

GENERAL COMMENTS TO THE REFEREES AND EDITOR:

We are very excited to have been given the opportunity to revise our manuscript, which we now entitle, "Competition and Scholarly Productivity in Management: Investigating Changes in Scholarship from 1988 to 2008," for AMLE. We carefully considered your comments as well as those offered by the three reviewers. Herein, we explain how we revised the paper based on those comments and recommendations. We want to extend our appreciation for taking the time and effort necessary to provide such insightful guidance.

The revision, based on the review team's collective input, includes a number of positive changes. Based on your guidance, we:

- Endeavored to improve the fit of the paper with the journal
- Provided a more interesting, yet balanced discussion of the study's results
- Clarified portions of the methodology
- Increased the journals and years considered in our study
- Added new tests to improve our understanding of the data
- Improved the paper's framing with management theory

We hope that these revisions improve the paper such that you and the reviewers now deem it worthy of publication in AMLE. Next, we offer detailed responses to your comments as well as those of the reviewers.

RESPONSES TO THE EDITOR'S COMMENTS:

1. COMMENT: 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:

RESPONSE: We are pleased, Dr. Arbaugh, that you offered us an invitation to revise our work for AMLE. Also, we truly appreciate you assigning such qualified Academy of Management Learning & Education reviewers to our manuscript. Their efforts and insights were a tremendous help to us during this revision.

2. COMMENT: 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.

RESPONSE: Thank you for this direction. We have followed through and now increase our use of management theory, primarily industry-level theories, to help frame our paper, along with strategic group research. Also, we have adjusted our introduction and references to better illustrate how our paper draws upon and follows the mission of AMLE. We hope that you find these revisions an improvement.

3. COMMENT: 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.

RESPONSE: We agree with your assessment. As such, we have endeavored to extend the paper's contributions by enhancing our discussion section. We now discuss the specific issues you highlight: farm schools, resource constraints, senior assistants, etc. Furthermore, we have also increased our consideration of management as a whole. While in the previous version we did not discuss how senior colleagues could champion junior scholars with different research foci, we now do so. In total, we think that the revised discussion is more interesting and balanced. We hope that you agree.

4. COMMENT: 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.

RESPONSE: After re-reading the manuscript with the reviewers' comments in mind, we agree with the suggestion that we redress some of our presentation, both in tone and empirical treatment. While you did not require us to do so, we have re-run our analyses to include, along with our previous journals, *Organization Science*. As you can see in our results section, there was no substantive change due to this inclusion. We do recognize that no journal "list" is perfect and that other avenues exist for a scholar to make important contributions. As such, we now acknowledge these realities as limitations of our work.

We also worked to improve the tone of the paper. We did not intend to project any potential divisiveness between the sub-disciplines. Thus, as we revised the paper, we worked to provide a more balanced presentation of our arguments and results. It is our intention, after all, to promote a discussion that will help management as a whole to continue its remarkable growth and importance in society. We hope that our revision, based on the input of the reviewers, accomplishes this objective.

5. COMMENT: 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?

RESPONSE: We find this an interesting query. We agree that some of the most important work management scholars can pursue spans levels of analysis, which requires the traditional delineation of research (micro/macro) to be blurred. This is in part why we have added more discussion of cross-area support. In addition, we refer to *AMJ*'s December 2007 issue as an example of how the line between these two sub-disciplines may become more obfuscated in the future.

CLOSING COMMENTS TO THE EDITOR:

Again, we appreciate the opportunity to revise our work for consideration for publication in *AMLE*. We hope our revision meet your approval. We next detail our responses to each reviewer's concerns and comments.

RESPONSES TO REFEREE #1's COMMENTS:

1. COMMENT: 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.

RESPONSE: Thank you very much for your kind words about our paper. We are delighted to hear that you think our work will spark debate in our field. In the following sections, you will find our responses to each of your points and suggestions. We are grateful for the time and energy you expended on our behalf.

2. COMMENT: 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.

RESPONSE: Thank you for this suggestion. In the revision, we attempted to describe differences between the two sub-disciplines in a balanced manner; one that promotes a healthy respect and discussion between the sub-disciplines within management. Nonetheless, we do maintain some of these arguments. Per your suggestion, we now include in the abstract how competition in macro is more pronounced than in micro.

3. COMMENT: 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.

RESPONSE: You raise a very valid point about the terminology we employed in our original submission. Per your advice, we have eliminated our references to norms and now use "scholarly productivity" instead. We agree that this is a more accurate description of the phenomenon we are exploring. Thank you for this great suggestion.

4. COMMENT: 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 sub-disciplines -- 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.

RESPONSE: You raise an important question regarding competition in our industry. To better assess how competition has changed over time, we performed supplementary analyses. Specifically, we calculated

for each year in our sample the number of unique authors publishing an article in the journals in our sample in the previous five years. In 1988, for example, we calculated how many individuals published an article between 1984 and 1988. This approach proxies for competition, because it assesses how many individuals compete for journal space. We found a substantial increase in the number of people publishing over time. In 1988, for example, we found that 1572 individuals published an article between 1984 and 1998. In the last year of our sample, 2008, we found that this number increased to 3158. This is important information, as we are now better able to discuss changes over time. With this addition we do not emphasize demographic shifts until we reach our discussion. We hope that our revised discussion now more clearly develops the linkages to which you refer.

5. COMMENT: 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).

RESPONSE: Thank you for this great suggestion. As we discussed in our response to your previous point, we do just this. In the revision we introduce Figure 2, which illustrates how competition has evolved over time in management as well as the subdisciplines. This new figure will allow readers to compare the figures in Tables 2a and 2b with the increasing competition. We might speculate, then, that achieving these milestones has become more difficult over time.

6. COMMENT: 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.

RESPONSE: Thank for you providing these ideas. We now explicitly cite Fabian's (2000) work and discuss more scholars trying to compete (numbers game) as well as increased sophistication and fragmentation

(intensity of incremental research). We think that this increases the clarity of the underlining processes directing the conclusions proposed in H2. We hope that you agree.

7. COMMENT: 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.

RESPONSE: Thank you for this point. In the revision, we have changed this wording.

8. COMMENT: 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).

RESPONSE: You raise an important point with respect to coauthorship over time. This was also a point raised by Reviewer 2. In our original submission, we tried to implicitly note this by distinguishing between each journal's articles and authoring events.

To more explicitly examine this issue, we now include Figure 3, which shows how coauthorship has changed over time for each sub-discipline. Thank you for this valuable suggestion.

9. COMMENT: 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 under-represent micro papers, but your results would only be stronger if you coded each paper in SMJ.

RESPONSE: This is an interesting perspective. We have added language suggesting that our coding scheme is not perfect. Our sense is that some micro researchers might argue that some of articles published in micro journals examine macro topics as well. At the end of the day, we are not sure that one group has an advantage in this respect. As such, we now include this issue as a limitation of our study.

10. COMMENT: 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...

RESPONSE: This is another good point. In the revision, we attempt to deal with this issue. First, we add Organization Science to our set of journals. Second, we try to do a better job highlighting the fact that

our results are substantively similar when we use JAP as our only micro journal. Relative to OBHDP and PP, JAP publishes a larger number of articles.

11. COMMENT: 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.

RESPONSE: These authors were utilized where appropriate. We could not determine their research foci, and the vast majority did not publish 5 articles in our sample journals. Thus, they were less useful for our analyses that examined differences between the sub-disciplines. However, we did include such authors when examining the management field as a whole (e.g., Figure 1). We hope that this makes our methodology more clear.

12. COMMENT: 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.

RESPONSE: This is another great point. As we indicated previously, we now explicitly document coauthorship practices with Figure 3. We think this information illustrates quite clearly that coauthorship varies by sub-discipline. Additionally, as we mentioned in our response to Comment # 4, we have added new analyses that investigates unique authors. Specifically, we calculated for each year in our sample the number of unique authors publishing an article in the journals in our sample in the previous five years. In 1988, for example, we calculated how many individuals published an article between 1984 and 1988. This measure proxies for competition, because it assesses how many individuals are competing for journal space. We found a substantial increase in the number of people publishing over time. In 1988, for example, we found that 1572 individuals published an article between 1984 and 1998. In the last year of our sample, 2008, we found that this number increased to 3158.

Again, we appreciate all of your insightful comments. We worked hard to be responsive to them. Thank you for taking the time and energy to help us improve the paper.

RESPONSES TO REFEREE #2's COMMENTS:

1. COMMENT: I list my specific concerns for the manuscript below.

RESPONSE: Thank you for all of your detailed comments and suggestions. We found them quite useful as we approached our revision.

2. COMMENT: 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.

RESPONSE: This is a good point that Reviewer 1 also pointed out. In the revision we attempted to address this issue in two ways. First, we expanded the number of journals in our sample to include Organization Science. Second, we attempt to underscore that we ran supplementary analyses where we included only one journal as our primary micro journal, JAP, and the results were substantively similar. Looking at the dispersion of journals over time, JAP represents a large majority of the articles published. So, we conclude it is not a merely our sample selection that matters, but instead our finding is representative of trends in the field.

3. COMMENT: 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.

RESPONSE: Thank you for this valuable comment, which was echoed by Reviewer 1. Based on this suggestion, we now include in our discussion section more discussion of coauthorship over time. In addition, we now created Figure 3, which illustrates how coauthorship for each journal evolved between 1988 and 2008. As seen in Figure 3, there is a slightly positive trend in terms of coauthoring. Thank you again for this valuable suggestion.

4. COMMENT: 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 overdispersed (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).

RESPONSE: Thanks for raising this important point. Since we included only one control variable in our models, we decided to include the results only in the text. We felt that this approach helped to conserve

valuable manuscript space. For your references, though, we are including the regression results in an appendix to these responses.

You are correct that our dependent variable is a count variable. As such, we should rely on Poisson or negative binomial models. In supplementary analyses that we now include in a footnote, both approaches yield virtually the same results. We decided to continue to rely on OLS regression in the body of the paper, however, because the coefficients make more sense intuitively (which we discuss). In contrast, Poisson or negative binomial results are expressed as likelihood values. We hope you find our approach acceptable. Thank you for the suggestion, because it helps a great deal to illustrate that our results remain consistent across a number of analytic techniques.

5. COMMENT: 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.

RESPONSE: We now elaborate on this point more in our discussion section. We are reluctant, however, to take a strong position on this point. In our experience working at research universities, there is not a sliding scale in terms of tenure requirements based on research focus. In fact, we also are not aware, based on our experience, with the publication rates that you specify for macro scholars being sufficient for tenure.

Furthermore, we are hesitant to change the assumptions of our process for the following reason. One of the objectives of our paper is to compare the relative productivity of macro and micro scholars. This becomes more difficult if one group is held to one standard while another group is held to a different standard. Moreover, we do not want to appear as if we are arguing for less rigorous tenure requirements for one group as compared to the other. Instead, we are reporting the data and hope the readers will reach their own conclusions.

6. COMMENT: 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.

RESPONSE: Per your suggestion, we have included Organization Science as a journal in our sample. We also now discuss the notion of new journals in our discussion section. Thank you for the great suggestions.

7. COMMENT: 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.

RESPONSE: Thank you for this comment. Thankfully the editor provided us with a great deal of guidance regarding how to better position the article to suit the mission of AMLE. Part of this guidance involved pointing us to related articles that have appeared in AMLE. We are hopeful that you agree that this revision is better targeted toward AMLE.

8. COMMENT: 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.

RESPONSE: Thank you once again for your valuable comments and suggestions. We are hopeful that our supplementary analyses and revised focus helps to improve your opinion of work.

Appendix: Full Regression Results

OLS

Regression

Poisson Negative

Binomial

Intercept -21356*** -22.581*** -22.456***

(2566.96) (3.196) (2.760)

Year 10.70*** .014*** .014***

(1.31) (.002) (.001)

Munificence 1.95*** .002*** .002***

(.19) (.000) (.000)

RESPONSES TO REFEREE #3's COMMENTS:

1. COMMENT: 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:

RESPONSE: We appreciate you taking the time to offer us your comments and insights related to the paper. We found your feedback very constructive. We tried to be responsive to your concerns. For example, we have improved our analogy of management as an industry. We hope you find these revisions rise to your expectations.

2. COMMENT: 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.

RESPONSE: Thank you very much for this insightful comment, which was also referenced by Reviewer 1. We agree with your assessment that there is perhaps a subtle difference between knowledge creation and publishing papers. Per your suggestion, we have changed our references of "knowledge creation" to "scholarly productivity."

3. COMMENT: 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.

RESPONSE: We are appreciative of this comment. Per the editor's advice, we have attempted to tighten the linkages between our area of inquiry and management theory. In addition to developing the role of industry in understanding scholarly productivity, we have also delved even deeper to use existing research on strategic groups to understand differences between micro and macro.

4. COMMENT: 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.

RESPONSE: We agree that the distinction between micro and macro is sometimes difficult to understand. We agree that the field of entrepreneurship is important, and two of the authors on this team have published in such journals. Nonetheless, we elected to omit entrepreneurship journals from our sample, because we are not aware of any research suggesting such journals are "top-tier" journals in

the field of management. We do touch on the entrepreneurship field in our discussion section. Thank you for the idea.

5. COMMENT: 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 AACB? 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.

RESPONSE: We apologize for the confusion. We have gone through the paper and eliminated unnecessary references. We hope that you find our references are now effectively used.

6. COMMENT: 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?

RESPONSE: Unfortunately, we do not have data on the length of each article. Because we believe that most decisions in business schools (tenure, promotion, etc.) rely on articles as opposed to pages published, we focused exclusively on articles as opposed to pages.

7. COMMENT: 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.

RESPONSE: This is a great point and one that made us think a great deal about our findings. Due exclusively to your comment, we performed supplementary analyses that we think provide one of the primary contributions of our work. (You will note that the numbers differ from those in your original comment because we added Organization Science to our list as well as all articles published in 2008.)

Specifically, for each year in our sample we attempted to create a proxy of the competition in the publishing industry. To do this, for each year we calculated the number of individuals publishing an article in one or more of the journals in the previous five years. In 2008, for example, we calculated the number of individuals who published at least one article in one of the journals in our sample in the five year window ending in 2008.

This procedure revealed several important points. First, competition in our industry has increased markedly over time. In 1988, 1572 unique authors published an article. In 2008, 3158 unique authors did this. At the same time—and as we pointed out in our original submission—the number of authors reaching these milestones has remained relatively flat over time. Normalizing the data in this way shows that achieving these milestones has become more difficult over time.

We also distinguished between micro and macro in this analysis. As you pointed out, this allows us to show that on a percentage basis the differences may not be as stark as we initially believed. This is important to show. In addition, though, we were able to see how competition has increased in each subgroup. In macro the number of individuals increased from 291 to 894. In micro, this number increased from 1085 to 1885.

This was an important addition to the paper, and we thank you for helping us conceive of it.

8. COMMENT: 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.

RESPONSE: We agree that this would be an interesting question, but our data are not well-suited to examine this question. For example, we now state in the paper that these journals are not the only way to contribute to the field. Accordingly, if we identify those that stop publishing, we may be actually misleading what these scholars contribute in terms of books and other publications. Nonetheless, because books will not lead to positive tenure decisions for most junior faculty we think that our investigation of trends looking from a scholar's entry into the field makes sense. We hope that you agree. Future research might look at transitions in scholars' productive efforts over their career.

Again, we appreciate all your insightful comments. We worked hard to be responsive to them. Thank you for taking the time and energy to help us improve the paper.