Peer Review

Peter H. Schonemann

This section provides corroborating documentation to the timeline sketched in the introduction of Schonemann &Scargle (2008). I would like to thank Professors Darlington, Thomas, and Bloxom for making this material available and for their permission to use it here.

1. Re: Darlington: Positive review. Schonemann to Bloxom, 11. 28. 1980.
2. Re: Darlington: Encouragement to revise. Bloxom to Darlington, 12. 1. 1980.
3. Re Darlington: Rejection letter. Bloxom to Darlington, 10. 27. 1981.
4. Re: Thomas: Encouragement to revise. Myers to Thomas, 4. 19. 1985.
5. Re: Thomas: Rejection letter. Myers to Thomas, 2. 24. 1986.
6. Re: Thomas: Change of editor. Myers to Thomas, 4. 18. 1986.
7. Re: Thomas: Acknowledgement of receipt. Masters to Thomas, 5. 28. 1986.
8. Re: Thomas: Encouragement to revise. Hedges to Thomas, 12. 1. 1986.
9. Re: Thomas: Negative review. Unknown author, 3. 8. 1988.
10. Re: Thomas: Rejection letter. Hedges to Thomas, 3. 8. 1988.

Review of ms. #80439: Another peek . . .

Overall: Unless I am missing something very subtle, the main point the author makes seems correct: Rosenthal's (1979) file drawer formula is invalid, because the file drawer hypothesis (FDH) conflicts with the basic assumption from which the various pooling formulae were derived (viz. that sampling is random over the whole range of the distribution under H_0). Hence this part of the paper is well worth publishing to counteract the possible damage uncritical use of Rosenthal's formula might cause.

However, I am not certain the author appreciates the full significance of his discovery. It is, in my opinion, that "metaanalysis" is invalidated by the FDH. If we have reason to entertain the FDH (and I believe we do) then we have no business pooling the p's under H_0, because they are then sampled from a truncated (in the simplest, all-non case) or a transformed distribution under H_0 (if the biassing function is more general).

I am, therefore, somewhat sceptical about the various alternatives the author proposes to deal with the FDH problem (but I have no strong feelings about them).

Details

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Bruce Bloxom, Associate Editor
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December 1, 1980

Enclosed is a copy of your paper, "Another Peek in the File Drawers" (Ms. #80439), which you submitted to Psychological Bulletin. I am sorry to say that I cannot accept the paper for publication in its present form. However, a revision of the paper may well be acceptable.

As you can see from the reviewers' comments (enclosed), they are very skeptical about the proposed alternatives to Rosenthal's recommended use of Stouffer's method. However, one of the reviewers is persuaded of the validity of your criticism of Rosenthal's recommendation. A brief note containing just that point—with appropriate qualifiers—could well be Bulletin material. The qualifiers should include not only the reservations you currently express about the use of formula (2); they should also indicate that selection variables associated with the editorial process make formula (2)'s estimated number of studies an underestimate. (Let's not forget to peek in the editor's file drawers also.) Furthermore, the Baysian reviewer's criticism is worth mentioning as an alternative perspective on the enterprise of simply counting studies which have a particular outcome.
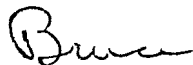
If you decide to revise and resubmit the paper—and I hope that you do—please send it to George Miller, Editor, with a cover letter indicating that it is a resubmission. He will assign it a new number and forward it to me. I will then send it out for review if it appears to need further review.

I look forward to hearing from you soon. If you can submit a revision of the paper within a couple of weeks and if the

review process goes smoothly, there is a chance the paper could appear in the May issue. Of course, Rosenthal would need to be given an opportunity to reply in the same issue, but his paper would need to go through the same review process as your paper has.

Sincerely,

Bruce Bloxom
Associate Editor

BB:jtc

Enclosures

October 27, 1981

Dr. Richard B. Darlington
Department of Psychology
Uris Hall
Cornell University
Ithaca, New York 14853

Dear Dick:

Enclosed is a copy of your paper, "Another Peek in the
File Drawers" (Ms. #81329), which you resubmitted to Psycho-
logical Bulletin. After giving you some tentative encourage-
ment to revise and resubmit the paper to us, it is especially
painful to have to say that I cannot accept the paper for
publication.

Neither of the two reviewers used earlier (#1 and #2)
thought that the major criticism made by one of them (#2)
was given adequate weight in the revision. Although reviewer
#1 thought a minor revision of the paper would be acceptable,
reviewer #2 indicated the problem was much more serious. (The
letters from both of the reviewers are enclosed.)

Before giving you more encouragement to revise, it seemed
prudent to obtain a third reviewer's comments on the first two
reviews as well as on the paper. That reviewer's comments
are enclosed also and are not at all supportive of asking you
to revise the paper. The main concern seems to be the same as
that of reviewer 2: the paper would be seen by many as a
corrected solution to the problem posed by Rosenthal, and it
should not in any way be seen as such; there are other, ap-
parently more important corrections which need to be made
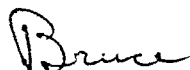before the problem should appear to be "solved."

In view of these arguments posed by reviewers 2 and 3
and in spite of the positive point made by reviewer 1, I do
not feel justified in encouraging you to revise and resubmit
the paper to us. Thank you, however, for pursuing publication
of the paper to this point. It is obvious that many are find-
ing fault with much that has been said on meta-analyses. The

Re: Ms. #81329

problem seems to be to develop a critique which effectively
exposes the most salient difficulties with such analyses.
I hope that the reviewers' comments will be useful in your
further thought about this problem.

Sincerely,

Bruce Bloxom
Associate Editor

BB:jtc

Enclosures

April 19, 1985

Dr. Hoben Thomas
Department of Psychology
Pennsylvania State University
417 Bruce V. Moore Building
University Park, PA 16802

Dear Dr. Thomas:

Enclosed are two reviews of your manuscript, "On the Pooling of $\underline{Z}$ Scores, Meta-analysis, and the 'File Drawer Problem' " (#85-014). The reviewers recommend against acceptance and my own reading of the manuscript leads me to the same conclusion and for much the same reasons. The points you raise are interesting, but there are problems of both substance and style. It may be that these can be remedied, but it would take a major revision.

With respect to substance, I agree with Reviewer A that you overstate the case for truncated models, oversimplify the issue of deciding on the truncation point, and make some rather strong assumptions of your own. The choice of a model is extremely difficult. You could make a real contribution by an evenhanded treatment in which you carefully considered circumstances in which various models (including that underlying the usual approach) might be reasonable and clearly pointed out and the possible difficulties with each. To my mind, this means noting many of the points that Reviewer A raises under his point 2. For example, like him, I am unconvinced by your footnote 2 that submission and publication are roughly equivalent. Perhaps the appropriate approach might be to take the standard statistic as a boundary condition (assuming no truncation) and to also consider other possibilities given truncation. Reviewer A suggests something of this sort.

A second substantive problem is the omission of relevant citations of previous work on truncation and on failures to publish nonsignificant work. Reviewer B provides several references for your consideration.

There are also several stylistic problems. Both the reviewers and I agree that you tone is overly adversarial. You do Rosenthal an injustice and even your barely concealed hostility to standard meta analysis practices is misplaced, particularly given that your own assumptions are debatable (again, see Reviewer A's point 2).
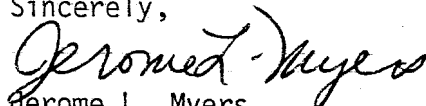
Reviewer B notes that the organization presents difficulties and Reviewer A's comment that you open with the "file drawer problem" but never really provide closure relates to this issue. I would put it more strongly: I began this paper believing it was about to present an alternative solution to the file drawer problem, was perplexed to find on p.6 that " the file drawer problem is not of

primary concern" (Why six pages largely on it then? And where are we headed?), and found the manuscript a seemingly random walk in a multidimensional space bounded by the appropriate test statistic, transformations, and the file drawer paper. It would be very helpful to have an introduction that clearly stated the purpose of the paper and then developed that purpose in an organized manner.

I also believe that the average reader who is interested in meta analysis but is not a statistician will find pages 7-9 unnecessarily formal and forbidding. There is a very simple point here: standard procedures for obtaining a test statistic for a body of literature reflect the assumption that the meta analyst has access to the complete body of studies. That assumption is incorrect if availability of results depends upon some critical value of the test statistic. In that case we have a conditional distribution which will not be the usually assumed normal. I don't really find the development on pp. 7-9 saying much more than that and the development seems unnecessary for our readers.

In summary, I think the point that there are alternative models, many of which may be more appropriate than the current one in some circumstances, is important. A discussion of these models and a presentation of relevant statistics would be useful. Such a discussion should be objective, taking into account the issues raised by the reviewers, and it should have a clearer focus and organization than the present paper. I cannot assess the likelihood of acceptance of such a revision but I would be willing to send it out for review again.

Sincerely,

Jerome L. Myers
Associate Editor

Monday February 24, 1986

Dr. Hoben Thomas
Department of Psychology
The Pennsylvania State University
417 Bruce V. Moore Building
University Park, PA 16802

Dear Dr. Thomas:

Enclosed are two reviews of your manuscript, "On the 'File Drawer' Problem" (#85-331). Both reviews recommend against acceptance for very similar reasons. In essence, they question your assumptions and the relevance of this work to the file-drawer problem as presented by Rosenthal. I find their arguments convincing.

I appreciate the opportunity to have considered this manuscript. I hope that this rejection will not discourage you from submitting other work to the Bulletin.

Sincerely,

Jerome L. Myers
Associate Editor

April 18, 1986

Dr. Hoben Thomas
Department of Psychology
The Pennsylvania State University
417 Bruce V. Moore Building
University Park, PA 16802

Dear Dr. Thomas:

In regards to your manuscript, "On the 'File Drawer' Problem" (85-331), you may well be correct in your critique of Rosenthal but apparently you failed to communicate your point to two competent reviewers, both of whom recommended outright rejection of the manuscript. If I were to continue as editor of the Bulletin's Quantitative Methods section, I would be willing to send a revision to additional reviewers. However, assignment of your manuscript to a new set of reviewers would entail an extension of my responsibilities well past the point at which they should terminate. Therefore, I am returning the copies of your manuscript to you. You should submit them to John Masters, the new editor of the Bulletin. I assume he will assign them a new number and forward them to Larry Hedges, my successor. Hedges would seem to be an excellent person to judge the merits of your argument and to find reviewers for it. You should include the number of this manuscript so that Dr. Hedges can request from me the names and addresses of the old reviewers. Even if you don't feel those reviewers were adequate, you certainly don't want Hedges to assign the paper to them again which, unknowingly, he might.

I regret that my action has so discomfited you, but I again suggest that the problem may be one of communication; it appears from your letter that you have been misinterpreted at several points. I hope that the revision is clear enough to afford you the reviews you feel you deserve. I wish you luck with the resubmission.

Sincerely,

Jerome L. Myers
Associate Editor

cc: Betty House

Psychological Bulletin

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May 28, 1986

Hoben Thomas
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The Pennsylvania State University
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University Park, PA 16802

Re: "On the file drawer problem"
     Ms. No. 0050586-169-R

Dear Dr. Thomas:

   Thank you for submitting your manuscript. I am willing to consider it as a new submission without prejudice, although I do believe that the the processing of the manuscript should be informed by prior reviews as well as an awareness of earlier editorial decisions. Since the manuscript was originally processed as a part of the Quantitative Methods section of the Bulletin, I am referring it to Larry Hedges who is now the associate editor of that section. He will be the processing editor. I am also sending him copies of relative prior correspondence, but let me reiterate that this is for information and I am trusting in his ability to process the manuscript without prejudice because of its prior history.

   You should be contacted in about two months concerning the status of your paper. If you have questions prior to that time, please feel free to call or write Larry Hedges directly.

Sincerely,

John C. Masters
Editor-Elect

REVIEW OF "THE FILE DRAWER PROBLEM"

p. 3

I disagree that Rosenthal's fail-safe approach is "conceptually flawed." It is a nice formulation of the file-drawer problem that aims at some of the simplest questions that arise in a meta-analysis — namely, questions about significance of the observed studies in the light of our beliefs about unobserved studies. It can be modified in many ways to handle many different modelling assumptions (such as the variance corrections in this paper). However, such modifications are relatively minor. Rather, the the focus of interest should be on more careful modelling of selection bias (arising from the many sources of publication and reporting bias) in a meta-analysis, of variability in the studies' effect size estimates, of variability in the study designs, and the like. From this point of view, it seems that this paper concentrates on these minor issues, and makes no contributions to the more involved issues in meta-analysis.

This paper seems to be too mathematical for its intended audience; also, the exposition is quite poor. Some specific comments about this paper follow:

p. 4

The sentence "In effect setting ..." makes no sense as is.

p. 5

Reporting the result of a single simulation is poor practice.

p. 5

Why include Appendix A if it is not of practical interest ?

Psychological Bulletin

December 1, 1986

Hoben Thomas
Department of Psychology
The Pennsylvania State University
417 Bruce V. Moore Building
University Park, Pennsylvania 16802

RE: MS #0050586-169-R, "On the 'File Drawer' Problem"

Dear Dr. Thomas:

I appreciated your letter of October 15, 1986. As a matter of fact, I had eventually received your letter to Jerry Myers dated March 18,1986, and I shared your concern that reviewers with adequate statistical training should be used. I sometimes find that people who are not trained in statistics departments miss important subtleties in statistical arguments. The delay in reaching this decision has stemmed, in part, from trying to ensure that the reviewers were up to the task. I solicited reviewers (including some from your suggested list of reviewers) who were either trained in statistics departments, are currently appointed in statistics departments, or who were among those mentioned in your letter of March 18, 1986. Several potential reviewers declined to review the paper. The eventual reviewers both have substantial reputations in statistics (as opposed to behavioral sciences) and are familiar with the context of meta-analysis or combining the results of studies. One of the reviewers had apparently seen the paper before.

Before discussing the reviews, let me state for the record that I am not particularly enthusiastic about Rosenthal's file drawer number. I believe it has not been given a sound conceptual base and that it is often used in ways that are misleading. The reviewers were not particularly positive about the manuscript. Reviewer A felt that the manuscript should be rejected and Reviewer B felt there was some chance that a major revision might be publishable. The reviewers feel that your criticism of Rosenthal is too strident and, perhaps, stressed technical detail to a greater extent than is fruitful. For example, the fact $P(\sum Z_i' = 0) = 0$ is true, but we could easily imagine an approximate analysis where we acknowledge only that $E(\sum Z_i') = 0$ and $Var(\sum Z_i') = x$. Such a fix also resolves the apparent error in the variance of $Z_?$. Reviewer A raised the crucial point that although all of the Z statistics have the same distribution before being classified as Z^* and Z' values, the Z^* and Z_i values do not necessarily have the same distribution. The distribution of Z^* values is conditional on being observed. In the strictest publication bias model (only significant results observed), no Z^* values that are small in absolute magnitude are possible since small (i.e., nonsignificant) Z values are unobserved. Note, however, that Rosenthal's assessment of the probability associated with Z_i can be thought of as avoiding the conditioning problem by using all Z'^S regardless of their status as observed or unobserved. I agree
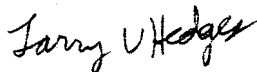
with the reviewers that the attack on Rosenthal's method may have a minor technical point, but it does not by itself demonstrate the unreasonableness of the procedure.

Reviewer B reacted positively to your alternative suggestion but felt that a clearer exposition was needed. I agree with that assessment. However, if you pursue this, I urge you to think precisely about the "importance" dimension. Exactly what is meant by importance can have a crucial effect on the sampling properties of counts in your table 1. For example, if importance means statistical significance, then the counts in the table are only identically distributed if the power of each test is the same. Hence, if you want to have identically distributed observations in each cell, you will have to add restrictions of equal sample size or effect size which may be unrealistic.

My decision, after thinking about your manuscript and the reviews, is to reject the current version of the paper. However, I will also give you the option of preparing a major revision along the lines suggested by Reviewer B. Such a revision would probably concentrate almost exclusively on your new proposal. Note that I cannot guarantee eventual publication of any revision that you decide to prepare. If you decide to prepare such a revision, send four copies to John Masters and indicate in your cover letter that the manuscript is a revision. He will then forward it to me for processing.

Even though this decision is not what you would have wished, I hope that the reviewers' comments are helpful to you. I also hope that you will consider Psychological Bulletin as a possible outlet for your work in the future.

Sincerely,

Larry V Hedges

Larry V. Hedges
Associate Editor

LVH:jmt
enclosures

Psychological Bulletin

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March 8, 1988

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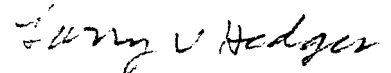417 Bruce V. Moore Building
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RE: Ms. #0-121787-321, "The 'File Drawer' Problem"

Dear Dr. Thomas:

I now have two reviews of your manuscript. Three reviewers were involved. The review labeled "A + B" was prepared jointly by two reviewers. Review C was prepared by another individual. Note that all three reviewers are statisticians. They hold appointments in two of the most prestigious statistics and biostatistics departments in the country. The reviewers are quite familiar with the file-drawer idea. Note also that they are not the same reviewers that I used for your earlier paper. However, both reviewers recommend rejection. The reviewers don't argue that the mathematics is incorrect. They argue that the paper doesn't make a sufficiently great contribution to warrant publication in Psychological Bulletin. Therefore I am rejecting your manuscript.

Even though this decision is not what you would have wished, I hope that the reviewers' comments are helpful to you. I also hope that you will consider Psychological Bulletin as a possible outlet for your work in the future.

Sincerely,

Larry V. Hedges
Associate Editor