Dear Matt,

Thank you and the reviewers for your very thoughtful reviews and discussions, and the effort you put in to liaise with ethics committees to get rounded opinions on these questions.

I have addressed all of your and the reviewers comments in the manuscript and supplementary materials. I respond to each point in line, where my responses are in italics and prepended with “*Author response:”.*

**Editor comments - Matt Williams**

Dear Ian,

This letter concerns your submission to Meta-Psychology, "Data is not available upon request". Thanks for your patience during the review process. I have now received three thorough, conscientious and helpful reviews (from Lisa Spitzer, Tobias Denlin and Gjalt-Jorn Peters).

All three reviewers see some merit in your study, and have also raised some critiques. I will let you read their points yourself, but I think the issue of ethical approval (which we've briefly discussed already) deserves some more discussion here. Gjalt-Jorn and Lisa seem relatively comfortable with your argument that the study does not constitute research with human subjects and thus did not require ethics approval. On the other hand, Tobias does not find the argument convincing and believes the study should have been subject to an approval process.  
  
I do think this is an issue about which reasonable people might disagree, and about which there would be significant variation across contexts (i.e., some universities would probably require that researchers apply for ethical approval for a study like this, while others would not). From my perspective as an AE, my primary concern is whether you adequately followed regulations and norms in your own context (i.e., at the university and country where you did this research). To that end I consulted with an editorial board member in Germany, who in turned chatted to a couple of ethics committee members in Germany to get their take on the situation. In each case though these experts felt like the situation was a bit too ambiguous to give a definite answer.  
  
Despite the fact there is some ambiguity about the situation, I am inclined to accept your rationale, and the consultation you based it on.

That said, given the issues noted above, and the fact you didn't seek consent from authors, I think it's especially important to be careful about de-identifying authors. I know you've already put significant thought into how to balance de-identification with reproducibility, but I'd just ask you to have another check over all the files you've placed on OSF and Github for this project and think about whether there's still space to further protect identifiability. E.g., did you definitely intend to share the file [data existence and availability.xlsx](https://osf.io/t9rfp), which seems to name authors you corresponded with and the outcome? How crucial is the file [authors.xlsx](https://osf.io/af658)? In the [blinded and shuffled data](https://osf.io/evp8m), might there be a way to offset Tobias's concern that knowledge of the journal and year might identify some authors? I haven't exhaustively reviewed the files myself, but it might be useful for you to have another look, now with some assurance that I agree we can sacrifice some small degree of reproducibility in the service of protecting identities. I'm happy to chat about any specific dilemmas that present themselves.

*Author response:*

*In the service of getting these results out there so that this systematic problem can be seen by a wider audience, even if it means making it impossible for readers to know which datasets that are stated to be available are not (therefore undermining the public service benefit of disseminating this information about data availability), I have followed your and the reviewers’ advice and fully anonymized all data associated with this project. I have exhaustively reviewed all files for any possibly identifying information and removed it. Specifically:*

* *“data existence and availability.xlsx” has been deleted from its github repo and scrubbed from the git commit history too. It has been replaced with a file called “datasets that are available but can't be posted publicly.xlsx”, which only lists the datasets that authors reported are available but can’t be posted publicly, and the contact details for those datasets. It does not mention any correspondence, data sharing statements in articles, or any mention of discussion authors or articles who did not share data. I.e., the authors mentioned in this dataset have already shared the data with me, but either stated that it could only be shared onwards privately (but not posted publicly) or stated that 3rd parties had to contact them directly for a copy.*
* *“authors.xlsx” has been deleted from its github repo and scrubbed from the git commit history.*
* *In “blinded and shuffled data.xlsx”, the journal column has been blinded. Multiple articles share each year, meaning that the year column by itself does not pose a deanonymization risk. The journal column has been moved to a separate file named “results of data sharing requests - journals only reshuffled.xlsx” where journal name is the only non-blinded column. The rows were then reshuffled. This file is used merely to provide a count of the articles per journal, and cannot be relinked to any other data to identify authors or what communication was had with them. All previous versions of these files were scrubbed from the git commit history.*

Outside of the issue of ethics, I think most of the reviewers' comments are ones where it's reasonably clear how the relevant issues can be addressed. As for any R&R I'd ask that for each point you clearly signal what changes you end up making, or a clear rebuttal for any suggested changes you believe to be unwarranted. I will then consider your revised manuscript.  
  
I have attached the reviews to this letter and cc:ed reviewers. My apologies that some of the formatting in the reviews may have been removed by our editorial system.  
  
Best wishes,  
Matt Williams

**Reviewer 1** **- Gjalt-Jorn Peters**  
Dear Matt (and Ian),

Thank you for the invitation to review this manuscript, and my apologies for the delay!   I enjoyed reading this article - and this isn't a pleasantry, but a reflection on the writing style. It struck me as more accessible than quite a few articles, in line with the implicit imperative by Mastroianni & Ludwin-Peery (<https://doi.org/j2rn>). I believe that making our work more accessible to a broader audience contributes to the aims of open science, and this was great to read in that respect, too.   I was especially glad to read the reflection on ethical approval. I feel we have too few discussions around when ethical approval is functional and when it is problematic, and therefore applaud contributions to such discussions.   Substantively, I think this study contributes to a growing body of pretty bleak literature illustrating fundamental problems in how we do research in psychology and how we structure the systems in which we do that research. I think this is a great example of meta-science, that hopefully can encourage others to engage in replications and so not only enable identification of patterns in data sharing (or failures to do so), but also contribute to a culture shift where data are shared openly upon (or preferably before) publication.   I have identified no major issues, but some minor and micro issues which are listed below.   Kind regards,   Gjalt-Jorn Peters

## Minor issues:

- On page 4, I find the sentences "If I did not receive a response offering to share the data, I then contacted the apparent senior author. If I again did not receive a response, I contacted other authors starting with the senior author." a bit confusing. They seem to suggest that the first step in the phase described in the second sentence consisted of repeating the step described in the last clause in the first sentence (i.e. contact the senior author)? However, that requires a bit more inference (which is always a bit shaky) than seems 'safe'. I think that this paragraph could maybe be clarified a bit more (a flowchart could even help, with explicit decision points and acts). It is kind of overkill, since the bottom line is "all authors were contacted in a given order", but to enable precise replication, I think it would be worthwhile.

*Author response: You are right that this was unclear, especially due to one incorrect word (‘first’ vs. ‘last’). I have clarified this paragraph, which I copy below. Note that this aspect of the procedure was more pragmatic than systematic, given it’s a small field and I know most of these authors personally.*

*“In the first instance, I attempted to contact the corresponding author using the email listed in the published article. If I did not receive a response offering to share the data, I then contacted the first author, if they were also listed as the corresponding author. If I again did not receive a response, I contacted other authors in order of apparent seniority (e.g., contacting professors before PhD students). Note that the IRAP literature does not, at the time of writing, conform to the norm present in some fields that the final author is considered to be the “senior” author, so seniority did not easily map onto authorship order. In many cases, a given author instructed me to speak to other co-authors to obtain the data. This was therefore a highly iterative process of sending email requests.”*

- I'm not clear on the meaning of the "c" in the bit between parentheses on page 4: "(50% within 15 days, c.85% within 30 days, c.97% within 60 days)"? Is that a LaTeX thing or something I'm just not familiar with?

*Author response: “c.” stands for circa – but I now see (a) this was unnecessary and (b) these numbers were not computationally reproduced in my analysis script from Tedersoo et al.’s* *data. I’ve updated the `analysis.Rmd` script to do this and updated the numbers in the manuscript, which now read:*

*“(50% within 15 days, 75% within 28 days, 90% within 34 days, 95% within 48 days, 97% within 59 days, and 99% within 90 days).”*

- On page 4, you mention that one study deposited data on ResearchGate. Given the nature of this study, it seems remiss to not discuss the problems here more in depth. ResearchGate is not an appropriate repository; its status is similar to that of other proprietary, "non Open Science" infrastructure in that it is not community owned (i.e., no open infra as described in the UNESCO Open Science recommendation, see <https://www.unesco.org/en/legal-affairs/recommendation-open-science>). While the COS has policies (and funds) in place to ensure persistence of OSF even if the associated legal entity would cease to exist, this does not seem to be the case for ResearchGate GmbH. In addition, ResearchGate seems mostly profit-driven (see e.g. [https://en.wikipedia.org/wiki/ResearchGate#Criticism](https://en.wikipedia.org/wiki/ResearchGate" \l "Criticism)), which makes it a risky place to rely on as a data repository. In an article focusing on data availability, neglecting to discuss the 'stewardship responsibility' despite a problematic example coming up seems a bit... Well, negligent, for lack of a better word?

*Author response: The following was added to this paragraph:*

*“(note however that I would recommend researchers to use services offered by not-for-profit entities with longer term data preservation structures in place, such the OSF, over for-profit entities such as ResearchGate).”*

- On page 4, in the last paragraph before heading "Impediments to data sharing", you don't acknowledge that the direction of any effect is opposite to what one would expect; and the last sentence even seems to suggest that this pattern wasn't reversed? After all, even if the Chi^2 was significant, it wouldn't have suggested that the presence of a Data Availability Statement was associated with a higher rate of actual data sharing upon request... Also, but this is in this case more of a personal nuisance than a large problem, given the direction of the effect: conducting an NHST test without having powered for it is problematic I think. Without first defining a smallest effect size of interesting and deliberately designing a test with desirable type-1 and type-2 error rates, I think a p-value is practically meaningless. You can also provide the confidence intervals for the percentages (in R, you could use e.g. `ufs::confIntProp(8, 31)` and `ufs::confIntProp(3, 18)`, to find [12; 45] and [0; 41]), which doesn't suffer from these problems but makes the same point. (And actually this also shows that these data are (or is) not easily reconcilable with any population estimate higher than half the studies sharing datal, which seems neat maybe)

*Author response: Thank you for the suggestion to add 95% CIs to all percentages reported in the results – I’ve done this throughout the manuscript. E.g., on page 4 the text now reads:*

*“Aggregating results across all co-authors of each article, I received a reply to my email(s) in 82.7%, 95% CI [69.7, 91.8] of cases (43 articles). Authors reported being able and willing to share their data in 42.3%, 95% CI [28.7, 56.8] of cases (22 articles). Authors actually shared their data in 26.9%, 95% CI [15.6, 41.0] of cases (14 articles).”*

*I’ve also added a clarification of the estimates prior to the Chi-square test. The text now reads (emphasis added):*

*“Of the 31 articles without a Data Availability Statement, 8 shared data upon request (25.8%, 95% CI [11.9, 44.6]). Of the 18 articles with a promissory Data Availability Statement, 3 shared data upon request (16.7%, 95% CI [3.6, 41.4]).* ***That is, data sharing was descriptively lower in articles with a promissory data sharing statement than no statement at all.*** *A Chi-squared test suggested that the presence of a Data Availability Statement was not associated with a higher rate of actual data sharing upon request, χ(1) = 0.35, p = .55.”*

- On page 6, you state "Worryingly, data sharing was lower in articles that stated that data was available upon request (16.7%, 3 of 18 articles) than those that included no Data Availability Statement at all (25.8%, 8 of 31 articles).", which is inconsistent with the earlier displayed commitment to NHST as a mechanism to test for differences. That test was, after all, not significant, so that suggests the percentages are the same (bar sampling/measurement/etc error). You can explicitly contextualize the statement (or no longer conclude that the percentages are the same earlier on, i.e. with the chi^2 test).

*Author response: Thank you for catching this – this language came from a previous version that used only descriptive results. You’re completely right that the “lower” now gains meaning when there is a chi-squared inference test, and is incompatible with it. The text now reads (emphasis added):*

*“Worryingly, data sharing was* ***no higher*** *in articles that stated that data was available upon request (16.7%, 3 of 18 articles) than those that included no Data Availability Statement at all (25.8%, 8 of 31 articles).”*

- In the sentence "Tedersoo (2021) examined data sharing upon request and quantified whether they were acquainted with the authors prior to the data requests they sent. ", the use of "quantified" seems a bit weird? How do you quantify a basically binary variable? Of course, you can call everything quantifying (i.e. storing as a 0/1 data series), but the quantification is trivial then, so probably not what you want to draw attention to? Or do you mean something like "counted" or so - but then shouldn't it be something like "quantified in which proportion of requests" or something like that?

*Author response: Well spotted. The authors used a three point scale, which I now explain in text:*

*“Tedersoo et al. (2021) examined data sharing upon request and quantified whether they were acquainted with the authors prior to the data requests they sent (i.e., ‘no contact’, ‘have met’, ‘have collaborated with’).”*

- Similarly to the point I made re: ResearchGate, one aspect that seems relevant but is not touched upon is the lack of a "professional working culture" exhibited here. The fact that it is often not possible to contact researchers, and that not being able to produce the data that forms the foundation of claims in the literauture seems acceptable by universities seems problematic. The norm appears to be that researchers manage a project, and upon publication, the host organization does not feel responsible for what was produced in that project: as if the publication is the goal, rather than contributing to a cumulative science. Most organizations develop and enforce internal policies to ensure that as staff members leave and new people join, the produced assets remain -- and for industry, the goal is merely profit, nothing as 'big' as knowledge production. Given that this research is publicly funded, this seems problematically lax and basically boils down to destroying a portion of those public funds. Universities seem to neglect their responsibilities as stewards. This is a different point from the implications for reproducibility. Basically, receiving tax money comes with responsibilities that, based on this evidence, are shirked by many institutions.

*Author response: I agree of course. One point though: at least some of the universities involved here do have policies requiring data sharing and the researchers were in breach of those policies. This is noted in the manuscript on page 3 (“Lastly, it is also reflected in the Research Data Management policies of many universities, including those with which many researchers contacted as part of this study were affiliated.”). However, additional complications are involved in trying to get the universities to enforce their own policies, which go beyond what the manuscript can afford to tackle.*

Micro issues:

- For some reason, "Munafò" became "too" in the citation and reference.

*Author response: This was a typo in Zotero, fixed.*

- On page 2, a period is missing after "exception".

*Author response: Fixed.*

- On page 5, a word seems either redundant or omitted in sentence before heading "The fourth case is apparently unresolvable as, despite university policy to (a) retain data and (b) share it upon request, because the researcher has moved institutions the university has little power to enforce its own policies."

*Author response: This was indeed unclear. The text now reads:*

*“The fourth case is apparently unresolvable as, despite university policy to (a) retain data and (b) share it upon request, the university has little power to enforce its own policies once researchers are no longer employed by that institution.”*

---   PS: GitHub, like ResearchGate, is a problematic piece of infrastructure to use for science; you could consider switching to another git forge, such as CodeBerg (or GitLab; GitLab is corporation-owned, but have an open core business model; Codeberg is a foundation, and so community-owned). GitHub is proprietary and owned by Microsoft, so not an idea Git forge. I know even open science proponents don't like to hear this (i.e. switching to another git forge is hassle), but practising what we preach etc.

*Author response: I have uploading a zip file of all the materials on the github repo to the OSF project it is linked to. This way, even if github disappeared, the OSF copy will persist. Moving from github to an alternative is on my to do list, but its hard to detach from github as much R package development happens there.*

**Tobias Dienlin**  
Dear Ian,

in the spirit of your accessible article written in the first person, here's also a personal review. In your article, you analyze if authors of papers working with IRAP are willing to share their data. The results is that, by and large, they aren't.   I like the article and the research. The research question is relevant, the article is well-written, and it exposes a profound problem in our research community: Sharing data is neither incentivized nor enforced. Even including statements that data are available on request is not associated with higher likelihood of sharing. This is a relevant finding.   Below I outline some points of concern. I would be curious to learn about your response.

Unfortunately, I do see it critically that the study has no ethical approval. I was wholly unconvinced by the argumentation that this study would not represent human subjects research. You do not reanalyze data, but you analyze scholars who have analyzed data. "Data are people, too" -- and I would add, scholars are people, too. You write "The act of contacting researchers to ask them to share data, and reporting the rate of data availability, involves no human subjects and therefore does not require ethical approval for human subjects research." But your study is whether scholars share data. Scholars are your population, your subjects. And you try to understand their behavior. Observed from afar, I don't see how this fundamentally differs from other areas of research where we require ethical reviews and participants consent. Yes, there might be ways in which we can look at this and say that this doesn't apply here. For example, because researchers are part of a public institution and hence need to respond to official requests, and that the public has a right to find out how well this functions. However, this would require an external balancing of interests. For example, I could imagine we would dispense the need to require a priori consent for the study to work; perhaps it would suffice to inform participants post hoc. Perhaps it would be required to allow researchers to withdraw their participation in the study. I mean, some authors can easily be reidentified given the online supplementary material (if a journals has only 1 publication on the topic, well). At our department, even content analyses of publicly shared materials require ethical approval, as the recontextualization of information creates new situations potentially detrimental to original content creators. So, this is problematic to me. If I were to evaluate the study ethically, yes, I'd likely come to the conclusion that it can be conducted. However, I'm critical that no impartial external assessment took place, and I wasn't convinced by the reasons why that's not necessary here.   I think your door-in-the-face approach to ask for data to then share it publicly is also a bit problematic. There's a difference between sharing data with other researchers (which is required), and sharing it publicly (which is not required). Your strategy was in a second step then to back down and to say that sharing data publicly would not be mandatory, you could also use it only internally.

*Author response: I won’t respond directly to this point here, as it is covered by the editor’s comments and the changes made on the basis of them (see above).*

So while I understand that your general point still stands, and of course authors should at least share personally/privately, sharing data is also a matter of trust and interpersonal mechanisms. If I perceive someone to be overreaching or impudent or whatever (lacking the right word here), I might just end the conversation at this point. Personally, I was not convinced by the rather strident approach taken here. Perhaps discuss that the ways and the circumstances in which we contact researchers to share their data might (somewhat) affect their likelihood to do so.

*Author response: I have added the following section to the discussion:*

*“****The impact of the phrasing of a data sharing request on sharing.*** *It is of course possible that the rate of data sharing upon request may be moderated by the nature of that request, e.g., the phrasing used in the email. Authors may perceive data sharing as a matter of interpersonal trust, and if they perceive the request to be overreaching, impudent, or otherwise distasteful they may be more likely to refuse, ignore or disengage from the request. At a psychological level, this may be plausible. To the best of my knowledge, no work to date has experimentally manipulated the phrasing of data sharing requests in order to assess its impact on the efficacy of such requests. Future research may wish to do this. However, at the same time, we should be wary of making compliance with data sharing requests the responsibility of the requester rather than the authors, where those requests are a matter of compliance with professional standards, journal policy, institutional policy, funding body policy, and/or the authors own written prior commitment to do so. Given that some authors are simply disinclined to share their data despite requirements to do so, it may be more effective for the field to attempt to enforce these existing requirements rather than attempt to solve the issue by finding the optimal way of phrasing the request.”*

„Worryingly, data sharing was lower in articles that stated that data was available upon request (16.7%, 3 of 18 articles) than those that included no Data Availability Statement at all (25.8%, 8 of 31 articles).“ In the results section you write that the difference is statistically nonsignificant. I understand that in this sentence you're only describing the sample and are not making inferences; still, I recommend presenting it differently or at least add some qualifications that the difference is not statistically significantly different.

*Author response: Fixed in a previous reply to reviewer 1.*

Typos:

„which are essential for maintaining the integrity of the scientific process (too et al., 2017).“

*Author response: Fixed*

„However, precise estimates of the rate of data sharing as yet unknown.“

*Author response: Fixed*

„Nonetheless, there are now many resources which practical guidance to researchers on how to share data more easily“

*Author response: Fixed*

„One previous study examined the efficacy of changes in journal policy to require open at the time of publication.“

*Author response: Fixed, removed this sentence which was redundant with the next one.*

Thank you for the opportunity to learn about this interesting and relevant research.

Tobias Dienlin

**Reviewer 3 - Lisa Spitzer**

I want to congratulate the author for this interesting work, which I enjoyed reading and reviewing very much.

**Summary**: The author inspected data availability statements and data sharing in 52 articles that employed the Implicit Relational Assessment Procedure and were published within the last five years. Prevalence of data availability statements increased over time, but data was rarely actually shared upon request, contradicting the journal policies of the inspected publications and the data availability indicated in the articles. The author concludes that non-enforced data availability statements and the practice of "sharing data upon request" are inappropriate approaches for ensuring data availability. I have some comments, which concern minor points:

* “too et al., 2017” (p. 1): Citation is not displayed correctly in the text/reference list, should be “Munafò et al., 2017”

*Author response: Fixed*

* “Journal policies requiring and explicating data sharing are to be applauded, as data sharing is essential to independent verification of results, efficient secondary use of data, and knowledge synthesis (Evans, 2022).” (p. 1): I would recommend putting the citation more at the beginning of the sentence, e.g. “Journal policies requiring and explicating data sharing are to be applauded (e.g., Evans, 2022), …”, as I understand that the cited editorial is given as an example rather than providing evidence for the thesis that data sharing is essential for the various points mentioned, which could be unclear with the current sentence structure

*Author response: Fixed*

* “Examination of the literature showed that this anecdotal experience was the norm rather than the exception Over six decades ago …” (p.2 ): Punctuation point missing

*Author response: Fixed*

* Ethical approval: I agree with the outlined reasoning for not requiring ethical approval for this study. Personally, I believe that to support this argument, referencing the various ethical guidelines suffices without drawing a comparison to IPD-MA. Consequently, I recommend omitting this comparison. Additionally, I suggest removing the examples in parentheses (“e.g., a tweet, email, or letter”). While it may be true that ethical approval might not be required to quote or paraphrase these forms of communication, I would argue that you would still ask the other person whether it is okay to use their correspondence. Based on my understanding, the authors you contacted were not explicitly asked for such permissions, thus I think this comparison is not completely appropriate.

*Author response: I won’t readdress this in detail here as it is already covered by my discussions with the editor above and in correspondence elsewhere. I have retained these two points about the relevant precedent set by IPD-MA (which the reviewer feels to be redundant but not wrong, and I think has value) and that ethical approval is not needed to paraphrase correspondence (which the reviewer substantively agrees with, and therefore I retain).*

* “Authors reported being able and willing to share their data in 42.3% of cases (22 articles). Authors actually shared their data in 26.9% of cases (14 articles).” (p. 4): What happened to in these cases where authors were willing to but did not actually share their data?

*Author response: I have added the following text after the section you quote:*

*“When authors initially reported that they would share the data but did not actually do so, the nature of these interactions and the reasons given took a variety of different forms: some authors simply stopped replying to emails, some raised ethical or legal issues, and others reported that the data could only be shared via a university platform that did not yet exist. Impediments to data sharing are discussed in more detail later.”*

* “Of those two actually provided the data (both via links to the Open Science Framework or ResearchGate). […] 66.7% of articles with data sharing statements implying actual data sharing at the time of publication shared data without the need to contact the authors.” (p. 4): Since the last sentence refers to the previous sentences and comprises the same information, I recommend integrating the reported percentages into the previous descriptions, e.g., “Of those two (66.7%) actually provided the data …”. I think this would improve clarity.

*Author response: Fixed. Text now reads:*

*“Three articles’ data-sharing statements represented claims of actual data sharing at the time of publication. Of those, only two (66.7%) actually provided the data as described, in both cases via links to the Open Science Framework and ResearchGate (note however that I would recommend researchers to use services offered by not-for-profit entities with longer term data preservation structures in place, such the OSF, over for-profit entities such as ResearchGate). One article stated that “All data generated or analysed during this study are included in this article and its supplementary information files”. However, no such supplementary materials were available on the journal’s website.”*

* “One article stated that ‘All data generated or analysed during this study are included in this article and its supplementary information files’. However, no such supplementary materials were available on the journal’s website.” (p. 4): I think it should be noted here that these data were shared upon request, though.

*Author response: Fixed. Text added:*

*“In this case, an author was able to provide the data upon request, however the inaccuracy of the data sharing statement is still notable.”*

* “However, precise estimates of the rate of data sharing as yet unknown.” (p. 6): ~ “are yet unknown”?

*Author response: fixed in a previous comment.*

* Discussion: Given that the study specifically examines articles from a narrow research field, discussing the generalizability of the results is very important. In my opinion, this was done well in the manuscript.

*Author response: Thank you!*

* From what I understood, this study was not preregistered. Therefore, I recommend highlighting that it is exploratory.

*Author response: I’ve added a section prior to ‘Ethical approval’ that reads:*

*“****Preregistration***

*This study was not preregistered and its results should therefore be considered to be exploratory.”*

* Online material: Whenever only one or a few articles are accessible from a particular journal, the authors remain identifiable in the anonymized data (e.g., there is only one article from the journal “Dementia”, which can be identified through the overview of all included articles). Therefore, it might be necessary to exclude information related to the journal to ensure anonymity.

*Author response: As I mention in the comments to the editor, the journal column has now been blinded. A separate data file containing only the journal column, with all other columns blinded and the rows reshuffled, is used to quantify the number of articles taken from each journal. These two datasets cannot be relinked, and no possibility of de-anonymisation remains.*

I think that an important point is made here, which is comprehensibly justified by the data. I was able to reproduce the included analyses using the analysis script and data provided online. Overall, I feel like this work is an important contribution and I hope that my comments will help the author improve their manuscript. All the best,  
Lisa Spitzer