant editor should start to prepare a draft of the summary report (and possibly a letter for the editor) based on what the team has received. In the worst case that no referee has responded by then, he should use his own referee report, with input from the associate editor. One referee report plus an editorial report is not uncommon under the current system, and the suggested system can always guarantee that minimum standard. The team should meet at the end of the ninth week to discuss the referee reports, draft summary, and recommendations on the paper. This is a key meeting for the associate editor to provide his opinions and feedback to the assistant editor (the associate editor should have read all the relevant materials by then, as reminded by the assistant editor two weeks in advance). The tenth week is then used for the team to finalize the review package. Once the package is sent, I also suggest that the assistant editor send the editor an email informing him to expect it.

During the ten-week period, the editorial team can always decide to take action before the ninth week if a sufficient number (preferably two) of the referee reports are received earlier. Also, for a revised paper, the time period should be much shorter, even if the revision need to be sent to the referees again, unless the revision is practically a new submission. I suggest at most a six-week turn-around period for revisions, allowing three weeks for referees. Always ask referees to respond by email or fax for comments and suggestions on revisions (typically referees only check if previous suggestions have been incorporated). The collaboration within the team should be similar to that for handling initial submissions, but at a faster pace.

Some Suggestions To The Editor

The suggested team system requires little change from the journal editor, but a few related suggestions are worth mentioning. The suggested system aims for a three-month turn-around time for the author. With an extra month to account for uncontrollable delay (referees' delays have been "censored" by the team system), such as during mailing, it will still be within four months. Some submissions are now handled within four months (excluding those being immediately rejected), but only as the lucky ones, not as the worst ones as under the team system. With the team system, the editor can confidently announce

in the initial acknowledgement that the author should hear from the journal within three to four months. Many current such letters do make similar promises, but many of us often find ourselves to be the "unlucky" ones who have to send inquires, often a couple of times, after the promised dates.

I suggest that the editor send a letter of apology *automatically* to the author by the end of the fourth month, if the journal is not able to reach a decision on the submission by then. The current practice is to send such a letter only when the author pushes. I believe it is much better for the journal to send the letter on its own initiative. Such initiative will not only help to ease the author's anxiety or even frustration, but will also help to build up a friendlier relationship with the author and thus will help to solicit future submissions. I believe this is in the best interest of any journal.

In the letter, I further suggest promising the author that he will definitely hear from the journal within another two months (between the postmarks of the editor's letters), or otherwise the paper will not be rejected (the journal can still ask for revision). Some would say that it is too naive and too dangerous to make such a promise, but I would say that, given the serious nature of the existing problem, it is the time to take dramatic and even painful action! Being forced to publish unqualified papers is severe punishment to the journal, as well as to our professional society as a whole, for not being able to act responsibly on individual submissions. I firmly believe that the possibility of such severe punishment is the incentive we need *now* to move the system! We only need one journal with enough courage to adopt such a policy; others will be forced to follow. Our profession society should establish a norm that no submission can go unanswered beyond six months (I welcome suggestions on more sensible time periods), except when such nonresponses are caused by events out of the control of the editorial board.

To ensure that the journal will be able to respond within six months, I suggest the editor appoint a knowledgeable, experienced, and responsible senior colleague (at the same institution) as a special associate editor. When the editor does not hear from an associate editor within four months, and after contacting the associate editor it is still unclear whether a report will be sent in another two weeks, the editor should ask the special associate editor to handle the manuscript as an emergency case. The special associate editor should serve both as referee and associate editor for that paper, and should report to the editor within a month so that the editor can respond to the author by the sixth-month deadline. The special associate editor should never be asked to handle regular cases, that is, he is only a back-up, a last-minute rescuer. He can appoint his own assistant editor, but only if such a back-up team is more efficient than him alone.

Under the current system, such a back-up system certainly would not work, as the special associate editor would soon be "burned out". Under the suggested team system, with the combined efforts of both assistant and associate editors, I expect that emergency cases will be rare. The appointment of the special associate editor is only to ensure that

15

the journal can keep its promise even under the worst circumstances.

A minor suggestion to the editor, under the team system, is to include the assistant editor's name on the envelope when corresponding with the associate editor, whenever appropriate. This ensures that even when the associate editor travels (many senior researchers travel frequently), the paper can still be handled promptly. There are now enough means for the assistant editor to communicate with the associate editor efficiently. In the worst case, the assistant editor can inform the editor promptly that the associate editor is unavailable.

Some Cautions And Remarks

The team system is only suggested for those associate editors who need assistance. For those who already have efficient working systems that guarantee a ten-week turn-around time, please ignore my suggestions (but let us know your system and who you are!) — unless you want to help some junior researchers, provided that the team system does not reduce your present level of efficiency. The number one purpose here is to make our review system more efficient. Similarly, the team system might not be suitable for some journals, such as *Statistical Science*, due to their special aims and editorial structures.

For the same reason, the team system should never be implemented simply as another layer of bureaucracy, that is, the associate editor simply passes on everything to the assistant editor, and lets the assistant becomes another layer of handler. It is a **team** effort! The assistant editor should never find himself working alone, and should resign if that happens. Similarly, if the associate editor finds the assistant editor does not cooperate responsively, he should replace the assistant editor or simply work alone. In any case, the team system should never be allowed to be less efficient than the current system.

Also out of the consideration of efficiency, I do not encourage junior associate editors, like myself, to appoint assistant editors, even if we would very much like to. New researchers like myself are simply not able to provide enough incentive to attract other colleagues to work for us. This is a voluntary system; lack of incentive often means lack of cooperation and thus efficiency (lacking of visible incentive is in fact the reason to many for the current slow referee process). Hopefully, we are still young enough to have extra fuel to burn, and the incentive comes proportionally with the heat! This is a system that the busier one is, the greater number of junior researchers that would like to work for you (we naturally like to be attached to more active researchers in the field). If an associate editor experiences difficulty finding a suitable appointee, then perhaps he really doesn't need one, or even if he appoints one, it will not serve the two purposes of the team system. I also discourage appointing an assistant editor from a different location, unless the team efficiency can be absolutely guaranteed.

It is essentially costless for a journal to implement such a team system. The only extra cost that I can think of is the added list of assistant editors in the journal. Compared to the potentially tremendous benefit, especially to the journal, this should be of no concern.

THE ADVANTAGES OF THE TEAM SYSTEM

I see many advantages of the team system, when implemented properly. I'd like to recapture below its main advantages to substantiate my suggestions.

Advantages To Our Professional Community

1. To have more energetic individuals to share the large load of editorial work and thus improve the efficiency of our publication system;
2. To have a formal mechanisms in linking junior and senior researchers beyond graduate school (including post-doctors), and thus to help the career development of junior researchers;
3. To have a formal system to encourage new researchers' involvement in community activities, and thus to reduce the gap between junior and seniors researchers due to various perceptions, perceived correctly or wrongly;
4. To have a larger pool of experienced candidates for future editorial and other community appointments.

Advantages To The Associate editor

1. To have a personal editorial assistant who also knows the field;
2. To have someone to maintain an efficient editorial schedule for each submission;
3. To have a great opportunity to attract an energetic and talented research collaborator;
4. To be able to serve as a mentor.

Advantages To The Assistant Editor

1. To gain first-hand experiences about editorial work at very early stage of your career;
2. To have great opportunities to improve your writing skills, especially if you are a non-native English speaker;
3. To have a formal opportunity to work with and learn from a senior partner, and to establish a research collaboration as well as a personal relationship with him;
4. To have one more senior and often well-known researcher to write reference letters for you;
5. To have formal name recognition and an editorial position to put in your resume.

LET'S DOUBLE OUR EFFORT

New researchers benefit most under the team system, if implemented properly. I thus sincerely hope NRC will support me in suggesting that IMS, or any other statistical society or journal, investigate the feasibility of this expanded editorial system. I am also eager to hear general comments and suggestions, especially from my fellow junior colleagues and from experienced (associate) editors. If nothing happens, i.e., my suggestions turn out to be entirely naive, I hope that they will at least stimulate more new and workable ideas for improving our publication system, just as the debate on double-blind refereeing motivates me to write this article. The problem of the lengthy review process is so serious that it is

simply the time to double and redouble our effort for solving it. I believe the double effort from the team system is at least in the right direction.

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Figure 1:   WORKING SCHEDULE FOR THE EDITORIAL TEAM
(JA—Junior Assistant Editor; SA—Senior Associate Editor).

WEEK

1 { **JA & SA screen the paper;**
First team meeting;

Further review?

Yes   No

2 { JA prepares and sends
referee requests;   JA drafts a rejection letter
for the journal Editor;

3 {
JA prepares a referee   SA modifies JA's draft;
report, if time permits;   JA sends the letter and
4 { SA provides input and   an email to the Editor.
feedback to JA's report;
↕
6 {   End

7 {
JA sends email reminders to referees;
JA reminds SA of reading paper and
referee reports;
JA sends second email reminders,
if necessary;

8 { SA calls referees, if necessary;
SA reads reports;

9 {
JA drafts a summary report
and letters;
Second team meeting;

10 {
Team finalizes the review package;
JA sends the package and an email
to the journal Editor.

End