Gmail **Ian Hussey <ianhusseysipod@gmail.com>**

meta analysis software

15 messages

ian hussey <ian.hussey@ugent.be> 1 April 2019 at 20:57
To: Nigel Vahey <NigelVahey@gmail.com>

Hi Nigel,

Quick question - what software did you use for your meta analysis?

Best
Ian

--
Ian Hussey
Postdoctoral research fellow
Ghent University
Department of Experimental-Clinical and Health Psychology
Henri Dunantlaan 2
Gent 9000
Belgium

www.researchgate.net/profile/Ian_Hussey
ianhussey.github.io

Nigel Vahey <nigelvahey@gmail.com> 4 April 2019 at 12:17
To: Ian Hussey <Ian.Hussey@ugent.be>

Hi Ian,

I used the software explained in the following paper:

Field, A. P., & Gillett, R. (2010). How to do a meta-analysis. *British Journal of Mathematical and Statistical Psychology*, *63*(3), 665-694.

Can you provide me with any details of the problems that you've found with the meta-analysis and/or details of the Bayesian analyses that you've otehrwise performed for the WorldCon symposium?

All the best,

Nigel

Nigel Vahey, BA Hons (Psych) MBPsS PhD

Postdoctoral Research Fellow
Room GF-14 | Trinity College Institute of Neuroscience | Trinity College Dublin | Dublin 2 | Ireland.

Research Profile
http://www.whelanlabtcd.org/meet-the-team/nigelvahey/

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ian hussey <ian.hussey@ugent.be> 4 April 2019 at 12:37
To: Nigel Vahey <nigelvahey@gmail.com>

Cheers!

We're aiming to share all the materials on OSF about a month before ACBS so that everyone has time to look at them.

Best
Ian
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Nigel Vahey <nigelvahey@gmail.com> 4 April 2019 at 12:45
To: Ian Hussey <Ian.Hussey@ugent.be>

Ok, thanks Ian.

Would you mind popping me an email when the time comes?

Nigel Vahey, BA Hons (Psych) MBPsS PhD

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ian hussey <ian.hussey@ugent.be> 4 April 2019 at 13:08
To: Nigel Vahey <nigelvahey@gmail.com>

Sure thing!
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Nigel Vahey <nigelvahey@gmail.com> 4 April 2019 at 13:10
To: Ian Hussey <Ian.Hussey@ugent.be>

Thanks Ian,

Appreciate it

Nigel Vahey, BA Hons (Psych) MBPsS PhD

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ian hussey <ian.hussey@ugent.be> 4 April 2019 at 14:00
To: Nigel Vahey <nigelvahey@gmail.com>

The paper you sent suggest implementations in both spss and R - could you tell me more about what you used specifically? Do you have scipts or syntax I could use to computationally reproduce the analyses?
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Nigel Vahey <nigelvahey@gmail.com> 4 April 2019 at 18:40
To: Ian Hussey <Ian.Hussey@ugent.be>

Hi Ian,

I used the SPSS syntax provided by Field & Gillett for the Hunter and Schmidt (2000) random effects method (cited at start of IRAP meta-analysis results section).

I also remember that in relation to the publication bias statistics where there was no SPSS option I used R scripts -- but likewise in each case all of the relevant scripts are either provided by Field and Gillett, or else by another journal article citing the relevant publication bias technique.

I generated the forest plot using Graphpad Prism based upon output from above scripts.

Hope that helps,

Nigel

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ian hussey <ian.hussey@ugent.be> 4 April 2019 at 18:41
To: Nigel Vahey <nigelvahey@gmail.com>

That's useful - if you had the actual scripts and code you used that would be even more so?
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Nigel Vahey <nigelvahey@gmail.com> 4 April 2019 at 18:48
To: Ian Hussey <Ian.Hussey@ugent.be>

They should all be available for download with the relevant paper -- honestly, it would take me quite a bit of time to dig out those scripts from computer backups given that I performed the relevant analyses two computers ago. All of the relevant scripts should be available with the supplementary methods of all the papers mentioned -- can you try and obtain them that way and sure if you run into problems let me know and I'll see what I can do? For efficiency's sake I'm keen to postpone deep diving into the meta-analysis paper until there are specific criticisms for me to address

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ian hussey <ian.hussey@ugent.be> 4 April 2019 at 18:57
To: "Jamie.Cummins@UGent.Be" <Jamie.Cummins@ugent.be>

Thoughts on the below exchange. My point here is to ask whether the analyses are computationally reproducible, and the answer seems to be no. Perhaps I can clarify that to him, that replicability applies to meta analyses too? I have a working workflow in metafor, and the burden of responsibility for reproducibility isn't on me here. At this point, if I'm asked/when I present on it I'll be saying "we contacted the original authors who didn't supply us with materials to computationally reproduce their meta analysis, beyond pointing us to the papers they used."

I guess my point here is that I don't need to reproduce them, I'm asking if it's possible. I won't be taking the effort to do so, I'm asking him if he can reproduce them and its in his interests to. Thoughts welcome.
[Quoted text hidden]

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ian hussey <ian.hussey@ugent.be> 5 April 2019 at 11:48
To: Nigel Vahey <nigelvahey@gmail.com>
Bcc: "Jamie.Cummins@UGent.Be" <Jamie.Cummins@ugent.be>

Hi Nigel,

Cards on the table here, I'm asking if the analyses reported in the paper are computationally reproducible. From an OS perspective, the question of computational reproducibility has inherent value as it's a key criterion for meta analysis replicability. I'm not necessarily asking about comp repo on the back of some criticism, but because knowing whether it's possible is a useful discussion point in and of itself to illustrate replicability pitfalls. Even if it is, the relative difficulty of reproducing an analysis (e.g., because files are buried deep in private backups) is also a useful metric of reproducibility, and is an argument in favour of open data and materials on public archives such as OSF. I hear what you're saying that the generic scripts used in the papers you reference are likely available and that it's notionally possible to construct an analysis that mimics your own with some work. However, this is non trivial from the sounds of it? At the moment, the comp repo status seems to be either "not possible" or "possible with the direct help of the original authors", but not the ideal category of "trivial using publicly available materials".

Best,
Ian
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Nigel Vahey <nigelvahey@gmail.com> 5 April 2019 at 17:14
To: Ian Hussey <Ian.Hussey@ugent.be>

Hi Ian,

It's going much too far to suggest that it is not possible to computationally reproduce the IRAP meta-analysis. From my point of view, it's even questionable whether a reader would ***need*** "the direct help of the original authors" to computationally reproduce the IRAP meta-analysis.

Granted, it may not be convenient or indeed trivial to reproduce the IRAP meta-analysis but that is hardly the same thing as saying that the author's direct help is required -- and it is certainly a relative concept. As I'm sure you'll agree, when we drafted the meta-analysis back in 2014/2015 the open science movement was still in its infancy in psychology and there were no clearly adopted standards of reproducibility at the time. It was certainly not a requirement (or even still) that the relevant analysis should be trivially reproducible -- particularly given that we are discussing a meta-analysis that incorporates a whole research literature (rather than simply one study). I am sincerely all for moving towards FAIR data standards without delay -- but it is hardly helpful to single out an analysis that predates these standards for not adhering to them. To make such arguments would be tautological and specious unless it were part of a much larger proposal to implement some sort of 'grandparenting' procedure for rehabilitating data/analyses that predate the current 'ideal' standards that you appear to be comparing the IRAP meta-analysis against.

Incidentally, the IRAP meta-analysis certainly went beyond standard reporting practices for meta-analyses at the time of its preparation/publication when it provided the reader with an extensive and explicit breakdown of the effect-sizes included in the meta-analysis. In addition, my coauthors and I have quite literally provided the reader with a step-by-step instruction manual for computational reproducibility in the form of the Field and Gillett (2010) paper -- it even provides the reader with the necessary software.

Indeed, with regard to the question you asked in your previous email about whether I used SPSS or R to produce the relevant meta-analysis -- there needn't be any confusion here either. The IRAP meta-analysis paper explicitly states that we used the Hunter and Schmidt method, and then refers the reader to Field and Gillett (2010) for an implementation of this method which is only provided in SPSS and not in R. Field and Gillett do provide R scripts for some of the supplementary (bias) analyses that we performed -- but again, in each case there wasn't both an SPSS and an R version so in principle there needn't be any input from me to proceed. In answering your earlier questions, I was simply trying to be helpful.

As far as I am aware, the open science community is currently still very much focused upon mainstreaming open science standards with regard to new research, but for now it has largely disregarded the issue of FAIR-ifying data/analyses that predate the new standards. I have attended multiple workshops on FAIR standards by leaders in the field and the question of how to fund FAIR-ifying new data is still very much a live issue -- much less the question of how to FAIR-ify data that was not collected/analysed with those standards in mind in the first place. My point here is that it is not reasonable to expect authors to bear the cost of developing the equivalent of specialist GUI's that make their retrofit analyses from half a decade ago to be interactive in a trivially user-friendly fashion (i.e. think software development and portability). In recognition of this fact, literally hundreds of millions of euro worth of research funds have already been earmarked globally towards the development of general solutions for FAIR-ifying legacy data.

Now, with all of that being said I am prepared to dig out and collate my original analyses to share them with you for the purposes of minimizing any further misunderstanding. It is just a case of finding the time to do so. Like I said to you in an earlier email I'm currently transitioning from one employment to another so it's not a convenient time for me to say the least. I will do my best to provide you with the relevant materials next week or failing that the week after. In return, could you clarify for me, even in summary, what was the empirical basis of the abstract that you submitted to ACBS WorldCon 2019 criticizing the IRAP meta-anlysis?

All the best,

Nigel

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ian hussey <ian.hussey@ugent.be> 5 April 2019 at 18:24
To: Nigel Vahey <nigelvahey@gmail.com>
Bcc: "Jamie.Cummins@UGent.Be" <Jamie.Cummins@ugent.be>

Hi Nigel,

I'm not at all asking you to do this, or saying that it should be done, or saying that the meta analysis is irregular for its time, or a few other inferences you've made. This is just me asking a question that has emerged recently in the replicability literature and discourse, specifically, is an analysis be trivially computationally reproducible. When I say apparently no, triviality it key to the question and answer. I have a meta analysis workflow in R/metafor so it's not that I'm looking for a way to do it in general, just whether your method is trivially computationally reproducible. Indeed, even if you take the time to dig out your materials (and I assume this is a bunch of work given that it must be more

than that we've poured into writing emails about it back and forth), the answer to the question is still "computationally reproducible, but highlights that material archival systems are a potential point of failure, as the authors pointed out that it was far from trivial to recreate these analyses or find their original code".

>From my point of view, it's even questionable whether a reader would need "the direct help of the original authors" to computationally reproduce the IRAP meta-analysis. Granted, it may not be convenient or indeed trivial to reproduce the IRAP meta-analysis but that is hardly the same thing as saying that the author's direct help is required.

But this isn't a notional situation, I am said reader, and I have had to contact you for insights into your implementations. I can and have conducted my own meta analysis using your public data, but this isn't the same as computationally reproducing your own analyses. From my point of view, inconvenient non trivial reproducibility is too low a bar for us to set.

As a sidebar, it's worth noting again that my goal here isn't to take a pop at your meta analysis (although, that's not to say that this makes it a priori immune from criticism somehow), it's to highlight issues in the IRAP literature as an example of broader issues in CBS's analytic practices. Some of these practices might have been exemplary for the time they were written, but progress happens and that's a good thing. My question is "what does the IRAP literature tell us given most recent stats knowledge" not "how many gotchas can I find here". Indeed, for my current question about computational reproducibility, I would have been extremely surprised if your answer had been "yes, here's the script, just press run", because we as a field aren't there yet. We can only get there by reflecting on our past and current practices, seeing how they do or could causes issues, and making suggestions for the future. Reflecting on the meta analysis and its underlying studies is a good case study for everyone's work going forward.

best,
Ian

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Nigel Vahey <nigelvahey@gmail.com> 5 April 2019 at 19:08
To: Ian Hussey <Ian.Hussey@ugent.be>

Hi Ian,

Thanks for your quick response.

I'm in complete agreement with your final paragraph. From that point of view, I really do think that it could be useful to reflect on where we've been and where we could go with future synthesis of data analyses.

I'm glad that you clarified this for me because the conversation originally began a few weeks back very much from the point of view of criticizing the IRAP meta-analysis in isolation from other contemporary meta-analyses/literatures....and in the absence of any specific empirical claims/evidence to that effect, I'm sure you understand that it's natural that I might wonder about this. By contrast, I'm enthusiastic about moving in the direction you now summarize in your final paragraph below where new standards are not being used to play gotcha with old analyses. The latter would surely involve getting bogged down in trivialities that would take away from our ultimate goal of moving FAIR standards forward.

In hindsight, as you suggest we're probably getting to the stage now where it would have been quicker to dig out the relevant materials for you but it's probably a good thing that we clarified these things for each other -- so not a wasted effort. I've gotta run now but I will make my best effort to package my original datafiles and pipeline for you next week. I hope that this timeline works for you.

All the best for now,
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