

# **REVIEW TIMES IN STATISTICAL JOURNALS: TILTING AT WINDMILLS?**

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## **SUMMARY**

Using limited data, I argue that the review times in statistics are far too long for the field to keep pace with the rapidly changing environment in science. I note that statisticians do not appear to believe in statistics, because data on the review process are not widely available to members of the profession. I suggest a few changes that could be made to speed up the review process, although it would appear that a change in our culture is required before the problem will be solved.

Key Words: Publication delays; Review times; Simulations; Survival analysis.

## **1. INTRODUCTION**

Dissatisfaction with review times in the core Statistics journals has become a point of major interest and discussion, with the editors of most of the major journals linked via email. An interesting article, with responses, is given in the IMS Bulletin, volume 24, number 6, 1995, pages 607-608, and the comments of Shayle Searle and Hugh Miser in the 1996 issue, pages 244-246. To quote Searle, “we should change the current procrastinatory behavior of referees”, and “I see no reason why our culture of slowness cannot be abandoned. It is something of which statisticians should be very ashamed”. Miser asked for “a more discriminating statistical analysis” than was shown in the original article: this note tries to address his point, but in fact the lack of available data on review times is itself a striking comment on the problem.

Having been the editor of *JASA Theory & Methods* (1987-1990) and *Biometrics* (1997-2000), I am acutely aware of the distress that lengthy review times cause authors. As an author of a fair number of papers in core statistics journals in the last 25 years, I feel the same frustration.

It is not controversial to say that review times should be quicker, but the problem is deeply ingrained in our culture. I will argue that the reasons typically justifying slow review times are largely specious, and that the effects are pernicious.

I have accumulated various statistics in order to characterize the review process, and I have talked to many people about the issue to try to understand their view of why papers take so long to appear in print. In Section 2, I summarize these points. In Section 3, I summarize a few ideas that can be proposed to speed up the process. Section 4 gives a few concluding remarks, including an analysis of possible times from submission to publication.

It is worth starting off this diatribe with my own answer to the question: why should we worry about review and publication times in Statistics? Searle lists his own choice of three points, and here are three of my own.

- 1) *For the future of our discipline:* There are many areas where contributions to statistical methods can come not just from statisticians, but also from engineers, computer scientists, mathematical modelers, etc. The Statistics literature is sometimes

years behind these other fields, to the point that the other fields dominate, and our literature becomes fairly unimportant, except to us. Data Mining is a classical example of this phenomenon, and there is real concern that the same may happen in the statistical analysis of DNA microarray data, or more generally in bioinformatics, despite the clear need for statistical advances in these areas.

a) It is useful to think of research as a conversation among experts, and publications as sentences. In Statistics, each sentence of the conversation takes 2+ years on average (see Section 2), while in other areas the conversation takes one year or less. It is no wonder that other fields can develop statistical methods far faster than we can, since the give-and-take takes place at a much faster rate.

b) One of the editors of a major journal cited a senior scientist, essentially as follows. Slow publication times in statistics journals handicaps the statistics community. Much of the recent innovative statistical methodology in his field has been developed by computer scientists, a fact that is partly attributable to statisticians not being quickly exposed to new ideas. Because statistics journals are so slow to publish, they lose a lot of their potential influence.

2) *For the perception of the active intellectual life of our discipline:* The perception among researchers in many fields is that Statistics is largely a dead subject, since we tolerate a lengthy publication process. The biologists and nutritionists I work with were perplexed when a recent paper to a core journal took over 8 months to get a first review. They are convinced that nothing much happens in Statistics, and that articles in Statistics exist mostly for the tenure and promotion process.

3) For the tenure and grant process: As mentioned above, both within and outside the profession there is a cynical view that research papers in Statistics exist for no other reason than to get grants and obtain tenure. Of course, the same can be said for research in any academic discipline. Some of this cynicism is valid of course, but my guess is that most of us write papers because we have ideas that we want other people to understand. Even if one credited the cynical view, the fact is that tenure and grants are an important part of academia, and how we as a discipline handle these issues is therefore also important. I believe that review times make it more difficult for statisticians to compete for tenure and grants.

a) When junior statisticians come up for tenure review in the U.S. (typically at the start of their 6<sup>th</sup> year in rank), their record mainly reflects what they have done in their first four years, with papers submitted in the 5<sup>th</sup> year often still under review. Candidates in other areas with faster review times will have a full five years of accepted publications. Our paper counts then look correspondingly thin. Many of us have been in the uncomfortable position of having to argue that our candidates need to be evaluated under special circumstances. Some are more successful than others in this argument, but the net effect is that we do not look productive.

b) The same argument applies for grants. Junior statisticians typically will have only a few actual publications at the end of their second year as an assistant professor, and this is not particularly impressive in a competitive environment for grants. Even in the NIH review process (the one I know the best), statistics grants are now being judged simultaneously with methods grants from social scientists.

Social scientists on these panels have a hard time taking our excuses particularly seriously.

## **2. STATISTICAL AND PERSONAL OBSERVATIONS**

### **2.1. The Problem of Review Times**

It is useful to start off with some statistics. Editors screen out papers (see below), so that the relevant statistics on times to first review are for those papers that *actually make it into a formal review* (defined by me as being in the review process for longer than one month). One can quibble with the definition, but I have no data that allow me to separate editor rejects from associate editor rejects.

The data I discuss were accumulated by me in 1999. Some of the data are public (*JRSS-B*, the *Annals of Statistics* and *Biometrics* routinely publish the data), while for *JASA Theory & Methods* and *Biometrika* I had to rely on the good offices of the editors. I therefore cannot guarantee the accuracy of the data I present, except for *Biometrics*. Perhaps this article will spur editors to publish more accurate and timely versions of data, although even with revised data I doubt that the major picture will change.

The journals listed above are highly selected of course. Selection bias is rampant in this analysis, because these are among the journals that I subscribe to personally. I apologize in advance to the editors of all those other excellent journals whose data I did not gather (or in some cases was unable to gather because of a lack of response), but I hope most

readers will agree that the journals I have selected are *among* the core journals in the field.

In Figure 1, I display the 75<sup>th</sup> percentile of the time to first review for reviewed papers of these major journals. Figure 2 lists the percentage of those papers that were in the review 6 months or longer.

The results only can be described as depressing. Regardless of journal, at least 25% of reviewed papers take 5 months or more to obtain a first review.

## 2.2. The Problem of Publication Times

We also have statistics on times from submission to publication of published papers. Figure 3 compares the mean number of months to publication in 1994 with the same times in 1999. Bradlow and Wainer (1998) present similar data. The means and medians were essentially the same. The upper quartiles were even longer of course, generally by about 3 months. These data were gathered by examining all issues of each of the listed journals, although some issues were missing from my personal library.

The times from submission to publication are distressingly long: two years is the norm even for papers that are good enough to be published. There appears to be only a slight change since 1994, an odd finding given the rapid change in electronic communication over these years.

Some argue that the excessive length is due to authors who do not submit revisions in a timely fashion. I do not think this is the case in general: see the statistics given below.

Certainly, there is some tardiness, but in my experience there are four major factors:

1. Lengthy times to first review (see above)
2. Requests by referees for additional simulations/research (see below)
3. Lengthy times for second review (anecdotal, since only *Biometrics* publishes this figure)
4. Publication delays after acceptance.

Points 1-3 are the culture of the field, and these can be changed. Point 4 requires action and costs on the part of the societies sponsoring the journals.

### 2.3. The Claim That Editors Should Screen Out More Papers: Statistical Analysis

Many papers simply have no chance of being published in the journal to which they are submitted. There is a sense in the community that editors are not doing their job here, and are simply sending out the papers for review and cluttering up the review process. Like much folklore, this is subject to a statistical analysis. In Figure 4, I list the percentage of papers that I judge to have been screened without formal review: my algorithm is that the review time is less than a month for most journals, while for *Biometrics* I was able to use the database. Generally, approximately 35% (and between 20%--50% depending on the journal) of submitted papers are rejected without formal review. This is a high figure, considering that for core journals the subject matter ranges across the discipline, and

editors cannot judge the merits of papers in fields for which they have little expertise. I conclude that it is not the editors who are at fault here: they are screening out a good fraction of submitted papers.

#### 2.4. The Problem of Referees Rewriting Papers Instead of Reviewing Their Content

In my experience as an author and as an editor, besides the time to review, there is a single factor that stands out between reviews in statistics and reviews in epidemiology/science. Specifically, statistics reviewers tend to avoid the main point (is the paper worth publishing?), and instead focus on forcing authors to rewrite their papers into a style the reviewer is comfortable with. Always attached is a detailed list of typographical errors, as if to convince the editors that the paper really was read in detail. Editors should make it clear to reviewers that review means review of content: editors and associate editors can and should take care of exposition.

#### 2.5. The Problem of Reviewers Asking For Yet Another Simulation: Personal Observations

A common request from referees is for more simulations. Clearly, many papers do indeed need more simulations, or at least they need more realistic ones. What seems to be forgotten is that casual requests for more simulations add immensely to delays in the publication process. Programs have to be resurrected, modified, rerun, accumulated and then checked (by careful authors, namely 99% of them) by sensitivity analysis. What is required is selectivity on the part of reviewers: will additional simulations of a more realistic nature possibly change the conclusions? It seems to me that if authors have

presented what appear to be careful and realistic simulations, then referees should hesitate to ask for yet more. Associate editors especially, and editors as well, should carefully evaluate requests for simulations to insure that they are targeted requests aimed at making the conclusions of the papers firm and convincing.

#### 2.6. Are Fast Reviews Bad Reviews, or Type II Errors are Important Too

Inevitably, in any discussion of review times, objection to quicker reviews will be based on the issue of Type I errors: if bad papers appear in our journals then bad statistical methods will be used. The argument typically takes the further form that papers in Statistics are more difficult to judge than in other fields, that reviewers in Statistics are more careful than in other fields, and that both mean that reviews of 6+ months should be tolerated. This can be expressed (and has been expressed to me) as: *core journals should focus on thorough exploration of methodology. If it takes a little longer to get this right, then so be it.*

I disagree with all of this. My experience in fisheries research, nutritional epidemiology, basic nutritional biology and molecular biology, and from what I have heard about the major social science journals, is that reviews are brutal, skeptical, and thorough. Statisticians do not hold a monopoly on careful reviews, nor do we hold a monopoly on intellectual difficulty. The differences are that statistical reviews take much longer (although Bradlow and Wainer document that *Psychometrika* is even slower than major statistics journals), focus more on editorial issues than on content, and often add frivolous requests for further simulation experiments: they are not more thorough or careful. Our

field, as fascinating as it is to us, is not more intellectually challenging or difficult than other fields.

### 2.7. Do Statisticians Believe in Statistics?

It is remarkable that the figures I have accumulated are not readily available for all the major journals. In the age of data base management, it is easy for journals to publish the following data routinely and accurately:

- Percentage of papers rejected by the editors without formal review by referees.
- Percentage of papers handled by the editor and associate editor with no referees (not quite calculable from the above because some papers are accepted without being sent to referees), broken down by original submissions and revisions.
- Kaplan-Meier curves of time to first review and times to second review for (a) papers that are handled by the editor and associate editor only and (b) papers that are sent to referees.
- Times (mean, median, 75<sup>th</sup> percentile) from formal acceptance of a paper until its publication, i.e., publication queue delays.
- Times (mean, median, 75<sup>th</sup> percentile) from submission of an accepted paper until its publication.

That journals in Statistics do not publish statistics is shocking. I believe that authors in Statistics should have ready access to these statistics, so that they can choose intelligently the journal to which they will be submitting.

### **3. WHAT CAN BE DONE ABOUT REVIEW AND PUBLICATION TIMES?**

I believe that it is well within our abilities to reduce the average time to publication to a little more than a year, something already very nearly the case for the *American Statistician* (Bradlow and Wainer, 1998).

#### **3.1. What Editors Can Do:**

There are a series of steps that editors can undertake that will speed up the review process. I plead guilty to not having followed (c) and (e) as rigorously as I should have.

- a) Encourage electronic submissions and reviews. The new editors of *Biometrics* allow electronic submissions: this paper was submitted electronically. They then send the papers electronically to associate editors, who communicate electronically with referees, etc. The statistical computing journals have been doing this for some time.
- b) Publish statistics (see above) on your review process, on your web site and in your journal. Unless you know the extent of the problem, it is hard to motivate changes.
- c) Set a time limit on reviews, e.g., 6 weeks. At that time, if a referee has not responded, have your editorial assistant begin to call and send emails asking for a review forthwith.
- d) Emphasize to referees that reviews are not meant to be editing. Reviewers need to be reminded that their task is to review the content and the results for importance, originality, and correctness. They should be discouraged from going through the text line-by-line to make editorial changes.

- e) Carefully monitor requests for new simulations. These can take a great deal of time for authors to accomplish, often to little use.
- f) Discourage further reviews by referees of revisions. Most papers that are submitted as revisions are invited as revisions. The editor/associate editor can usually check quickly if the authors have responded to the initial round of reviews, and only rarely is it necessary to contact the referees.
- g) It has been suggested that the major core journals institute a “points system” for good refereeing, to be taken into account when decisions on accepting papers are made. All editors are tempted to do this, and indeed superb reviewers clearly win the hearts and minds of editors, but as a formal mechanism the idea is ludicrous. Any U.S. editor or U.S. incorporated entity (such as the IMS, ASA, IBS) that participated would instantly open themselves to threats of lawsuits.

### 3.2. What Journals Can Do:

- a) The major societies and publishers have the obligation to their members to allow for speedy transmission of original results.
- b) Publication backlogs need to be monitored and removed by the relevant journal oversight committee. In my term as the editor of *Biometrics*, we shortened the publication backlog from 13 months to 9 months by a combination of extra pages from the International Biometric Society and a *rigorous limit on lengths of papers*.
- c) Electronic dissemination of papers as they become typeset would appear feasible. Many statistical societies are investigating this issue now. For example, in *Biometrics*,

if we made available electronic versions of papers as they are typeset into Latex with hard copies following, results would be available four months earlier.

### 3.3. What Associate Editors Can Do:

- a) Take my points to heart!
- a) Be something more than a mailbox that filters referee reports to the editors. Read the papers before sending them out to reviewers.
- b) If the paper is unacceptable, simply tell the editors and write a report documenting why you think this is the case.
- c) If you do send the paper to referees, give them guidance, e.g., what you thought of the paper, what you would like them to concentrate on, etc.
- d) Although some authors feel that they deserve two reports or else the process is not fair, two reports are almost always (more than) enough. Do not let the process drag on for months waiting for an extra report: you will almost always be disappointed.

### 3.4. What Referees Can Do:

- a) You are not the editor or proofreader. Focus on originality, correctness and importance, and do not waste time/effort on details. If a paper is original, correct and important, your insisting that material be moved around or restated to fit your own preferences is a waste of everyone's time.
- b) Do not ask for additional simulations unless they are vital. Resist the temptation to have authors look at yet another special case.

- c) If you do not want to review a paper, either because it is not in your area or because you cannot get to it in a timely fashion, tell the editors *immediately*. They will not mind, unless you turn down most requests for reviewing while at the same time submitting numerous papers yourself to the journal (this happens!).
- d) Set a limit on the number of papers you will review in a year, and review the papers quickly.
- e) For revisions, go through the authors' responses to your comments, usually given to you in the form of a letter. If the authors have been reasonably responsive, just tell the editors that you are happy with the results. Resist the temptation to start the review process from scratch.
- f) If you cannot review a paper, take a few minutes to suggest some alternative reviewers, particularly more junior people. This will help the editors, and will also get more people involved in the editorial process.
- g) Hope (against hope?) that if you review papers more quickly, others will too, and your own submissions will receive more timely response.

### 3.5. What Authors Can Do:

Essentially every reader of this manuscript has commented upon a perceived link between quality of the preparation of the manuscript and review times. Surely there is some relationship, but presumably every author tries to submit a coherent manuscript.

## **4. FINAL COMMENTS**

### **4.1. How Well Can We Do?**

In Figure 3, I compared the mean time from submission to publication for the selected Statistics journals and for *the American Journal of Epidemiology* (AJE), the premier journal in its field. The AJE is considered to be somewhat leisurely in its reviews, but still takes only 15 months on average from submission to publication in 1999, five months faster than the Statistics journal (*Biometrika*) that has the shortest mean time from submission to publication.

I believe that it is well within our reach to change our publication times from the current 2+ years to something approximating the 1994 figure (12.8 months) for the AJE. To do this, I consider the experience of *Biometrics*, for which I have some detailed data. Figure 3 states that the average time from submission to publication in 1999 for *Biometrics* was 25 months. The papers published in this journal in 1999 were from a mixture of four different editors, two main editors and two “Shorter Communication” editors. The publication queue was approximately 12 months at this time, with the mean and the 75<sup>th</sup> percentile of the times in review being 12.4 and 16.0 months, respectively.

To obtain a more focused analysis, I went back through the *Biometrics* data base for papers that I handled as editor from February 1997 through December 1999 that have been accepted for publication (this is approximately 98% of the papers that were submitted to me that will be published). For these papers, I have available the times for a

first review, the time that it takes the authors to submit a first revision, and the times for a second review.

Table 1 first lists the mean and 75<sup>th</sup> percentile of these times, plus one month for the total review process to account for a final submission with all typographical errors removed. This allows a rough view of the time an accepted paper spends in the review process. I also added 6 month and 10-month publication queues. As stated above, in 1999 in *Biometrics*, the publication queue was approximately 12 months.

One of the interesting points in Table 1 is the time between first review and the submission of a revision. The mean time is 4.3 months, and the upper quartile is 5.3 months. Of course it is tempting to conclude that major savings in time would occur if the authors would simply revise their papers more quickly. Certainly there is some of this, e.g., papers submitted in 1993 that finally appear in 1999 do indeed exist. I have argued above however that one problem with this conclusion is that authors are often asked for extensive new work, especially simulations, and that these are not easily or quickly done. Editors can have a major impact here simply by being more judicious in choosing when demands for additional work are really necessary.

We see from Table 1 that with a 10-month publication queue, the mean time from submission to publication using these figures alone is approximately 20 months, reasonably close to the mixture-of-editors 23 month mean that would have occurred with a 10-month publication queue, although still an improvement. Clearly, a considerable

savings in time to publication can be gained by publishing accepted papers within six months of acceptance, something that is technically possible. In other words, if *Biometrics* had a 6-month publication queue, at least for one editor the mean time from submission to publication would approximate that of the AJE, namely 16 months. The 75<sup>th</sup> percentile would be 18.4 months.

In Table 1, I have also shown what would happen if three things were to happen:

- 1) First reviews were to take no more than 3 months: the data were truncated at this limit.
- 2) Authors returned revisions within 4 months: again the data were truncated at this limit. Most will if they do not have to do additional extensive simulations.
- 3) Second reviews take no more than 2 months, also mostly feasible: again the data were truncated

All three of these are feasible with the electronic submissions and reviews that are currently possible in *Biometrics*. When we assume these as given, the mean time to publication becomes 12.8 months if there is a 6-month publication queue.

In sum, in 1999, *Biometrics* had a review process that for accepted papers took approximately 12.4 months on average, with an upper quartile of 16.0 months. Using a single editor's data and adding in seemingly realistic review/revision limits, it appears possible to cut the mean review time to 7.8 months, with an upper quartile of 8.9 months. This is a savings of 4.6 months or so on average (8.2 in the upper quartile) in the review

process made possible by not too stringent changes in the culture of reviews. Even 4 months is a significant savings in time for a fast-moving field. Further savings in time to publication can be achieved by the journals themselves, either in lowering their publication queues, or making the papers available electronically before formal print publication.

I would welcome current editors of major journals providing analyses such as I have done. It seems to me to be a reasonable conjecture that with changes in the culture of reviews a four months savings in time in the review process and perhaps more is possible. Approaching the publication times of a relatively slow journal such as the AJE also seems feasible.

#### **4.2. Final Thoughts:**

I believe that shortening review times, and shortening times to publication, are critical for the health of our profession. We face a slow extinction, or at least a narrowing of the fields in which we can make an impact, unless our journals reflect the fast pace of change in statistical methods development. I hope that some of the suggestions I have made will be adopted by reviewers and editors.

My strongest hope for this article is that, as a profession, we build statistics into the review process. Journals could publish the statistics as I have suggested above, longitudinally, so that authors can compare and select journals sensibly. We have all

suggested to our clients and collaborators that they should gather statistics: perhaps we as a profession can take our advice to heart.

I would of course happily accept current data from editors and post the analyses on the *Biometrics* web site at Texas A&M University (<http://stat.tamu.edu/Biometrics>).

### **Acknowledgments**

I thank Xiao-Li Meng, James Calvin and Wolfgang Haerdle for their many useful comments on this manuscript. Ann Hanhart, the superb editorial assistant for *Biometrics*, prepared the data displayed here for that journal. I also want to thank the many associate editors of *Biometrics* for their work for the journal.

### **References**

Bradlow, E. T. and Wainer, H. (1998). Publication delays in statistics journals. *Chance*, 11, 42-45.

Figure Captions:

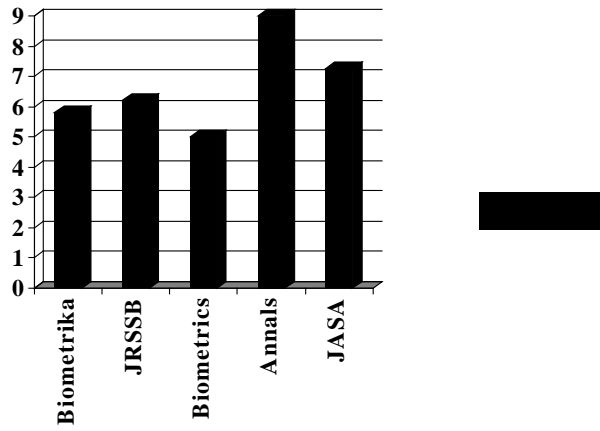
Figure 1: Times to first review for reviewed papers, 75<sup>th</sup> percentile.

Figure 2: Percentage of reviewed papers under review for 6 or more months.

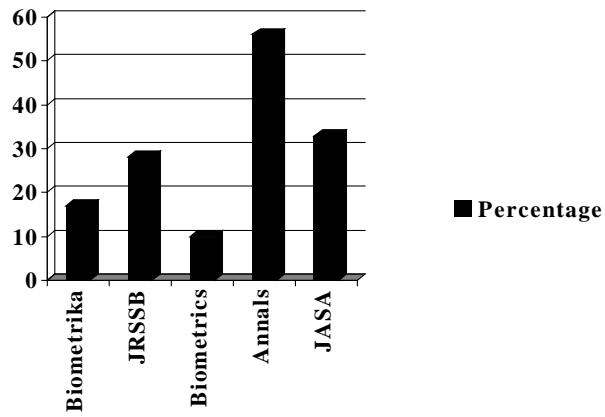
Figure 3: Mean time to publication (in months) for accepted papers: 1994 and 1999.

Figure 4: Percentage of papers rejected without formal review.

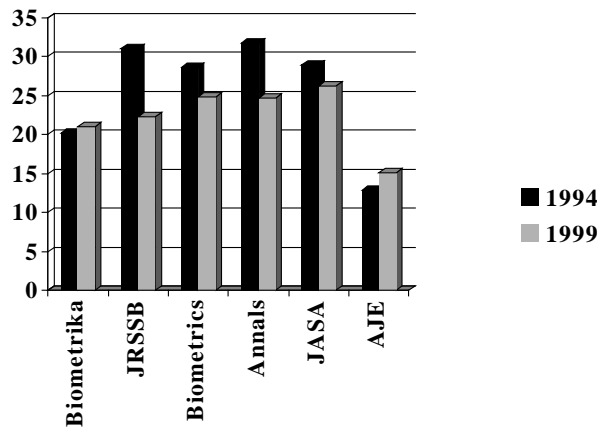
**Figure 1: Time to First Review (in months)  
for Reviewed Papers: 75th Percentile**



**Figure 2: Reviewed Papers: Percent Taking 6+ Months for First Review**



**Figure 3: Mean time to Publication (in Months) for Accepted Papers: 1994 and 1999**



**Figure 4: Percentage of Papers Rejected Without Formal Review**

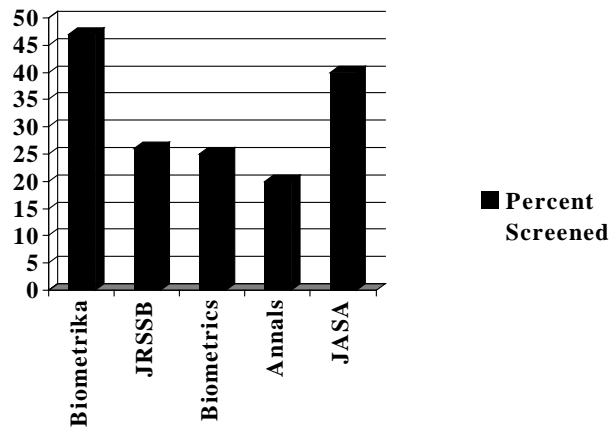


Table 1: Times to review in months for accepted papers in *Biometrics* for a single editor.  
<sup>1</sup> Indicates that one month has been added to account for final revisions. See text for explanations.

		Actual Data	Proposed Data
First Review	Mean	3.8	2.6
	75 <sup>th</sup>	4.9	3.0
Author Revision	Mean	4.3	3.0
	75 <sup>th</sup>	5.3	4.0
2 <sup>nd</sup> Review	Mean	1.8	1.1
	75 <sup>th</sup>	3.0	2.0
Review Process <sup>1</sup>	Mean	11.0	7.8
	75 <sup>th</sup>	13.4	8.9
With 6 month queue <sup>1</sup>	Mean	16.0	12.8
	75 <sup>th</sup>	18.4	13.9
With 10 month queue <sup>1</sup>	Mean	20.0	16.8
	75 <sup>th</sup>	22.4	17.9