

Decision Letter (AMLE-RR-2009-0037)

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CC: amle@aom.pace.edu

Subject: AMLE - Decision on Manuscript ID AMLE-RR-2009-0037

Many thanks for submitting "COMPETITION AND KNOWLEDGE CREATION IN MANAGEMENT: INVESTIGATING CHANGES IN SCHOLARSHIP FROM 1988 TO 2007" (AMLE-RR-2009-0037). The review process is now complete, and three thorough reviews from highly qualified referees are included at the bottom of this letter. Although the reviewers found considerable merit in your paper, they also identified some concerns. Therefore, I invite you to respond to the action editor and reviewer(s)' comments and revise your manuscript. In the space provided for Response to Reviews, please document any revisions and be sure to respond to both the action editor and the referees in a point by point fashion. In order to expedite the processing of the revised manuscript, please be as specific as possible in your response to the reviewer(s) and be sure to avoid adding any author identifying information.

I thought each of the reviewers did a fine job of commenting on the manuscript, so I'll not repeat all those comments here. I would, however, like to identify some area of potential focus as you contemplate your revision effort:

1. Perhaps the primary challenge of this manuscript for AMLE is how to make its orientation fit more explicitly with issues in the journal's domain. Contrary to Reviewer 2's concerns about fit, I believe this is a topic well within AMLE's domain in light of the journal's recently broadened mission (see James Bailey's FTE for the March 2006 issue). However, the reviewer does make a point here. How might the manuscript address topics in a way that make it seem less like another version of the recent review articles you've cited? I can see at least a couple of ways to address this. One of the things I've called for is for management scholars to use management theory as a way to frame research questions on topics of learning and education research (see the FTE for the March 2008 issue as an example of that call, see my article in AMLE's March 2005 issue as a feeble attempt to try to do this). Reviewer 3 suggests that you further develop your analogies from management theory to describe the academic management publishing industry, and I certainly encourage you to do so.

Second, I think you have some underdeveloped implications for business schools and scholars in your discussion section that could be further mined for issues of relevance to AMLE readers. Morgeson and Nahrgang (2008) recently raised the issue of the problems of business schools throwing increasing amounts of resources to ascend rankings lists even though the occupants of those top spots have been essentially set from the inception of such lists. I think further engaging the implications of this article in your discussion would be helpful. I also think you raise some interesting implications with the "Senior Assistant" phenomenon for doctoral programs, the potential "farm schools" that may end up hiring new Ph.Ds only to lose them once they gain publishing experience, new Ph.Ds. that have the potential to be top scholars but might not develop these skills because they start at schools with greater teaching loads,

etc. I don't have specific guidance for how to address this here, but I do encourage you to stretch in your thinking regarding potential implications.

2. Each of the reviewers noted concerns and suggestions with your delineations of macro and micro. I think the co-author phenomenon (as noted by each reviewer), the appearance of specific differences that need further clarification (as noted by Reviewer 1), and the possibility that promotion and tenure expectations for macro and micro scholars (as noted by Reviewer 2) may be different are issues you may wish to devote further attention. Related to this issue, I think the discussion of the rise of other outlets during this time period needs to be developed more fully. One possible explanation for why fewer scholars are reaching the "5 in 5" or "10 in 10" threshold is that publications such as *Org Science* and the *Journal of Management* have increased in stature and scholars are publishing in them, thus reducing the need to publish as many articles in the journals that constitute your sample (comments from Reviewers 1 and 2 get at this issue). I'm not saying that you need to include these journals in your sample, but I do think you need to address this issue more explicitly in some way.

3. Related to the micro vs. macro issue, what might be some potential implications for these distinctions in light of recent efforts to cross these boundaries such as the special research forum in *AMJ*'s December 2007 issue or Bamberger's FTE in *AMJ*'s October 2008 issue? Will these distinctions continue to be useful, and if so, for how long?

To revise your manuscript, log into <http://mc.manuscriptcentral.com/amle> and enter your Author Center, where you will find your manuscript title listed under "Manuscripts with Decisions." Under "Actions," click on "Create a Revision." Your manuscript number has been appended to denote a revision.

You will be unable to make your revisions on the originally submitted version of the manuscript. Instead, revise your manuscript using a word processing program and save it on your computer. Please be sure that there are no track changes present in your document.

Once the revised manuscript is prepared, you can upload it and submit it through your Author Center. You can also upload your Response to Reviewers at this point or you can cut and paste your document into the space provided.

NOTE: Shortly you'll be receiving an invitation to participate in our AMLE Writers Workshop in Chicago where if you accept the invitation you'll have opportunity to interact with editorial board member facilitators regarding issues you deem most significant in your revision effort. Please be watching for that invitation since participation in the workshop will have implications on the guidance in the following paragraphs. Please don't hesitate to contact me if you have questions as you contemplate this invitation and the revision effort.

IMPORTANT: Your original files are available to you when you upload your revised manuscript. Please delete any redundant files before completing the submission.

Because we are trying to facilitate timely publication of manuscripts submitted to the Academy of Management Learning & Education, we would like your revised manuscript and response to reviewers to be submitted within the next 90 days. If you feel that you need more time to revise, please contact me and give me an estimate of the time you might require.

Once again, thank you for submitting your manuscript to the Academy of Management Learning & Education and I look forward to receiving your revision.

Sincerely,

Dr. J. B. Arbaugh
Editor, AMLE
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Reviewer(s)' Comments to Author:

Reviewer: 1

Comments to the Author

This is an interesting, well-written paper. I think you bring up some important issues that will spark considerable debate, both in terms of what you have actually found and in terms of the policy implications (tenure, recruiting, pay, etc.). My comments are mainly aimed at tightening up the logic and clarity of what you have communicated and tested. I summarize my comments below based on the order I found them in the paper because these seem to be the points where the issue comes up -- I'd like to see you tackle all of these (very doable) and that is why they are presented in page order as opposed to any other prioritization.

abstract -- starting with the abstract, and wherever you say there are differences between micro and macro, please say what you actually found, not just that there are differences. This point will make the paper controversial (if defensible) and as such lends to the value of having it published.

p. 2 - publication norms - this term is pretty loaded and ambiguous in its meaning. I suggest that you use something like "authorship" or "average productivity of management scholars," in lieu of publication norms since you don't really ever look at what it takes to get an article published. You do talk later in the paper about norms for earning tenure, but this is different.

p. 2 - demographic shifts - I don't think you ever connected the dots later in the paper with this early observation. It sounds like your underlying logic is that the number of authors has increased, and so has competition, but unevenly across subdisciplines -- if there will be a radical decrease in authors then does this mean that competition will decrease? You need to help the reader understand this important facet of the landscape.

p. 3 - competing for space - I agree with the way you portray this context but would like to see you more completely describe it. For instance, it seems that the growth in number of competing authors needs to be mentioned here, as I don't think you get to this until much later in the paper. In fact, I'd like to see you get this paper to the point where you clearly and cleanly can contrast the level and nature of competition across the different periods of time. Perhaps here is where you start with something about number of authors (though this is different than potential authors) and the level of concentration or fragmentation (i.e., average papers per author and number of authors with more than 5 in 5 and 10 in 10). Then after you test your hypotheses you can present a side by side comparison of how this has changed (along the three dimensions).

p. 7 -- underlying process -- its about H2 that it strikes me that you might help the reader understand whether this is (1) simply a numbers game -- i.e., more competitors -- or (2) a change in the nature of competition from broad-brush to incremental research contributions. When our field was young, and there were relatively few scholars, in many way the field was more munificent in terms of research opportunities. Today, the field is much more topically fragmented, and most new studies offer only incremental, albeit highly sophisticated, advances. Replications too are not welcome in top journals, such that these later authors have to do new, incrementally novel things. The general context in which researchers find themselves seem to lend itself less to a few high-volume producers and more to a situation favoring many authors an topic and methodologically fragmented world. This AMR might be useful in thinking about how theory development and testing has evolved and thus contributed to the changing competitive landscape that you study. Fabian, F.H., 2000. Keeping the Tension: Pressures to Keep the Controversy in the Management Discipline. *Academy of Management Review*, 24 (2): 350-371.

p. 7 -- "increased munificence may moderate the negative effects" -- this reads like a hypothesis where there is an explicit interaction. I think you want to reword this so that it better maps to your main effect prediction. Besides, I'm not what the functional form of this would look like.

p. 7 -- micro/macro -- this makes sense and is interesting but somehow I'd like to see you better foreshadow how you eventually test this. You also talk about some of the fundamental difference between micro and macro publishing but don't talk about the fact that micro research has a higher number of co-authored papers (at least I assume it is a fact and your data can tell me if that is true). This difference in coauthorship would seem to have important implications for your results as well, since a paper with five authors would appear many more times than a paper with one or two authors (apologies if you dealt with this key issue but I missed it).

p. 10 -- micro/macro coding -- I think this is a great section and I'd like to see you foreshadow this stuff a bit starting on p. 7. When you talk about SMJ (one of my home journals) you might add a sentence that states: While SMJ does publish micro papers, any such papers that were not counted based on our heuristic would have made the micro results that much stronger. Or something like that. Your coding may slightly underrepresent micro papers, but your results would only be stronger if you coded each paper in SMJ.

p. 10 -- tautology? -- Can you help the reader better understand why, if you sampled more journals with micro papers why you would not find more micro authors? And wouldn't this problem be amplified in your data if there were more micro authors per paper? If this were a regression, its almost like I'd like to see you be able to say we controlled for the number of predominately micro and macro journals and still found that micro authors...

p. 11 -- "this procedure revealed..." -- I could not tell from this paragraph and later in the paper if you included all these papers in your analyses, or excluded most of the 840 articles/authors.

p. 14 -- middle paragraph following "insert Table 2" -- reiterate my question or concern about number of authors for micro papers. Across the sets of authors, how many actual unique papers were there? I know this won't be the case, but what if 177 = 5 authors on the same 35 papers and 76 is 2 authors on 38 papers -- it won't be the case but I hope you get my meaning. How many unique papers are there? It seems that competition is at the level of the paper, and if authorship varies systematically across micro and macro, then you aren't communicating or comparing apples to apples.

Reviewer: 2

Comments to the Author

I list my specific concerns for the manuscript below.

1) Given the journals selected (i.e. mostly micro in nature, it seems nearly impossible that H4 would not be supported given that JAP, OBHDP, and Psych are all exclusively micro.

2) I think an interesting, but unexplored point is whether academics have changed their research strategy in light of the increased expectations of research production. One strategy in particular, using more co-authors is a way in which researchers can increase their productivity, even if space in top journals is limited. This strategy may be particularly salient to macro researchers, given the extremely limited space available.

3) I found interpreting the results of the empirics used to test the hypotheses nearly impossible due to the fact that no tables or correlations or regression coefficients were offered. While the results section does reference the outcomes of some OLS regression models, the full results are never shared. Further, my understanding of the analyses performed suggests that OLS is not the appropriate regression model for the data being analyzed. My understanding is that the dependent variable in all regression models is a count variable. In such instances the regression models should employ either the poisson or negative binomial models, depending upon whether the data are over-dispersed (Cameron & Trivedi, 1986). Such models recognize that the distribution of count dependent variables cannot take on negative values. As such, I have little faith in the results reported using OLS and the analyses should be re-done using on the above models (with tables presenting the results, of course).

4) As it relates to the actual implications of this study the authors wrote, “the pool of suitable candidates for the micro position is nearly three times larger than the pool for similar macro candidates” (17). This makes the assumption that tenure requirements for the number of top journal hits is identical for micro and macro researchers and, in my experience, this is simply not true. Based upon some phone calls I made to research universities, macro researchers can get tenure with 4 or even 3 hits in top journals where the expectations are higher for micro researchers. I think this is a point the author(s) should mention and discuss.

Running some sensitivity analyses may help on this point. What happens when the micro researchers must publish 6 articles and macro researchers must publish 4? If the results still hold, I think the author(s) have a stronger case to make that the size of suitable candidates for each discipline are significantly different.

5) Further, the field has recognized the shortage of macro journals with the addition of Organization Science in 1990 (which, based upon my phone calls, is universally considered a top journal) and Strategic Organization (which is not yet considered a top journal, but may be in the future). While I realize the time period of the study did not consider top journals, to the extent that conclusions are drawn about the current state of academia, such changes must be recognized and discussed. Indeed, the founding of such journals seems to be a response to the findings of the current study.

6) Lastly, it seems that this article may not be a great fit for AMLE. Whenever I read an article and this suspicion hits me I always check the references to check the rate at which the target journal is cited. In this case only 3 of the roughly 30 references are from AMLE. This confirmed my suspicion and suggests that AMLE may not be the proper target for this article in its current form.

Overall, I think there is a potential contribution here, but the manuscript must be revised substantially in order to do so. In its present form it does not seem to fit AMLE very well, I am not confident the statistical analyses were performed properly, and I am dubious of some of the conclusions drawn in the discussion given the current state of the field.

References

Cameron, A., & Travendi, P. 1986. Econometric models based on count data: Comparisons and applications of some estimators and tests. *Journal of Applied Econometrics*, 1: 29-53.

Reviewer: 3

Comments to the Author

The authors seek to answer two questions. First, have competition and publishing activities changed over time? Second, do differences exist between macro and micro authors/activities? In order to answer these questions, the authors take an interesting approach identifying and analyzing the field of management research and the publication process as an industry. I suggest the authors build out this

analogy even further because it enters and exits the manuscript in an unstructured way. For example, publications are products of the industry and some products sell better than others. A simple citation count could address this. I read the paper with great interest and the enthusiasm of the authors' voice was evident throughout the paper. I offer the following comments:

- The link between numbers of publications and knowledge creating is not well established. The assumption is there but it may be a leap. Without a better understanding of the content in the papers, how often the research is cited, and discussing findings as they relate to actual practice, I don't think the authors can use the increase/decrease in number of publications (even in top-tier journals) as a proxy for knowledge creation. It may, however, be a proxy for academic productivity since you are looking at the authors that publish 10 (5) papers in 10 (5) years.
- The hypotheses seem like they were created after analyzing the data and seem forced into strategic management theory. If the authors better develop the industry analogy the hypotheses may have a better fit with the story of the manuscript.
- It's unclear what is macro vs. micro. It seems that strategy is perceived as the only macro discipline but I question the authors disregard for the subfield (and macro field) of entrepreneurship.
- Revisit when and where citations are used. Two examples to consider: 1) on page 3, last line why is Boyd et al., cited and unless you are talking about the specific legitimacy work of Hambrick then your use of the word legitimacy is broad and does not require citation; 2) page 4, line 39 the authors state, "Two recent changes in the institutional environment (i.e. Scott, 1995)." does not indicate what Scott did. Did Scott address the changes in AACSB? If yes, then cite at that time. These are just two examples but there are more throughout the paper. Please do not cite unless you are really building on or using a previous authors work to further your position or argument.
- The claim that journals have changed and grown both in articles and pages published. Have all the journals in your sample grown in both ways? Or have some grown in one way and not the other. In the 1980s article length was a lot shorter than article length today but this should not indicate that we are generating more knowledge today with longer articles. Does page space really equate with an increased level of munificence?
- The final sample included 17,650 authoring events representing 8,095 scholars (page 10). Let's remove the 840 who were not classified. This is 7,255 authors. The number of micro authors is 5436 or 75%. The number of macro authors is 1756 or 24% of the sample of 8095. Then, of course, 1% was both. The findings in support of Hypothesis 4 on page 14 are misleading. You state that 177 micro scholars and 76 macro scholars publish five articles in five years. From an absolute value perspective your findings make sense. However, 177 micro scholars is only 3% of the total micro sample in your data; whereas, 76 macro scholars is 4% of the total macro sample. So, normalizing the data using percentages may alter the results of your finding.

- It would be interesting to know how many authors stop publishing after 5-7 years given the tendency for faculty to stop producing after tenure. This may not be applicable to the higher journals and higher-level scholars but it would be interesting to test with your data.