**Peer Review and Communication History**

**Ms Title:** A Call for Greater Modesty in Psychology and Cognitive Neuroscience

**Author Names:** Richard Ramsey

**Submitted:** Jan 10, 2021

**Editor First Decision—Revise & Resubmit**

Mar 29, 2021

Dear Rich,

I have now received all reviews of your manuscript, “A call for greater modesty in psychology and cognitive neuroscience” from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript makes important points, and also believe that it could benefit from considering the points raised by me and the reviewers below. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology. I do not plan to send your revision back out for review.

The reviewers provided clear and thorough reviews. I will add a few points below. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers’ comments, outlining each change made in your manuscript or providing a suitable rebuttal.

1. I agree with reviewer 1 that the parts of your manuscript having to do with values and value judgments were a bit vague and hard to connect to the rest of your mansucript. Related to this, I had a hard time reconciling your advice to be explicit about our values and value judgments with your advice that we should not propose that one approach to doing science is right. I can see a difference between the two, but I wonder what a reader should take away if, for example, they believe that one approach is better than another. Should they just be more explicit and make the case for this value judgment, or should they be more modest and refrain from taking such a position?
2. I thought that one of the more intriguing and unique aspects of your manuscript is that you call for greater modesty not only in empirical research, but also in policy/reform proposals. Reviewer 2 (point #6) wanted you to take more concrete positions on specific reform issues. I agree this would add to your manuscript, but I will leave it up to you whether you would like to take your manuscript in this direction (right now, this is one of but not the main point of the paper).
3. I was not as bothered as the reviewers by your relatively quick handling of the definitional issues (Reviewer 2) and the incentives issue (Reviewer 1). On the definitional issue, I think there will likely be a shared common-sense undertsanding of modesty in the scientific domain among readers. I do think that Reviewer 2’s point number 2, however, would really strengthen the paper (connecting the concept of modesty to concrete reasoning practices in science). On the incentives issue, I fear that there is not much more to be said about incentives than what has already been said, and I think there is a tradeoff between placing too much emphasis on the incentives/structural issues to the extent that it may let individual researchers off the hook more than is warranted. That said, I completely agree with Reviewer 1 that incentives are one of, if not the, biggest obstacle to modesty in science, so if you do have more ideas about how to deal with that than you currently present, I don’t want to stop you from including them!
4. Regarding Reviewer 2’s points 5 and 7, I think some of the difference in views about how bleak things are in the psychological and brain sciences might come from differences in personal experience. I know that I often struggle to justify my bleak view of the state of the literature in my subfield sometimes, and while I can point to metascientific results such as failures to replicate, low rates of transparency, etc., that doesn’t fully capture the reasons for my less-than-rosy take on things. I think Reviewer 2 makes an important point that, to many readers (especially those that have had different experiences than you and I likely have had), your somewhat negative characterization of what we/science is capable of might need more justification.
5. Another perspective to consider is the view that science advances not because individual scientists strive for objectivity and are cautious in their claims, but by having scientists with competing theories/positions argue for their preferred explanation without any attempt to be dispassionate. The idea is that by pitting the best arguments, and counterarguments, each “side” can come up with against each other, the best theory/explanation will win out. I don’t have a great reference for this position, unfortunately. The only paper I can think of that presents something like this argument is Mitroff’s (1974) “Norms and counter-norms in a select group of Apollo moon scientists”. But I think others who wrote arguing against Merton’s norm of “disinterestedness” also made similar points. I’m not saying I agree with this view (I don’t), but I think it might be worth considering how you’d respond to this type of argument.
6. I had a few thoughts go through my head as I was reading your paper that I don’t think are worth presenting to you in my role as editor of the piece, because it’s an opinion piece and should reflect your opinion and not mine, but may be useful to you as “thoughts from a reader who is pretty far on the reform side of the continuum”. Feel free to take or leave these reflections - I’m only presenting them here in case they might be useful to you, but you do not need to respond to them in your revision or cover letter. -On page 7, you describe some activities of the reformers as “sending the message, whether intended or not, that if only we use p-values appropriately or correct for bias in meta-analysis (for example), all our problems will disappear.” I agree that a lot of reform suggestions come across - or at least are received - that way. I have struggled, however, to figure out how to avoid readers intrepreting reform discussions this way. Do you think this is inevitable, i.e., that if someone writes a paper proposing X reform, even if they make it clear that they don’t think it’ll cure many problems, but just alleviate one specific, narrow problem, it will be interpreted by at least some as a claim that X reform will solve all problems? Sometimes it seems to me that the way reform papers are read is quite divorced from how they are written, and I would love to know what advice you have for how authors could do more to prevent this kind of interpretation. -On page 13 you write “I cannot see how any one individual, subfield, or organisation within psychology or brain science gets to decide what the “right” lines of investigation are to study the human brain.” I’m not sure I understand this point (and similar points made in one or two other places. Surely, there are individuals, subfields, and organisations that have to make decisions (e.g., about whom to hire, what to fund, where to place resources, etc.), that depend in part on their assessment of what the “right” approach or line of investigation is. Moreover, in our roles as reviewers and critics of each other’s work, we often make the case that another approach is better than the one used in the paper we are critiquing. Should such decisions or critiques not be allowed? If they are allowed, what are the boundary conditions on when we can make judgment calls about right or wrong (or at least better or worse) lines of investigation?

Minor points: -p. 3 “For an alternative example” - this seems like a similar, rather than alternative, example. -p. 10 - is the main claim made in the full paragraph on this page controversial? It seems most people would agree that “any signle piece of basic research may provide valuable advances to knowledge without leading to any such practical value.” (This is also related to Reviewer 2’s point 3 that in some places your advice was quite general, in a way that may make it seem uncontroversial or consistent with received wisdom in the field. If you think it goes against current norms, it would be helpful, here and elsewhere, for you to push the point a bit further to illustrate where you think it goes against the grain.) -p. 14 "“we that we could learn” should be “that we could learn” -p. 14 “I remain a novice is” should be “I remain a novice in” -p. 16 “innovative work became a lot harder to do in the last 5-10 years” I would argue that it didn’t become harder to do, we just became more aware of how hard it is to do.

In summary, I think this is a promising manuscript and I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please contact the editorial office at [editorialoffice@collabra.org](mailto:editorialoffice@collabra.org).

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Simine Vazire

**Reviewer 1**

**Open response questions**

Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

This paper outlines a compelling argument in favor of greater modesty and humility by researchers in psychological and brain sciences. I am in full support of the overall goal of the paper, but I do have a number of suggestions that I hope will help tighten the paper.

* It seems to me that the elephant in the room is incentives, which are discussed but only briefly. Currently, the need for hype suffuses the entire ecosystem. Grant officers are incentivized to hype the work that they fund so that the politicians who support them can crow about it. Journals are incentivized to hype the work they publish so that people will cite it and pump up their impact factor. Universities are incentivized to hype the work by their faculty so that donors will give more money. I think that a sober discussion of the difficulties of change in light of these overwhelming incentives would be useful.
* It was not quite clear to me how 2.2 fits with the rest of the paper. In part, I found it somewhat ineffective due to the vagueness about what “value judgments” might mean. I think that if you wish to retain this section, it would be useful to provide some more specific details about the kinds of value judgments you are referring to and exactly how they would play out.
* I found the brief discussion of philosophy of science in section 2.3 to not add much; the comparison to physics is somewhat facile, given the large amount of philosophical work on the nature of theory in psychological and neuroscience. I’d probably just drop this.
* I think that there are very good examples from neuroscience of projects that have enabled the large-scale aggregation of knowledge, such as neurosynth, which might bear mention in Section 3.3
* I don’t understand this claim: “Cognitive neuroscience researchers who complete training studies, for example, which require vast hours of training per participant plus repeat and costly neuroscience techniques (e.g., Apšvalka, Cross, & Ramsey, 2018; Cross, Hamilton, & Grafton, 2006), may still feel that this work provides sufficient value because it brings insight into neural plasticity, which few other approaches can provide. Such value again, however, may come at the cost of reduced certainty.” Why would this particular kind of study (if performed in a rigorous manner) necessarily provide reduced certainty? And reduced in comparison to what?
* At several points in the paper, statements are made to suggest that it may be impossible to understand the mind and brain: e.g. p. 19, “it may be fundamentally impossible for humans to understand the mind and brain, at the level at which we wish to understand it”. Ironically, these statements seem quite hyperbolic and lacks any real evidence. I’d suggest tempering or removing them altogether.

Signed, Russ Poldrack

**Rating scale questions**

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  | ✔ |  |  |

**Reviewer 2**

**Open response questions**

Please write your review here. The author(s) will see this review. Your identity will not be revealed to the authors unless you also include your name (i.e., sign your review) in this box. It is up to you whether to reveal your identity or not, either is fine.

Review of ‘A call for greater modesty in psychology and cognitive neuroscience’.

This is an interesting article on an important, but neglected, topic. Its main aim is that “in addition to methodological reform, greater modesty is required across all levels – from individual researchers to the systems that govern science … to redirect expectations regarding what psychological and brain science can effectively deliver.” The article draws from a wide array of relevant literatures and makes a number of plausible recommendations in this regard. It is very well written and clearly structured.

Although the article has merit, I do not believe it is entirely successful. My major reservation is that the author does not articulate, in an informative way, what modesty is. The result is that the idea of modesty is does not properly animate the author’s work. I note that the author refrains from giving a detailed treatment of a number of the topics deals he deals with, and the recommendations he makes. Perhaps, this is because of the broad-brush nature of the article, but I was left feeling somewhat dissatisfied. It is commendable that the author is informed about a wide array of relevant literatures, but the content of many of the references provided is not employed. I think that this is a general problem with present-day scholarship.

I recommend revision and resubmission of the present article, in light of the feedback contained in my review.

I should acknowledge that in July/August last year, I reviewed an ancestral version of this paper for another journal. It was rejected. Although he author received extensive feedback from three reviewers, the present submission is essentially the same manuscript, with just over two pages of additional text, and three quarters of a page of references, added. Because the present manuscript is not substantially revised, my suggestions for change are largely those that I made about the first version of the manuscript. I have not considers the author’s treatment of the feedback from the other two reviewers. I should also say that I now know the identity of the author.

The following comments indicate a number of ways in which I think the manuscript can, and should, be improved:

1. My major reservation stems from the belief that the key idea of modesty needs to be properly developed. It seems to function as a primitive concept in the present article, and cannot carry the cognitive load that it might reasonably be expected to bear. The author cites the philosophical work of Roberts and Wood (2003), and Whitcomb at al., (2015) on humility, but does not make any use of those works, apart from stating that intellectual humility in science is a strong stance to adopt – an interesting point that I think deserves to be expanded on. Also, the use of the terms ‘intellectual humility’ in the third major section heading (‘How do we embrace intellectual humility?’, p. 11), raises the relevant question, how do the notions of humility and modesty relate to each other? The author references Burton (2018), and Bommarito (2018) on this matter, but relegates his treatment of the topic to a 5-line footnote. I urge the author to develop a decent, and workable, concept of modesty (humility?). More than this, questions such as whether modesty is intellectual virtue, and how it connects with related virtues might be addressed, as might the question of whether modesty is a disposition to think and behave in characteristic ways.
2. I also think that the concept of modesty needs to be developed in a way that engages with the sorts of scientific reasoning on finds in scientific practice. These might include, but not be limited to, judging the strength of inferences made (e.g., those contained in plausibility judgments as distinct from truth claims); exercising the principle of charity regarding an opponent’s viewpoint; engaging in comparative theory evaluation; employing strong statistical hypothesis testing (e.g., as recommended by the error-statistical perspective); and so on.
3. On page 8, the author sensibly asks researchers to adopt more realistic expectations of their work by showing how it fits into a bigger picture of understanding. He does this by referencing a number of very recent papers on theory construction that he thinks provide helpful blueprints. Unfortunately, he does not make use of any of the substantive content of these papers. I think that this is a major omission, because the reader is left with nothing more than the general claim that we should improve our theory building. Providing an informative example of at least one blueprint, and showing how that blueprint serves as a perspective for more specific work, would strengthen the paper.
4. On page 9, the author states that “it is not enough to focus only on the kind and quality of data”, and maintains that aims and context need to be considered as well. I find it puzzling that the author is intent on contrasting data with values judgments in this section of the article (judging data to be good involves methodological value judgments). I agree that broader factors such as aims and context are important (I would also add methods, theories and institutions here), but I think it would be more appropriate to contrast data with their natural methodological contrasts of empirical phenomena and explanatory theory.
5. In arguing for “a more sober view of our capacities and achievements” (p. 2), the author speaks of “the feeble nature of our abilities” (p. 5). I think that more needs to be said on this score to be convincing. For example, how does this claim sit with the fact that science continues to make genuine progress, even in the face of irreproducibility? The author cites Shiffrin et al. (2018) in this regard earlier in his article, but offers little comment on it. Relatedly, how is it that these feeble abilities combine to offer us a way forward with the collaborative team science that the author recommends? Further, is it really the case that the extensive research on the nature of human abilities paints a dismal picture of human intelligence?
6. The author indicates that he will address the matter of whether claims made by the methodological reformers are suitably modest. Although he is “greatly encouraged by the reform agenda”, I do not think that he gives this matter sufficient critical attention. The following are examples of what I have in mind:

* Are the claimed low levels of reproducibility as low as commonly thought, given that research often employ forms of reproducibility that are not named as such? Even if levels of reproducibility are (unacceptably) low, might not the use of other validation strategies, such as methodological triangulation, paint a more encouraging picture of success?
* Regarding the ongoing debates about p values (and tests of statistical significance), is the position of the so-called “new statisticians” referred to immodest, given its strong insistence on prioritizing effect size estimation over of p-value reporting? I note that the primary advocates of the new (frequentist) statistical movement have not publicly responded to the major criticisms that have been levelled against it (e.g., the Bayesian new statistics).
* Is it not the case that some of the mainstream understanding of questionable research practices needs to be questioned, or at least finessed? A good example of this is Rubin’s (2017; 2019) fine-grained, and informative, analysis of HARKing, which convincingly argues that there are different forms of HARKing, some of which have a legitimate place in good science.
* The author refers to the contrast between exploratory and confirmatory research. Is our standard understanding of this contrast adequate, and does it justify claims, such as confirmatory research without preregistration is confirmationally worthless (e.g., Wagenmakers et al., 2012)?
* What might “a more systematic approach to the theory and method cycle” be? The author is not forthcoming on this. Despite referencing an appropriate literature on the topic, the author draws little from its content. I think that this a general problem with modern scholarship.

1. The author’s claim that “in many ways, aspects of modern psychology and brain science resemble a creative writing class more than a systematic science of brain or mind”, strikes me as exaggerated, and unfair. I do not think the claim follows from the research ills listed immediately before it is made. At the very least, I think that it is incumbent on the author to provide a decent justificatory link here.

Minor Matters

1. Page 10, fn 2: Given the heavy emphasis on effect sizes these days, I don’t think that its basic characterization is needed.
2. Middle of page 8: Correct “As one example approach”
3. A number of the articles on theory construction that are cited are now in press with Perspectives on Psychological Science.
4. The Fried article on theory building is now published in Psychological Inquiry.
5. The Yarkoni reference is now in press with Behavioral and Brain Sciences (2021).

Brian Haig Professor Emeritus School of Psychology, Speech & Hearing University of Canterbury University Drive, Ilam Christchurch 8041 New Zealand

24/02/2021

**Rating scale questions**

|  | Strongly Disagree | Disagree | Neutral | Agree | Strongly Agree |
| --- | --- | --- | --- | --- | --- |
| The study/studies in this manuscript have strong construct validity (good measures and/or manipulations of the constructs the authors wish to study). (Choose “Neutral” if this is not an empirical manuscript) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong statistical validity (appropriate statistical tests, assumptions are clear and reasonable, no statistical errors, appropriate statistical inferences, etc.). (Choose “Neutral” if this is not an empirical manuscript) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong internal validity (any causal claims or implications are well-justified, alternative explanations are thoroughly considered, etc.). (Choose “Neutral” if this is not an empirical manuscript, or no causal claims are made or even vaguely implied.) |  |  | ✔ |  |  |
| The study/studies in this manuscript have strong external validity (authors appropriately constrain their conclusions based on the limits of the generalizability of their findings to other contexts (including from lab to real world), other populations, other stimuli or measures, etc.) |  |  | ✔ |  |  |

Author Response

May 10, 2021

Dear Professor Vazire,

I write to submit a revised paper entitled “A call for greater modesty in psychology and cognitive neuroscience”.

Please find included a revised manuscript with all changes tracked using track changes. I also include a point-by-point response to the comments raised by you as well as the two reviewers.

Finally, I would like to thank you and the two reviewers for taking the time to read the manuscript so carefully and provide such detailed feedback. I look forward to hearing from you.

Yours sincerely,

Dr. Richard Ramsey

I would like to thank the editor and the reviewers for their thoughtful and constructive comments. In the following, I respond to all of the points raised.

Dear Rich,

I have now received all reviews of your manuscript, "A call for greater modesty in psychology and cognitive neuroscience" from qualified researchers. I also independently read the manuscript before consulting these reviews. I agree that your manuscript makes important points, and also believe that it could benefit from considering the points raised by me and the reviewers below. I therefore encourage you to submit a revised version for further consideration at Collabra: Psychology. I do not plan to send your revision back out for review.

The reviewers provided clear and thorough reviews. I will add a few points below. In your resubmission, please include a document with a point-by-point response to both the points I list here and the reviewers' comments, outlining each change made in your manuscript or providing a suitable rebuttal.

1. I agree with reviewer 1 that the parts of your manuscript having to do with values and value judgments were a bit vague and hard to connect to the rest of your mansucript. Related to this, I had a hard time reconciling your advice to be explicit about our values and value judgments with your advice that we should not propose that one approach to doing science is right. I can see a difference between the two, but I wonder what a reader should take away if, for example, they believe that one approach is better than another. Should they just be more explicit and make the case for this value judgment, or should they be more modest and refrain from taking such a position?

Response: I agree that this section could have been clearer. I have now substantially re-written this section (pages 10-13). I now provide more concrete examples on what I mean by value judgements and how they might be used, as requested by Reviewer 1. I can also see the tension that you outline between value judgments, taking a critical position and modesty. I have tried to make my position on these points clearer in the revised text.

1. I thought that one of the more intriguing and unique aspects of your manuscript is that you call for greater modesty not only in empirical research, but also in policy/reform proposals. Reviewer 2 (point #6) wanted you to take more concrete positions on specific reform issues. I agree this would add to your manuscript, but I will leave it up to you whether you would like to take your manuscript in this direction (right now, this is one of but not the main point of the paper).

Response: I have made various changes throughout the manuscript that respond to Reviewer 2 (point#6). I detail these changes in my response to Reviewer 2.

1. I was not as bothered as the reviewers by your relatively quick handling of the definitional issues (Reviewer 2) and the incentives issue (Reviewer 1). On the definitional issue, I think there will likely be a shared common-sense undertsanding of modesty in the scientific domain among readers. I do think that Reviewer 2's point number 2, however, would really strengthen the paper (connecting the concept of modesty to concrete reasoning practices in science). On the incentives issue, I fear that there is not much more to be said about incentives than what has already been said, and I think there is a tradeoff between placing too much emphasis on the incentives/structural issues to the extent that it may let individual researchers off the hook more than is warranted. That said, I completely agree with Reviewer 1 that incentives are one of, if not the, biggest obstacle to modesty in science, so if you do have more ideas about how to deal with that than you currently present, I don't want to stop you from including them!

Response: I agree with you on the definitional issue. Regarding reviewer 2’s point number 2, I have provided more evidence that demonstrates the fallibility of human reasoning processes (Hintzman, 1991; Kahneman, 2011). In addition, by referring to Scott Lilienfeld’s work, I have also outlined the possible ways that we could incorporate and acknowledge such an appreciation of human reasoning into graduate training, as well as scientific frameworks more generally, in the hope of minimising the influence that such biases and limitations in human reasoning can have on scientific output (pages 14-15). See also my response to Reviewer 2’s point #2.

Regarding incentives, I agree with you and Reviewer 1 that they are, of course, very important and nearly always misaligned with the production of a cumulative science (Ritchie, 2020). So much so that I fear one may just stop trying to encourage reform, given that they seem so powerful and stubborn. However, my take would be that the scale of reform needed requires reform at every conceivable level. And individuals are certainly part of it and should therefore shoulder some of the responsibility, as you outline above, and as has been outlined before (Yarkoni, 2018; Ritchie, 2020). Sadly, I don’t think I have any bright ideas on incentives that have not already been put forward. Instead, I have chosen to include a paragraph in section 5, which outlines why I think individuals should still care about modesty and shoulder responsibility, even if incentive structures remain stubborn beasts for the foreseeable future (page 22, paragraph 2).

1. Regarding Reviewer 2's points 5 and 7, I think some of the difference in views about how bleak things are in the psychological and brain sciences might come from differences in personal experience. I know that I often struggle to justify my bleak view of the state of the literature in my subfield sometimes, and while I can point to metascientific results such as failures to replicate, low rates of transparency, etc., that doesn't fully capture the reasons for my less-than-rosy take on things. I think Reviewer 2 makes an important point that, to many readers (especially those that have had different experiences than you and I likely have had), your somewhat negative characterization of what we/science is capable of might need more justification.

Response: This is a valuable point. I have tried to provide further justification for my position. See my response to Reviewer 2 points 5 and 7 for details.

1. Another perspective to consider is the view that science advances not because individual scientists strive for objectivity and are cautious in their claims, but by having scientists with competing theories/positions argue for their preferred explanation without any attempt to be dispassionate. The idea is that by pitting the best arguments, and counterarguments, each "side" can come up with against each other, the best theory/explanation will win out. I don't have a great reference for this position, unfortunately. The only paper I can think of that presents something like this argument is Mitroff's (1974) "Norms and counter-norms in a select group of Apollo moon scientists". But I think others who wrote arguing against Merton's norm of "disinterestedness" also made similar points. I'm not saying I agree with this view (I don't), but I think it might be worth considering how you'd respond to this type of argument.

Response: This is an interesting perspective. Maybe the adversarial collaboration would be a good tool to use here. In general, I would still argue for a more sober and proportional link between data and inference than is common. But that does not mean that setting up purposely adversarial collaborations might not be useful, as suggested before (and as I cite on page 16). Pre-registered adversarial collaborations would seem especially valuable, as they may allow more freedom to adopt more extreme positions that usual, under the clear remit that the exercise required a combative approach. With this said, the outcome would hopefully still be a judgment that required a proportional link between data and inference, so I’m not entirely sure if that ultimately changes my overall argument. That is, unless the adversarial collaboration involves an unusually large increase in scale that spans many different methods, approaches, samples etc., inferences should be fairly modest. I have included a section on this perspective in the revised manuscript (page 20, paragraph 4).

1. I had a few thoughts go through my head as I was reading your paper that I don't think are worth presenting to you in my role as editor of the piece, because it's an opinion piece and should reflect your opinion and not mine, but may be useful to you as "thoughts from a reader who is pretty far on the reform side of the continuum". Feel free to take or leave these reflections - I'm only presenting them here in case they might be useful to you, but you do not need to respond to them in your revision or cover letter.

**Response:** Thanks. I have chosen to respond here, as I think they are all interesting points.

-On page 7, you describe some activities of the reformers as "sending the message, whether intended or not, that if only we use p-values appropriately or correct for bias in meta-analysis (for example), all our problems will disappear." I agree that a lot of reform suggestions come across - or at least are received - that way. I have struggled, however, to figure out how to avoid readers intrepreting reform discussions this way. Do you think this is inevitable, i.e., that if someone writes a paper proposing X reform, even if they make it clear that they don't think it'll cure many problems, but just alleviate one specific, narrow problem, it will be interpreted by at least some as a claim that X reform will solve all problems? Sometimes it seems to me that the way reform papers are read is quite divorced from how they are written, and I would love to know what advice you have for how authors could do more to prevent this kind of interpretation.

**Response:** This is a good point. I, of course, am likely to suffer the same fate. For example, this paper could be wrongly characterised as “if we can all just be a little more modest, all will be well”, even though I explicitly say that this is not the case. I don’t have any fully developed answers to this, unfortunately. I have one observation and one tentative suggestion.

The observation is that, even though Reviewer 2 seems to disagree, I just don’t think human cognition is completely fit for the task we have given it. Indeed, human cognition cannot operate in a bias free manner across the required complexity information in order to make more “balanced” decisions. That is, decisions that take into account more than a few factors. I now cite Hintzman (1991) for a review of the evidence for limitations in human reasoning in general. I also cite Kahneman (2011) for human biases and use of heuristics. We take mental shortcuts and use heuristics and the field of interest is broad and diverse, and issues are complicated. And these facts are just not happy bedfellows for ambitious individuals with demanding schedules, so it is not a trivial problem to solve in my view. I now make this point on page 13 (paragraph 2).

The tentative suggestion that I have is to avoid verbal arguments or at least supplement them with figures, models, diagrams and other ways of presenting information. The reasoning is the same as that supporting formal modelling approaches – it makes the account explicit and aids transparency (e.g., Guest & Martin, 2021; Hintzman 1991; Reichle, 2020; Smaldino, 2017). So, instead of just writing a paper about p values, you build a model of the scientific process and then situate p values within it. Then it becomes visually and/or computationally clear that the model of science being followed involves 10, 20, 50+ interacting parts and p values are but one part of the statistics sub-component. It could even be a box and arrow diagram of science, just to reinforce the position that many factors are involved. Again, these ideas are not fully developed. And I know some computational researchers are modelling how science can make progress or not (e.g., Smaldino & McElreath, 2016, Royal Society). But I think it would help avoid work being superficially characterised in a narrow way.

I have modified the text on page 17 (paragraph 2) to try to make the point clearer about more formal and explicit ways to situate reform tools within the bigger picture.

-On page 13 you write "I cannot see how any one individual, subfield, or organisation within psychology or brain science gets to decide what the "right" lines of investigation are to study the human brain." I'm not sure I understand this point (and similar points made in one or two other places. Surely, there are individuals, subfields, and organisations that have to make decisions (e.g., about whom to hire, what to fund, where to place resources, etc.), that depend in part on their assessment of what the "right" approach or line of investigation is. Moreover, in our roles as reviewers and critics of each other's work, we often make the case that another approach is better than the one used in the paper we are critiquing. Should such decisions or critiques not be allowed? If they are allowed, what are the boundary conditions on when we can make judgment calls about right or wrong (or at least better or worse) lines of investigation?

**Response:** These are good questions. I think I could have been clearer. I have substantially re-written section 2.2 to clarify my arguments (pages 10-13). Some arguments I have simply removed, as I think they were adding more confusion than clarity and they did not form a central part of the paper. Other arguments have been clarified.

In short, I agree that for some questions, methods or approaches, there may well emerge a fairly universal understanding and acceptance of best practice. At which point, these guidelines should be communicated broadly. I am not arguing against this suggestion in any way. Instead, however, I think there must always be aspects of what are the “right” questions and approaches to study, which cannot be universally agreed upon. We should expect this. We should also expect different researchers to make different trade-offs. And these trade-offs should be explicitly justified. I think this makes for a much stronger science because it tries to provide an environment that encourages a balance between risk-taking and rigour. I now make this point clearer in the manuscript (page 12, paragraph 3 and page 13, paragraph 1).

Minor points:  
-p. 3 "For an alternative example" - this seems like a similar, rather than alternative, example.

**Response:** Yes, I agree. I have changed this to “For a related example”.

-p. 10 - is the main claim made in the full paragraph on this page controversial? It seems most people would agree that "any signle piece of basic research may provide valuable advances to knowledge without leading to any such practical value." (This is also related to Reviewer 2's point 3 that in some places your advice was quite general, in a way that may make it seem uncontroversial or consistent with received wisdom in the field. If you think it goes against current norms, it would be helpful, here and elsewhere, for you to push the point a bit further to illustrate where you think it goes against the grain.)

**Response:** No, it was not meant to be controversial. However, as part of the revisions to this section, I have now removed this argument, as I felt it was unnecessary.

-p. 14 ""we that we could learn" should be "that we could learn"

**Response:** This has been corrected.

-p. 14 "I remain a novice is" should be "I remain a novice in"

**Response:** This has been corrected.

-p. 16 "innovative work became a lot harder to do in the last 5-10 years" I would argue that it didn't become harder to do, we just became more aware of how hard it is to do.

**Response:** Good point. This has been modified accordingly (page 19, paragraph 3).

In summary, I think this is a promising manuscript and I hope you will revise it for further consideration at Collabra: Psychology. I look forward to receiving your revision.

Please ensure that your revised files adhere to our author guidelines, and that the files are fully copyedited/proofed prior to upload. Please also ensure that all copyright permissions have been obtained. This is the last opportunity for major editing, therefore please fully check your file prior to re-submission.

If you have any questions or difficulties during this process, please contact the editorial office at [editorialoffice@collabra.org](mailto:editorialoffice@collabra.org).

We hope you can submit your revision within the next six weeks. If you cannot make this deadline, please let us know as early as possible.

Sincerely,

Simine Vazire

Reviewer 1

This paper outlines a compelling argument in favor of greater modesty and humility by researchers in psychological and brain sciences. I am in full support of the overall goal of the paper, but I do have a number of suggestions that I hope will help tighten the paper.

* It seems to me that the elephant in the room is incentives, which are discussed but only briefly. Currently, the need for hype suffuses the entire ecosystem. Grant officers are incentivized to hype the work that they fund so that the politicians who support them can crow about it. Journals are incentivized to hype the work they publish so that people will cite it and pump up their impact factor. Universities are incentivized to hype the work by their faculty so that donors will give more money. I think that a sober discussion of the difficulties of change in light of these overwhelming incentives would be useful.

**Response:** I agree that incentives are very important. However, my take would be that the scale of reform needed requires reform at every conceivable level. And individuals are certainly part of it and should therefore shoulder some of the responsibility. I now make this point clearer in section 5 with supporting evidence (page 22, paragraph 2).

* It was not quite clear to me how 2.2 fits with the rest of the paper. In part, I found it somewhat ineffective due to the vagueness about what “value judgments” might mean. I think that if you wish to retain this section, it would be useful to provide some more specific details about the kinds of value judgments you are referring to and exactly how they would play out.

**Response:** I agree that this section could have been clearer. I have now substantially reworked section 2.2 and provide several concrete examples (pages 10-13).

* I found the brief discussion of philosophy of science in section 2.3 to not add much; the comparison to physics is somewhat facile, given the large amount of philosophical work on the nature of theory in psychological and neuroscience. I’d probably just drop this.

**Response:** I disagree. I think that it is worthwhile to emphasise that recognition of the brain’s inherent complexity should demand intellectually humility. As such, I choose to keep the section in the manuscript, although I have modified the text to try to clarify the section.

* I think that there are very good examples from neuroscience of projects that have enabled the large-scale aggregation of knowledge, such as neurosynth, which might bear mention in Section 3.3

**Response:** Thank you - this is a good suggestion. I have added further examples, as requested (page 17, paragraph 2).

* I don’t understand this claim: “Cognitive neuroscience researchers who complete training studies, for example, which require vast hours of training per participant plus repeat and costly neuroscience techniques (e.g., Apšvalka, Cross, & Ramsey, 2018; Cross, Hamilton, & Grafton, 2006), may still feel that this work provides sufficient value because it brings insight into neural plasticity, which few other approaches can provide. Such value again, however, may come at the cost of reduced certainty.” Why would this particular kind of study (if performed in a rigorous manner) necessarily provide reduced certainty? And reduced in comparison to what?

**Response:** I acknowledge that this point could have been made in a clearer fashion. I now elaborate on this point and provide more detail regarding this example (pages 10-13).

* At several points in the paper, statements are made to suggest that it may be impossible to understand the mind and brain: e.g. p. 19, “it may be fundamentally impossible for humans to understand the mind and brain, at the level at which we wish to understand it”. Ironically, these statements seem quite hyperbolic and lacks any real evidence. I’d suggest tempering or removing them altogether.

**Response:** I disagree that such statements lack real evidence. At various points, I cite work from philosophy through to cognitive neuroscience and the history of neuroscience to make the claim that the brain is tremendously complex and may be impossible to understand in the ways we wish to understand it (Bassett & Gazzaniga, 2011; Cobb, 2020; Gazzaniga, 2010; McGinn, 1989; Yarkoni, 2020).

I do agree that such statements should be tempered, however. As such, I have softened the statement that you cite above, in order to emphasise that this reflects present day understanding. I hope that tempering the claim in this way avoids any implication that such understanding might be impossible in principle (and at any time in the future), which is not the argument that I am making. I would also add that such a view does not imply that we have acquired zero understanding of the mind or brain to date. The point that I want to make is that our understanding is far from the level that we desire, and we should build structures that keep reminding us of this because I think it will build a stronger scientific base for building cumulative knowledge. In my view, a more cumulative knowledge will emerge because there will be a larger collective of scientists who appreciate that we cannot move on yet to the next question because we have not fully understood the previous parts yet in a sufficiently comprehensive manner.

Signed, Russ Poldrack

Reviewer 2

This is an interesting article on an important, but neglected, topic. Its main aim is that “in addition to methodological reform, greater modesty is required across all levels – from individual researchers to the systems that govern science … to redirect expectations regarding what psychological and brain science can effectively deliver.” The article draws from a wide array of relevant literatures and makes a number of plausible recommendations in this regard. It is very well written and clearly structured.

Although the article has merit, I do not believe it is entirely successful. My major reservation is that the author does not articulate, in an informative way, what modesty is. The result is that the idea of modesty is does not properly animate the author’s work. I note that the author refrains from giving a detailed treatment of a number of the topics deals he deals with, and the recommendations he makes. Perhaps, this is because of the broad-brush nature of the article, but I was left feeling somewhat dissatisfied. It is commendable that the author is informed about a wide array of relevant literatures, but the content of many of the references provided is not employed. I think that this is a general problem with present-day scholarship.

I recommend revision and resubmission of the present article, in light of the feedback contained in my review.

I should acknowledge that in July/August last year, I reviewed an ancestral version of this paper for another journal. It was rejected. Although he author received extensive feedback from three reviewers, the present submission is essentially the same manuscript, with just over two pages of additional text, and three quarters of a page of references, added. Because the present manuscript is not substantially revised, my suggestions for change are largely those that I made about the first version of the manuscript. I have not considers the author’s treatment of the feedback from the other two reviewers. I should also say that I now know the identity of the author.

The following comments indicate a number of ways in which I think the manuscript can, and should, be improved:

1. My major reservation stems from the belief that the key idea of modesty needs to be properly developed. It seems to function as a primitive concept in the present article, and cannot carry the cognitive load that it might reasonably be expected to bear. The author cites the philosophical work of Roberts and Wood (2003), and Whitcomb at al., (2015) on humility, but does not make any use of those works, apart from stating that intellectual humility in science is a strong stance to adopt – an interesting point that I think deserves to be expanded on. Also, the use of the terms ‘intellectual humility’ in the third major section heading (‘How do we embrace intellectual humility?’, p. 11), raises the relevant question, how do the notions of humility and modesty relate to each other? The author references Burton (2018), and Bommarito (2018) on this matter, but relegates his treatment of the topic to a 5-line footnote. I urge the author to develop a decent, and workable, concept of modesty (humility?). More than this, questions such as whether modesty is intellectual virtue, and how it connects with related virtues might be addressed, as might the question of whether modesty is a disposition to think and behave in characteristic ways.

**Response:** I acknowledge that any paper needs to define key terms in a clear and accessible manner and at a level of detail that fits the purpose of the paper. I feel that I have done this already. In my view, the depth that you are requesting amounts to a separate paper on the psychological and philosophical aspects of modesty and humility, which would include a detailed analysis of the possible similarities and differences between the terms and much else besides. Including this level of detail would derail the paper from its primary purpose, which is not focussed on fine-grained definitional issues. Therefore, I leave the definitions as they are and instead choose to spend more text elaborating on the why and how of taking a more intellectually modest approach.

1. I also think that the concept of modesty needs to be developed in a way that engages with the sorts of scientific reasoning on finds in scientific practice. These might include, but not be limited to, judging the strength of inferences made (e.g., those contained in plausibility judgments as distinct from truth claims); exercising the principle of charity regarding an opponent’s viewpoint; engaging in comparative theory evaluation; employing strong statistical hypothesis testing (e.g., as recommended by the error-statistical perspective); and so on.

**Response:** This is a good point. It has broad and wide-ranging implications. To address it, I refer to the work of Lilienfeld and colleagues who have outlined how graduate training in clinical psychology programmes might be broadly motivated and structured around epistemic humility (Lilienfeld et al., 2017), as well as checklists for spotting hype in science (Michenbaum & Lilienfeld, 2018). I think these examples serve to demonstrate the pervasive nature of intellectual humility and how it can have a wide-ranging impact across many aspects of the scientific process. I do not think the paper has sufficient space to go into vast detail, however. Instead, I make it clear that there are many implications of adopting a more modest approach and I am only highlighting some of them (page 15, paragraph 1).

1. On page 8, the author sensibly asks researchers to adopt more realistic expectations of their work by showing how it fits into a bigger picture of understanding. He does this by referencing a number of very recent papers on theory construction that he thinks provide helpful blueprints. Unfortunately, he does not make use of any of the substantive content of these papers. I think that this is a major omission, because the reader is left with nothing more than the general claim that we should improve our theory building. Providing an informative example of at least one blueprint, and showing how that blueprint serves as a perspective for more specific work, would strengthen the paper.

**Response:** This is a good point, thank you. I know provide more detail in this section, as requested (page 8, paragraph 3 and page 9, paragraph 1).

1. On page 9, the author states that “it is not enough to focus only on the kind and quality of data”, and maintains that aims and context need to be considered as well. I find it puzzling that the author is intent on contrasting data with values judgments in this section of the article (judging data to be good involves methodological value judgments). I agree that broader factors such as aims and context are important (I would also add methods, theories and institutions here), but I think it would be more appropriate to contrast data with their natural methodological contrasts of empirical phenomena and explanatory theory.

**Response:** Although empirical phenomena and explanatory theory are important, they are not the focus of the current paper. Hence, I will refrain from adding more text on this topic to an already lengthy manuscript. It is worth mentioning, however, that based on feedback from the editor and Reviewer 1, I have substantially re-written the section on value judgments, which hopefully makes the central arguments in this section clearer.

1. In arguing for “a more sober view of our capacities and achievements” (p. 2), the author speaks of “the feeble nature of our abilities” (p. 5). I think that more needs to be said on this score to be convincing. For example, how does this claim sit with the fact that science continues to make genuine progress, even in the face of irreproducibility? The author cites Shiffrin et al. (2018) in this regard earlier in his article, but offers little comment on it. Relatedly, how is it that these feeble abilities combine to offer us a way forward with the collaborative team science that the author recommends? Further, is it really the case that the extensive research on the nature of human abilities paints a dismal picture of human intelligence?

**Response:** These are good comments and questions. First, I now provide further evidence that reviews evidence for limitations in human reasoning as well as the tendency for systematic biases in human judgments (see Hintzman, 1991; Kahneman, 2011; page 5, paragraph 2). The studies covered in these works demonstrate the general fallibility of human reasoning. Second, limited reasoning capabilities does not mean that no progress in science has been made. I have not made that suggestion and therefore my argument sits fine with Shiffrin and colleagues (2018) paper. Indeed, rather than suggest that no progress has been made, I am simply arguing that the foundation for building a cumulative science could be vastly improved and as a consequence we could make considerably more progress. Third, team science could be effective by pooling resources across individuals with distinct skill sets. I am not suggesting that working in teams will completely offset the inherent limitations in human reasoning. Instead, I am suggesting that diverse and complementary skill sets should be brought together much more often. Fourth, I am not suggesting that human intelligence is dismal in an unqualified way. I am suggesting that given the complexity of the problem that we are trying to understand in psychological and brain sciences and the complexity of modern-day science, human intelligence is a limited resource that cannot possibly encompass the space required in the manner required. Given this reality, we have to develop specialisations as individuals and then collaborate to pool resources in my view. I feel that my position on some of these points is already clear in the manuscript. For other points, I have tried to make them clearer in various places throughout the manuscript (see Footnote 1, as well as page 5 and page 13, paragraph 2).

1. The author indicates that he will address the matter of whether claims made by the methodological reformers are suitably modest. Although he is “greatly encouraged by the reform agenda”, I do not think that he gives this matter sufficient critical attention. The following are examples of what I have in mind:

* Are the claimed low levels of reproducibility as low as commonly thought, given that research often employ forms of reproducibility that are not named as such? Even if levels of reproducibility are (unacceptably) low, might not the use of other validation strategies, such as methodological triangulation, paint a more encouraging picture of success?

**Response:** I have two responses to this comment. First, levels of reproducibility are so low in some fields (approximately 25% in leading social psychology journals; Open Science Collaboration, 2015) that, in my view, something substantial needs to change no matter what else is going on in terms of other measures, such as triangulation.

Second, in terms of the potential for methodological triangulation to paint a brighter picture, I would say that it depends. In much of the work that I read, I rarely see evidence of convincing triangulation. But, yes, I take your point, I would be encouraged by multi-method approaches to the same question. It becomes a little more delicate when one considers finite resources. Is it better, for example, to do one large study that has considerable power/precision or split those resources across three considerably smaller studies that use different methods and that suffer from a lack of power? I think these are the kinds of questions that labs should be discussing. Maybe both are ok in certain circumstances. Maybe some labs would prefer to go one route over the other. Or maybe three labs should collaborate and each run one of the three studies.

One concern that I would have with only running small or suggestive studies on a range of different methods is the potential for “zombie” research programmes to emerge that have the sniff of a research programme because they span multiple methods. Such programmes may ultimately lack overall value and be wasteful because they produce only sparse datasets and suggestive evidence that it is not cumulative and each paper itself may be irreproducible. As I say elsewhere in the revised paper, however, exploratory research is valuable, but conclusions can only remain suggestive without follow-up confirmatory work of higher scale and rigour. In short, I think that if the field was dominated by triangulation, even if those studies were underpowered, it would be better than where we are now, which is dominated by the Cult of the Isolated Study (Nelder, 1986; Tong, 2019) i.e., large claims made on small evidence bases.

Although these are interesting points, I think that getting into any detail on them would detract from the main focus of the paper, which is not trying to debate the foundational aspects of the need for reform.

* Regarding the ongoing debates about p values (and tests of statistical significance), is the position of the so-called “new statisticians” referred to immodest, given its strong insistence on prioritizing effect size estimation over of p-value reporting? I note that the primary advocates of the new (frequentist) statistical movement have not publicly responded to the major criticisms that have been levelled against it (e.g., the Bayesian new statistics).

**Response:** No, I would not necessarily call it immodest. I would describe it as having a narrow focus. Instead, I would advocate for the position taken by Richard McElreath (2020), as I outline on page 7 (paragraph 2), which is that no matter how complex a statistical model (and no matter whether it is frequentist or Bayesian), it must be considered within a vast array of other factors such as importance of theory, the availability of open data and materials, pre-registration, meta-analytical approaches, computational modelling, experimental design, data science and visualisation and much more besides. I leave the text as it is, as I feel my stance on this point is already clear.

* Is it not the case that some of the mainstream understanding of questionable research practices needs to be questioned, or at least finessed? A good example of this is Rubin’s (2017; 2019) fine-grained, and informative, analysis of HARKing, which convincingly argues that there are different forms of HARKing, some of which have a legitimate place in good science.

**Response:** Yes, I agree. I now acknowledge this point in the manuscript (page 6, paragraph 2).

* The author refers to the contrast between exploratory and confirmatory research. Is our standard understanding of this contrast adequate, and does it justify claims, such as confirmatory research without preregistration is confirmationally worthless (e.g., Wagenmakers et al., 2012)?

**Response:** I would certainly argue that the standard understanding and use of the terms reflects an overly neat division between exploratory and confirmatory approaches. I would further add that most research has elements of both to some degree and psychology and cognitive neuroscience has a recent history of downplaying the importance of exploratory research or re-labelling it as confirmatory after the fact. There are also some researchers who argue that we should avoid the distinction completely (Szollosi & Donkin, 2021). I now mention these points in the manuscript (page 19). I avoid comment on Wagenmakers et al., 2012, as I don’t feel that it is the main focus of the paper. However, I do feel that more general comments about the division between exploratory and confirmatory research are very important, hence my changes to the text.

* What might “a more systematic approach to the theory and method cycle” be? The author is not forthcoming on this. Despite referencing an appropriate literature on the topic, the author draws little from its content. I think that this a general problem with modern scholarship.

**Response:** I accept this criticism. As is common during scientific review, previous commentators on this paper suggested that the paper be considerably shorter but include more content. So, something had to give. Hence, all aspects of the paper could not possibly have equal levels of detail. However, I have now provided more detail on the above, as I feel that it is important (page 8, paragraph 3 and page 9, paragraph 1).

1. The author’s claim that “in many ways, aspects of modern psychology and brain science resemble a creative writing class more than a systematic science of brain or mind”, strikes me as exaggerated, and unfair. I do not think the claim follows from the research ills listed immediately before it is made. At the very least, I think that it is incumbent on the author to provide a decent justificatory link here.

**Response:** First, I think you are quoting an older version of the manuscript. The full sentence in the updated version is:

“Indeed, although there have arguably been recent successes (Shiffrin, Börner, & Stigler, 2018), many aspects of modern psychology and brain science resemble a creative writing class as much as a systematic science of brain or mind.”

Second, I think it is important to note that I am referring to “many aspects”, rather than “all aspects” of psychology and brain science. Third, as you suggest, I now provide more justification for this position in the footnote 1 on page 3, which foreshadows the more detailed justification for this viewpoint that comes in later aspects of the paper.

Minor Matters

1. Page 10, fn 2: Given the heavy emphasis on effect sizes these days, I don’t think that its basic characterization is needed.

**Response:** This section of the paper including the footnote has now been removed. For what it is worth, however, if this section had remained in the manuscript, I would have preferred to keep this footnote for clarification purposes. In my experience talking to graduate students and faculty members, it is common to exclusively associate the term effect size with a standardised effect size. Hence, although it may seem obvious, it would have been worth repeating.

1. Middle of page 8: Correct “As one example approach”

**Response:** This has been changed.

1. A number of the articles on theory construction that are cited are now in press with Perspectives on Psychological Science.

**Response:** Thank you – these have been updated.

1. The Fried article on theory building is now published in Psychological Inquiry.

**Response:** Thank you – this has been updated.

1. The Yarkoni reference is now in press with Behavioral and Brain Sciences (2021).

**Response:** Thank you – this has been updated, although I can only find a 2020 reference.

Brian Haig Professor Emeritus School of Psychology, Speech & Hearing University of Canterbury University Drive, Ilam Christchurch 8041 New Zealand

24/02/2021

**Editor Final Decision—Accept**

May 12, 2021

Dear Richard,

I have now had a chance to read over your manuscript “A call for greater modesty in psychology and cognitive neuroscience”, along with the letter describing the changes you made. Thank you for your responsiveness to the concerns that the reviewers and I raised. I am happy to say that your paper is now officially accepted for publication in Collabra: Psychology. Congratulations on this work, I think it will make an important contribution to the literature and I look forward to seeing it published! I hope your experiences with Collabra: Psychology have been positive and that you will continue to consider it as an outlet for your work.

Just one very small point: during the copy-editing stage, I would suggest changing “p value” to “p-value” (with the “p” italicized) throughout your manuscript, to be consistent with convention.

As there are no further reviewer revisions to make, you do not have to complete any tasks at this point. Our managing editor will contact you in case there are any pre-prodution file related questions. You will have an opportunity to check the page proofs before we publish your article. Thank you again for publishing in Collabra: Psychology.

Sincerely, Simine Vazire Editor in Chief Collabra: Psychology