Article details: 2017-0095	
Title	The relationship between publication rate and financial conflict of interest among biomedical authors of high-impact oncology publications: and observational study
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Reviewer 1	Dr. Joel Lexchin
Institution	York University, School of Health Policy and Management, Toronto, Ont.
General comments (author response in bold)	This article is an interesting look at the relationship between academics and the pharmaceutical industry as it relates to their productivity when it comes to publications. **Thank you.** 1. One of the conclusions by the authors is that receiving payments from drug companies does not marginalize the productivity of researchers but is that conclusion equally true for both research-based publications and for other types, e.g., commentaries, editorials, etc. I would suggest that the authors consider doing a secondary analysis by looking at the association between payments and different types of publications. **Thank you, this is a great idea, which we will pursue in a future paper. Because the reality is it would require coding 20,000 articles as one of these types. Having looked at the data my feeling is that it will be research papers, but not the other 2 types simply because most of these papers are research papers, and I don't think we will have power to show it for the others. But that is a gut feeling, which we can follow up on in the future.** 2. Page 5, line 37: The Cochrane review (reference 11) has now been expanded and updated. **Thank you, and this paper is a great work! I have updated our manuscript**. 3. Page 5, lines 48-56: The issue about academic scientists ties with industry and research productivity has been studied in the past and the authors should refer to the paper by Zinner et al. Health Affairs 2009;28:1814-25. **We have added this wonderful paper and reference.** 4. Page 6, lines 35-47: How were the journals searched - using PubMed, hand searched, etc.? How many people extracted the data, how were conflicts resolved? **We add this"** **These journals were hand searched by two reviewers and differences resolved by discussion."** 5. Page 7, lines 37-39: How accurately does Scopus record an author's publications? The evidence is limited, but shows 98-99% accuracy (cite: https://link.springer.com/article/10.1007/s11192-015-1580-z) **We have added this to the paper.**
Reviewer 2	Dr. Barbara J Mintzes
Institution	University of British Columbia, School of Population and Public Health, Vancouver, BC
General comments (author response in bold)	This is an interesting analysis that extends the research on financial links between pharmaceutical industry and physicians to an issue than has been covered previously, specifically whether there is an association between financial conflicts of interest and publication rate. The interpretation is complicated by the fact that a person's research funding, regardless of source, would reasonably be expected to be associated with their publication rates. **Correct, we attempt to adjust for this by adjusting for research funding as a covariate in our primary finding.** Additionally, the direction of causality remains unclear: are pharmaceutical companies choosing physicians to fund who are more publishing more, or is industry funding leading to a higher publication rate? Finally, the issue of ghost and guest authorship has been widely discussed and its relationship to this study remains unclear, as the authors do not undertake any secondary analyses that might allow them to look more specifically at this question. **This is correct. Although I believe ghost authorship is common, I do not think it has been studied adequately. I am aware of gift or ghost-authorship, where the admission is made only privately. I am not sure anyone has mapped the size of this problem fully. Additionally, we do acknowledge possibility of reverse causality, but attempt to correct for it by adjusting for prior body of work.** I had a few general comments and questions: 1) the choice to focus on articles only within oncology and haematology requires better justification, as well as how original articles were defined (specific study designs

only such as RCTs? Or any original empirical study?). Was the selection process carried out using duplicate independent coding or by a single coder?

The reason for focus on hematology oncology is, these issues have a particular import in oncology, where many decisions are made in the gray zone—without clear data—and conflict may play a role. We have added this explanation to the manuscript with a reference to support it.

2) Related to this, a flow chart/ PRISMA-type diagram for the selection process would be useful, as it is impossible to guess from the text numbers of included articles, proportion of first and last authors who met inclusion critera, etc.

Thank you, we did not collect this information. PRISMA applies to systematic reviews of published literature, and this is merely the construction of a dataset. It is similar to papers like this (https://jamanetwork.com/journals/jamainternalmedicine/fullarticle/2532788) which do not provide such a figure. We are going through journals by hand, and enter names in the spreadsheet that meet our criteria. It is very straightforward. We do now add an explanation of who did this work.

3) Was the rate of 'general payments' correlated with research related payments? Did general payments clearly exclude any research-related payments? A Pro Publica report has shown an association between research payments and general payments: https://www.propublica.org/article/double-dip-doctors-paid-to-advise-promote-drug-companies-that-fund-research
Did you find a similar association between general and research payments from companies?

Yes, they are correlated and yes they exclude each other. As noted, others have explored this finding.

4) A number of other characteristics of the study report and of the author that might link it to guest/ghost authorship could also be examined, as the link between numbers of publications and being an academic 'guest author' warrants further investigation in this type of study, especially given the focus on a high revenue treatment area for the industry. Several potential indicators come to mind:
- outliers: authors who have published such large numbers of publications that they could not have possibly written or contributed to them all.
- authorship in studies with pharmaceutical industry co-authors;
- authorship in studies that acknowledge a medical writer's assistance.

These are all great suggestions for future papers. Dr. Prasad has personally called for universities to audit researchers with more than 50 papers a year.

I do want to mention that although I view the second 2 criteria suspiciously, as does the reviewer, many academics will deny ghost-authorship even when receiving writing assistance/ industry co-authors. I am not sure how one can prove the ghost nature of the authorship without examination of email & computer history.

5) in considering potential confounders, I was surprised that gender was not considered as well as date of medical school graduation (presumably a proxy for age). Is it possible to include gender within your analysis? Additionally, a table would be helpful showing the association of publication rates from 2013-2016 with prior publication rate, medical graduation date, seniority etc. in order to provide more context for your results.

We did not extract gender, and to do so would require profiling people based on their names, as we do not have access to gender. We have explored some of those basic correlations, but in a manuscript with 5 exhibits, I worry that too much data will miss the key message.

6) Figure 2 appears to indicate that relatively little of the variability in publication rate is explained by the amount of personal payments received. Related to this, the distribution of payments is clearly not normal based on figures 2, 3 and 4, and it is unlikely that there is a continuous linear relationship between payment rate and publication rate, with a single unit of extra payments having an equivalent effect on the outcome regardless of how much a doctor is paid in total. I also wondered about the decision to omit outliers, as these can be informative. It seems from Figure 2, that some people are listed as first or last author on more original articles than they could have possibly researched and written in 4 years.

I totally agree that these data suggest some people are doing more than possible. I am very sure that can only mean trivial contributions not meeting author criteria. This idea is so good, for a future paper, we will look at oncology papers and ask who is doing more than possible. I also agree the relationship may be non-linear, but my statistician colleagues caution that we may worry about over-fitting if we apply other terms.

7) I did not find figures 3 & 4 that informative, and would suggest providing a simpler single figure illustrating payment distribution (with 0 set at 0, rather than the median payment) and tabular results.

Zero is set at 0 in all figures. I sympthathize with this comment, but note that our figures are very similar to Figure 1 & 2 in this recent JAMA IM paper that is a similar exploration of financial conflict (https://jamanetwork.com/journals/jamainternalmedicine/fullarticle/2520680). We wanted to parallel other high quality publications

Some specific minor suggestions:

8) The title should refer to publication rate and not 'career success' as the latter has not been directly measured.

Absolutely, this has been changed. Great pick up.

9) The difference between the unadjusted and adjusted estimate for the increase in publication rate suggests that there was substantial confounding. I would therefore recommend listing the adjusted estimate in the abstract, as a likely more accurate estimate of the independent effect associated with funding, and also highlighting it in the results and conclusions,.

We do list it in the abstract. See last line of results in the abstract. I agree

10) the updated Lundh et al. 2017 Cochrane review, which includes 75 papers, should be cited;
Done.

11) on page 6, line 47, if you extracted both the first and last author (as appears to be the case) this should be "the first and last author" (not first or last)
Correct, sorry for the typo.

12) on page 7, you state that payments were recorded for 2013-2015 because "other years are not included in the Sunshine Act's disclosures". This could be misread as suggesting that these disclosures have stopped after 2015.

We replaced other with “prior.”

13) on page 9, when discussing research payments, please state, "median amount of research payments from pharmaceutical companies", as this is unlikely to represent total research funding received; it only reflects Sunshine Act reports.

Great point. I have added this, “and pharmaceutical company research funding reported by the Sunshine Act.”

14) The description of the beta coefficients on page 9 for the unadjusted association between prior publication rate and publication from 2013-2016, and general payments versus publication rate are reported differently, with the former reported with a confidence interval, and the latter without, and also the former not stated in terms of numbers of papers (e.g. if the unit is similar, it would be 0.19 additional papers in this time period per additional paper published before 2013).

Thank you for this. We added the confidence interval. The reviewer is 100% correct that is the interpretation of the Beta coefficient for papers before 2013 predicting papers 2013 to 2016. I agree completely.

15) as noted above, the adjusted analysis for payment versus publication rate should be highlighted more than the unadjusted analysis; additionally, if there is an interest in looking at prior versus current publication rate, similarly an adjusted analysis should be presented with seniority and years since graduate and ideally sex included.

I have added a sentence so it is equally highlighted. I agree with the reviewer that this is the true effect.

The interpretation in the discussion that it is reassuring that "the acceptance of industry payments is not associated with fewer publications" seems odd, given the finding of increased payments being associated with more publications. It is unclear why this finding would be interpreted as meaning anything about prohibitions not hindering researchers' careers.

**In the context of the paragraph, I think our quote makes sense; See
“Some are concerned that current policies against conflict of interest, which largely are confined to forms of disclosure may be used to cast aspersions upon conflicted biomedical researchers23. Major journal editors have been critical of policies that restrict review or editorial articles to authors free of financial conflicts24. Our results provide a reassuring note. The acceptance of industry payments is not associated with fewer publications; thus prohibitions against financial conflict of**

	interest are not presently so daunting they hinder researchers' careers. Further prohibitions may be considered." The analysis does not separate out authors from medical faculties that prohibit certain types of personal payments from others, so does not address this issue. **I am not aware of any center that has such a prohibition, is there one?** A commonly voiced opinion among those who are supportive of strong financial links between industry and the medical profession would be that industry payment is a reflection of expertise - it's often stated for example by those who are opposed to restricting conflicts of interest on guideline committees or advisory committees that this would be akin to excluding all experts. The most important finding that counters this opinion in this paper is that prior publication rate was a much stronger determinant of future publication rate than industry funding. **I think this is one interpretation. The other interpretation is that even after you adjust for conventional measures of expertise (prior papers, funding and seniority), taking money still means more papers—so it is not just financially good to work for industry, it helps your career.**