

The Need to Review Peer Review: The Regnerus Scandal as a Call to Action

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ABSTRACT

This article uses Mark Regnerus' methodologically flawed paper, "How different are the adult children of parents who have same-sex relationships? Findings from the New Family Structures Study," published in the journal *Social Science Research* (2012) as a catalyst to expose fundamental faults with the existing peer review system in the social sciences. The acceptance of this article has not only been damaging for same-sex equality, but it shows the utility of checks and balances in the current peer review system need improving. This is because: 1) the current system fails to utilize digital technologies of open review which have advanced multiple other scholarly disciplines; 2) the author nomination system facilitates the publication of friendly reviews; 3) anonymity in the review system permits reviewers to promote unchallenged, biased, or personally motivated publication decisions; and 4) impact factors might inspire editors to publish articles that might not otherwise be accepted. After first describing the Regnerus scandal and its political implications, the necessity of improvement in peer review on these four counts is discussed.

Keywords: Regnerus, sociology, peer review, open review

Introduction

A close academic colleague recently remarked to me:

Peer-review, like democracy, is the worst system except for all the others. It will sometimes fail miserably—giving us fodder for our research design classes. Still, I can't think of another feasible way to organize vetting of manuscripts for publication.

This article amounts to an extended commentary, designed to suggest to her, and my colleagues across the social sciences more broadly that—actually—there may be a better way of conducting peer review.

This paper was inspired by the publication of a politically harmful and methodologically faulty article. In (2012), Mark Regnerus published an article in *Social Science Research*, titled “How Different Are the Adult Children of Parents Who Have Same-Sex Relationships? Findings from the New Family Structures Survey.” As the title implies, this research concerns the outcomes of gay parenting on child development—ostensibly, as compared to children raised by a like group of heterosexuals. Unfortunately, the study design seemed to be stacked against the group of same-sex parents.

The Regnerus study involved the incompatible comparison of 1) adult children from heterosexual couples who have separated or divorced where one parent has subsequently entered a same-sex relationship for any specified period of time, with; 2) children raised by heterosexual couples without separation or divorce. The comparison of wholly unequal groups, unsurprisingly (and erroneously) concluded that there are disadvantages for children growing up in same-sex households. The uncontrolled tertiary variable is clearly the effect of separation and divorce on children's wellbeing. Thus, before discussing how the Regnerus paper went through peer review and was accepted for publication, we need to first discuss the misgivings of the paper, and its potential disastrous political implications for gay and lesbian equal rights. The idea is to remind social scientists that the misgivings of bad research not

only call into disrepute the author's reputation, but in this case it has helped erode civil liberties. It is for this reason we need to take the Regnerus case seriously and seek improvements in the peer review system.

The Regnerus Study

Further elaboration on the Regnerus paper is documented in a later issue of *Social Science Research*, 41(6) (Massey, 2012; Wright, 2012). The study's author (Regnerus 2012) asserts that he examines like-groups of same-sex parents raising their own children in intact households. Regnerus writes:

While the NFSS [New Family Structures Study] may best capture what might be called an "earlier generation" of children of same-sex parents, and includes among them many who witnessed a failed heterosexual union, the basic statistical comparisons between this group and those of others, especially biologically-intact, mother/father families, suggests that notable differences on many outcomes do in fact exist. This is inconsistent with claims of "no differences" generated by studies that have commonly employed far more narrow samples than this one (p. 765).

Given that Regnerus did not study intact LGBT households, who have their children since birth, he cannot measure or analyse the difference he alludes to. Regnerus further (2012) writes:

Even if the children in planned GLB families exhibit better outcomes than those from failed heterosexual unions, the former still exhibits a diminished context of kin altruism (like adoption, step-parenting, or nonmarital childbirth), which have typically proven to be a risk setting, on average, for raising children when compared with married, biological parenting ... In short, if same-sex parents are able to raise children with no differences, despite the kin distinctions, it

would mean that same-sex couples are able to do something that heterosexual couples in step-parenting, adoptive, and cohabiting contexts have themselves not been able to do—replicate the optimal childrearing environment of married, biological-parent homes (p. 765).

Here, Regnerus fails to consider that LGBT families raising children through these modalities would be compared to like-groups of heterosexuals raising children who are not from intact biological families. Again, there is ‘no difference.’

Regnerus (2012) also writes:

At a glance, the number of statistically-significant differences between respondents from IBFs [Intact Biological Families] and respondents from the other seven types of family structures/experiences is considerable, and in the vast majority of cases the optimal outcome—where one can be readily discerned—favors IBFs.

Yet, again Regnerus fails to immediately mention that his group of intact biological families is not compared to intact gay or lesbian families. He thus draws the term ‘optimal’ from a limited sampling of family structures. Finally, Regnerus concludes:

Do children need a married mother and father to turn out well as adults? No, if we observe the many anecdotal accounts with which all Americans are familiar... But the NFSS also clearly reveals that children appear **most** [emphasis added] apt to succeed well as adults—on multiple counts and across a variety of domains—when they spend their entire childhood with their married mother and father, and especially when the parents remain married to the present day (p. 766).

Again, Regnerus cannot claim that his result clearly reveals that children are ‘most’ apt to succeed when raised by their intact biological parents, because he has not researched children from intact opposite sex families to compare; nor has he studied a multiplicity of other family types (such as shared parenting or polyamorous families).

Same-Sex Parenting

Regnerus (2012) suggestion that same-sex parents produce poorer measures on childrearing outcomes is overwhelmingly disproved by more measured methods. Sociological consensus on the issue of same-sex parenting is however clear: children raised by same-sex couples maintain similar academic performance to children raised by opposite-sex couples. A meta-analysis of research (Lamb, 2012) highlights that their cognitive (Lavner, Waterman & Peplau, 2012), psychological (Wainright, Russell & Patterson, 2004), academic (Potter, 2012) and social (Crowl, Ahn & Baker, 2008) development are also equal to that of children raised by opposite-sex parents. Furthermore, children raised by same-sex couples maintain no disadvantage when it comes to risky or early onset of sexual activity (Patterson & Wainright, 2011) or substance abuse (Wainright & Patterson, 2006).

Decades of methodologically sound social science research from the fields of sociology, psychology and anthropology—and representing multiple nationally representative studies (Lavner, Waterman & Peplau, 2012)—indicate that positive child wellbeing is not the product of parents gender or sexuality, but instead a reflection of stability in the relationship between the two parents and (perhaps particularly) greater economic resources.

If this study's findings had been legitimate, there would have been no valid argument against the study's publication. Instead, the flawed preconceptions of the author and poor study design should have disqualified the paper from publication, and its acceptance is what calls into question the editorial and peer review process. It is not just because one bad article got through the system; it is because one bad article got through the system at a highly respected journal and it has had very serious, harmful, political implications for same-sex parents and same-sex marriage.

Political Outfall of the Regnerus Article

Regnerus' research has been widely adopted by political groups seeking to deny same-sex couples equality. Minnesota for Marriage, for example, highlights the study in a video series against gay marriage; and the notoriously socially conservative Family Research Council has also used the study to degrade same-sex parenting. More important, Regnerus's research has been cited in amicus briefs as providing evidence for the harm caused to children raised by gay and lesbian couples.

The paper was cited in the case of *Golinski v. United States Office of Personnel Management*, currently before a Federal appeals court—which may end up before the US Supreme Court. The U.S. Conference of Catholic Bishops also filed an amicus brief in the Supreme Court case *Hollingsworth v. Perry*, which is set to determine whether California's Proposition 8 ban on same-sex marriage is constitutional (of which the outcome will be released before this paper is published). In the brief, Regnerus' paper is cited to support the statement, "A mother and father each bring something unique and irreplaceable to child-rearing that the other cannot."

In August of 2012 U.S. District Judge Alan Kay cited Regnerus study in denying suit against Hawaii's gay marriage ban. His summary of the study: "(finding that children raised by married biological parents fared better than children raised in same-sex households in a range of significant outcomes)." And perhaps most significant, a group of religious organizations, including the Mormon Church, Southern Baptist Convention, and National Association of Evangelicals, have submitted amicus briefs to the United States Supreme Court arguing that it should uphold both the Defence of Marriage Act and California's Proposition 8. The briefs, written by Mormon Church lawyer Von Keetch, make points the inferiority of same-sex couples, citing the work of Mark Regnerus.

Modernizing Peer Review

The system of peer review adopted in the social sciences is designed to prevent this type of methodologically flawed research from publication. A research article is supposed to be sent to multiple independent reviewers for careful scrutiny of its methods, from data collection to analysis and interpretation. The Regnerus study instead seems to have been hastened to publication through securing reviews of academics who worked with or advised Regnerus on the study, hardly a source of non-biased review.

The publication of Regnerus's article therefore not only calls into question the values and integrity of the author, editor, and anonymous reviewers, but it also raises critical questions about the integrity of the system of review that social science research is subject to. The publication of Regnerus's article suggests that the peer review system is not robust enough; an extra layer of scrutiny is required.

If the publisher of the journal *Social Science Review* had fused technological advancements with the existing peer review system, the flagrant bias of Regnerus's work would have been detected before publication. Accordingly, an examination of contemporary practices in the peer review process operating in the social sciences is needed. We need to question whether we as social scientists are as rigorous as scholars in other disciplines. It would seem that our current system of peer review places too much importance on the reviewers and the editor, who even if dedicated to the reporting of social science without bias, are often overloaded with submissions—increasing the chances of unintentionally missing important details of any given submission. Furthermore, the now widespread system of authors nominating reviewers not only permits less criticality in the review process (as one can nominate like-minded colleagues), but that it also opens the system up to abuse from reviewers who are not held accountable for their decisions.

Part of the remedy to these issues comes from structural, technological changes to the manner in which academic research is reviewed and published, including more creative uses of open peer review systems already operating in other disciplines and among other, highly-ranked journals. We need to consider changes to the anonymity of reviewing in the social sciences, to the rejection of reviewers who have worked on the project with an author, and to the very manner in which reviewers are selected.

Open Peer Review

Digital technologies have made it possible to improve the peer review system. We no longer send three hard copies to a journal for review, and manuscript submission systems permit us to track the progress of our submissions. On-line- first articles also permit us to rush research into public consumption. These improvements have come from digital technologies to benefit the discipline. But we can also use digital technology to improve the accuracy and accountability of our research. Borrowing from the existing on-line first system, the idea is known as open review, and there are many iterations of it. I do not advocate any system in particular, but suggests that the social sciences need to engage with and evolve on the issue of peer review.

One system of open review holds that, after peer review and before acceptance, the editor places the manuscript into an access-controlled website where members of the editorial board are given an opportunity to comment on an article before it is designated as published. In this case, the journal generates an automatic email to all editorial board members, giving them the author, title and abstract. Additionally, the journal can generate an automatic list to authors cited within the article, adding another level of open peer review. Clearly, not all editorial board members will read every article, nor will all cited authors, but I suspect that most scholars would take the opportunity to at least examine the portions of an article in open

review that concern their own citations. In the case of the Regnerus paper, if the editorial board members received a monthly email, showing the abstracts of the articles placed into the waiting bay, at least one editor would have read the abstract and been alerted to the fact that an author was claiming something that went against the grain of the scientific body of research. Interested, he or she would have read the article, seen the obvious fault to the methods, and alerted other members of the editorial board.

Such a website can permit editorial members and/or cited authors to post comments for discussion by other members, creating an open and transparent review. The article's author could have access to the waiting bay as well, permitting them to see the postings concerning their article. This would mean that those posting critical commentary about the author's paper would need to be certain enough about them to state them without cover of anonymity. Editorial board members can be asked to check a box indicating 'I see reason/no reason why this article should be withheld from publication in its current form.' Rules of the journal can indicate that 60% (for example) approval is required before publication.

Upon closure of the open review period, the editor can evaluate the comments, requesting the author to address concerns; or, as in the case of Regnerus, discovering serious methodological flaws, and outright rejecting the paper. If *Social Science Review* had the 60% rule in place, the editor would be prohibited from publishing the article until the board members' concerns were addressed. Unfortunately, the way the system currently operates there is closed, anonymous peer review only. This gives the editor too much power (and burden) in making decisions.

Even without a voting system, giving editorial board members and authors cited in the work the opportunity to view an article before it is published would provide the opportunity for not only smaller mistakes to be corrected before its unveiling, but in a seriously flawed paper like the Regnerus study, the discussions that occurred in a later edition of the journal

41:6 would have occurred behind ‘closed doors.’ The article would have been denied publication before gaining attention of the media.

Such a system would not radically slow the time to publication, either. An article might appear for open-review for a period of just one week before being closed to open review. Furthermore, authors might appreciate such a system. Consider how frequently authors make minor, unintentional mistakes in their scholarship: perhaps citing the wrong date for research, or using incorrect spelling of author’s names. Illustrating this, one article recently quoted this author’s work, but left out a key word, thereby accidentally reversing the meaning of what my work was advocating. In the same article the authors wrote that the United States struck down sodomy laws 50 years ago, when this only occurred in 2002. An open review system could have caught these mistakes, making for a higher quality and more accurate publication. Authors of research should desire this type of communal fact checking. After all, the goal of academic publishing is supposed to be about producing quality scholarship, not just producing scholarship.

These types of smaller factual errors cannot be assumed to be within the remit of the anonymous reviewers, or the editor—their job is to be experts in the field of inquiry, not to serve as fact checkers. But subjected to open-review, a more accurate paper would have been published. In the case of the quoting of my paper, I would have received an email alerting me that my research was being used in an article and inviting me to participate in open review. I would have found mistakes and commented on them so that they might be addressed before appearing in publication. In other words, the open review system could permit a community of scholars to assist with fact checking.

For the Regnerus paper, a system of open review might have easily detected what Darren Sherkat (2012: 1347) has written about the review processes for the Regnerus paper after conducting an audit of it:

I see little serious engagement of the papers in any of the reviews. Both papers have serious flaws and distortions that were not simply ignored, but lauded in the reviews. Given that the reviewers were mostly comfortable conservatives, it is not surprising that the reviews were very positive, and contained minimal critique of the data, measures, or methods used in the studies. This lack of critical reflection on the part of reviewers could be because of ideological blinders, but it is also certainly related to reviewer fatigue—if you generally like a paper and have four more on your desk to review, you may not bother pointing out what you think are minor flaws (even when those flaws are not minor).

It is important to note here that a journal employing an open review system could choose (if desired) not to permit critiques based on theoretical choice. For example, I have been critical of R.W. Connell's (1987) hegemonic masculinity theory (Anderson, 2009). Some however might disagree with my theoretical position. In this case, convention of the open review system could dictate that Connell not critique me for this and I not critique her. She could indicate that the reviewers have missed a serious methodological flaw; she could highlight that I spelled an author's name wrong; but she could not register a veto because she disagrees with my take on her theory. I would have those same constraints in reviewing her work. This stops gate-keepers from preventing a discipline from theoretically evolving.

Open review would also help prevent an editor from serving as an ideological funnel for ideas or theory. It would also help keep in check an editor who might have a desire to cause social harm through the acceptance of a politically damaging article. In the case of Regnerus's article, it has been reported that the editor is not ideologically aligned with Regnerus. I asked his views on same-sex marriage and he responded with, "As for my personal political beliefs, no one who knows me doubts for an instant my commitment to equal civil rights for all people, including marriage and adoption rights for LGBT people."

However suppose an author, editor and three reviewers were ideologically determined to thwart a particular group's civil liberties. An editor could easily invite a submission from an author using faulty data, have it reviewed by those ideologically aligned, and publish it without the rest of the board knowing better. With an open review system, however, it would only take one member of the editorial board to see the faulty research and to alert the mother members of the board. These editors would then veto publication.

For those concerned with the time to publication, or a burden on editorial board members, one can, determine that only articles with policy implications (such as the Regnerus paper) be subject to this extra scrutiny—as it is these papers which can have a large impact when released to the media. Or, two editorial board members can be asked to review the abstracts for forthcoming articles and see if any are worthy of being placed into open review. There are many ways open review can be done. This proposed design is not the only possibility.

It is also worthy to note that Editors themselves might appreciate having further checks of their power. In the case of Social Science Research Editor, he acknowledges (Wright 2012) that peer review frequently does fail:

...that reviewers (and editors) often make mistaken judgments, fail to see flaws of conception and implementation, let their own biases, proclivities and preferences cloud their assessments, and thus recommend papers for publication that still need revision or perhaps should never be published at all, etc., all of which would follow from the fact that reviewers (and editors) are human (p. 1343).

Given the professional, political and social outfall from this article, one would imagine the editor would much rather have had the article be viewable in a weighting bay for further commentary before committing it to publication.

Again, this proposed system is just one of numerous iterations that journals can combine digital technologies and the willing labor of self-interested academics to promote scholarship. In fact, similar systems are in operation elsewhere. Other journals have implemented it. Theodora Bloom writes (Nature 2006):

preprint servers and online comment and reviewing systems allow an article to accrue commentary and analysis after it is published. This ranges from the 'post a comment' option operated by many online journals to the two-stage system used by *Atmospheric Chemistry and Physics*....

The open-access journal, *Electronic Transactions on Artificial Intelligence*, offers a promising open-review system. It begins the reviewing process as soon as an article is submitted, at which time it is posted on their website and advertised to the peer review community via e-mail. A three month discussion period occurs, where all comments are made openly. Authors are then given the chance to revise the paper before it is sent out for expert (anonymous) review. If accepted, the article is then included in an issue of the journal. This is a more rigorous system, adding to the already existing system. In order to avoid copyright infringement, the journal publishes the article on their website with copyright.

Changing the Reviewer Nomination System

If we are to promote unbiased peer review, journals also need to end the practice of permitting authors to nominate reviewers for their work. In the case of the journal under scrutiny, *Social Science Research* permits a submitting author to designate up to five preferred reviewers.

Ostensibly, asking the author of a submission to nominate preferred reviewers helps managing editors find willing and qualified reviewers for a manuscript. Ideally, authors also nominate individuals who are experts in their field, as submitting authors ought to be

knowledgeable about their research to provide informed recommendations. This is likely particularly true for a general journal, compared to a specialty journal. No editor, even with outstanding editorial assistants, can know multiple disciplines and their related experts.

Author nominated review systems are also helpful for journals with a high number of submissions; in that it helps managing editors secure reviewers. This is/was the case with the journal in question here, too. *Social Science Research* Editor, James Wright, wrote (2012: 1339):

The task of editing a journal and operating the peer review mechanism is an onerous one and not without its complexities. SSR now receives approximately 350 new submissions each year; and each year, we must also process a hundred or more revisions of papers that were originally submitted in previous years... . Regardless of the ultimate decision, every paper is reviewed by at least two referees; ideally, by three referees; and sometimes, depending on circumstances, by four or more referees. However, the system of authors nominating reviewers also opens up the likelihood of authors simply recommending their friends or like-minded colleagues, facilitating an easier and quicker route to publication. In permitting authors to select preferred reviewers, we promote the publication of scholarship with less judicious scrutiny.

Evidencing this claim, Wager and Parker (2006) found that author-nominated peer reviewers provide reports of similar quality and tone to reports by editor-chosen reviewers, but that the former are less likely to recommend rejection during the initial round of review (Wager & Parker 2006). In other words, papers are rejected less when the author nominates ideologically aligned colleagues. In the case of the Regnerus article, two of the three reviewers were actually paid consultants on his study.

Journals also need to cease the practice of asking authors to review papers for their journal when they have a submission under review themselves. This serves as one way of a

journal increasing its reviewer pool; however, it is problematic for two reasons. The first is that authors who are awaiting decision on their manuscript might be more inclined to reject another's article, effectively removing it as competition for limited publishing space. Second, when an editor asks the author of a submitting article to review an article before a decision has been reached, it places pressure on the author to agree to the review, even if they do not feel fully qualified to review it. After all, who wants to say no to an editor who holds the fate of your article in their hands?

In the case of *Social Science Research* article in question we do not know whether reviewers were chosen by recommendation of the author. In his account of events neither the editor, nor the independent auditor of the Regnerus events, comment on this. In describing the selection of reviewers for the Regnerus paper, all Editor James Wright tells us is that he lumped the Regnerus paper's reviewers with another paper, securing six reviews of the two papers. What the independent auditor suggests is that three of the six reviewers were "on public record opposing marriage rights for LGBT persons and the other three, presumably, had more moderate or liberal leanings" (Wright, 2012: 1342).

The scenario also highlight the need for reviewers to dismiss themselves from reviewing work that they themselves will profit from (financially or socially), and editors rejecting reviews from those. In the case of the Regnerus paper, two of the six referees were also paid consultants to the Regnerus survey. The editor knew of their association yet permitted them to proceed, with their 'assurance' that they would be objective. And while the editor of the journal in question here has tried to downplay their association by serving on a board designed at the beginning of the project, the independent auditor has taken a more critical stand:

Can you make an unbiased decision about research when you have consulted on a project? When you are former colleagues? When the paper is authored by a former

coauthor? When you have been funded by the same foundations? The answer is ‘maybe not’ (cited in Wright, 2012: p. 1343).

Removing Anonymity

Another approach to limiting the effect of friendly bias in the reviewing of research comes through removing the anonymity of reviewers. Regnerus’s article indicates that ‘truth’ and ‘objectivity’ is sometimes subverted, not enhanced by anonymity. Instead, it can enable the introduction of unchallenged bias and self-interest. Reviewers, unaccountable to anyone for their reviews, face little or no consequences when they use the review processes for nefarious purposes. While this behavior may not be frequent, Regnerus’s article certainly shows that it exists. As long as anonymity is considered the norm, the seeds of abuse are much more fertile.

One way to mitigate the problems of anonymous reviewing is to follow the procedures of the publisher Frontiers. This publisher has all reviewers who sign off on a paper for acceptance listed in the final publication. This practice holds reviewers more accountable for published work. In the case of Regnerus’s paper, one must wonder if the reviewers would have accepted the article knowing that their names would be attached to it. Conversely, authors of rejected papers would not know the names of their reviewers.

I recognize a counter to this argument. It might very well be that making evident the reviewers of an article scares more reviewers away, making it even more difficult to get reviews for an article. This can be mitigated by publishers (not individual journals) requesting that each author of a published article review four articles within their specialism before again publishing with that publishing company. Or perhaps publishers can pay reviewers a modest reviewing fee, as publishers of books do to authors examining book proposals.

Eliminating Impact Factors

Finally, there was concern that the editor of the Regnerus paper may have pushed the paper not because he wanted to cause harm to same-sex parents and the LGBT community, but instead because he rightfully assumed that a controversial paper would raise the journal's impact factor, the most frequently cited bibliometric for ranking journals. This method measures the volume and pattern of citations given to journals, where the amount of citation is said to be an indicator of the impact of a journal within its discipline. The Regnerus study will undoubtedly improve *Social Science Research's* impact factor, even if the study is cited only as an exemplar of poor scholarship. Perhaps a better method of ranking journals is to do so qualitatively; this is how the Excellence in Research for Australia (ERA) methods ranks journals. Articles are read from a journal and evaluated against articles read from another journal. This provides a better measure of quality. If scholars paid more attention to the ERA rather than Thompson's Impact Factor, an article like the Regnerus paper would be more likely to be rejected, as it would lower the quality of ranking.

Conclusion

Peer review is the essential component of the academy. It is peer review which sets academic publishing in the social sciences apart from journalism: we require evidence, and we require that evidence be measured by experts. But peer review is not perfect. In commenting on the Regnerus publication Douglas Massey wrote (2012):

Science never rests on a single study. It realizes that any single piece of research might be flawed or misleading in a variety of ways. The best we can do is subject a piece to peer review and hope for the best, recognizing that peer review is itself an imperfect process (p. 1378)

I suggest that Massey's *laissez faire* attitude concerning peer review is defeatist. The system of peer review is not timeless or static; it can be improved.

Two issues in this commentary reinforce each other. The first concerns the serious flaws in the Regnerus paper. It was this issue that inspired me to critically examine the situation which led to the publication of an article which appeared biased against same-sex parents. The second is a critical examination of how the Regnerus paper was published in the first place. These two issues are inseparable: they beckon us, as a community of social scientific scholars, to both react and improve upon our peer review system. Whether the publisher and journal change their practices in light of this problematic publication or not signals whether we, as a scientific community, are resolute in preventing such from happening to other groups of people with diminished cultural power.

The audit of the processes over the Regnerus paper (Sherkat, 2012) revealed that there was little serious engagement of the papers in any of the reviews, and the editor should also have given greater scrutiny to an article which not only went against the grain of decades of sociological scholarship, but one that had serious policy implications for sexual minorities. In short, this article had 'red flags' all over it; yet he rushed it into publication at a politically sensitive time. None of this would likely have happened had the editor not had unchecked-power and responsibility. This would have been less likely to happen had the journal in question adopted an open system of peer review.

Thus, speaking to the primary purpose of this commentary, I suggest that there exist digital technologies to check for accuracy when authors, reviewers and the editor all either fail, or intentionally seek to subvert the peer review system. These technologies can be achieved with minimal financial investment or human capital requirements, and they produce better accountability, transparency and ultimately better scholarship. It would behave

Elsevier, the publisher of *Social Science Research* to implement new peer review procedures as reparation for the damage caused by their journal.

The fault with the Regnerus article principally lies with its author. This article adopted a poor methodology and made politically motivated and charged conclusions that spoke well-beyond what the data permitted. As academics, we are responsible for carefully crafting our research methods to promote the most scientifically accurate data possible. We are responsible for checking quotations of others' work; contextualizing them by reading the works from which they were generated; and making sure that we construct our sentences as not to obfuscate meaning. But sometimes we do make mistakes; other times there may be mal-intent. The peer review processes was established to provide checks and balances in controlling for bias in the acceptance of papers for publication. However, the system does not always work. I therefore advocate for the use of digital technologies in creating a broader sense of open review of academic work. This should benefit the author, the discipline, and hopefully the citizens for whom our research oftentimes addresses. For further debate and reading on open-reviewing and studies of author-nominated reviewers see (Nature, 2006).

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