



The Editorial Process and Politicized Scholarship: Monday Morning Editorial Quarterbacking and a Call for Scientific Vigilance[☆]

Darren E. Sherkat

Department of Sociology, Southern Illinois University, Carbondale IL 62901 USA

The publication of Mark Regnerus' research paper claiming to show that gays and especially lesbians have poor parenting outcomes has unleashed a firestorm of controversy among social scientists who study family and sexuality—fanned by the author's cultivation of popular media reporting on the study. Less attention has been paid to the review paper by Loren Marks, arguing against the American Psychological Association's stance that same-sex parenting is not dysfunctional, but the process by which *Social Science Research* published that paper is also in question. At the request of the editor, Professor James D. Wright (and at the suggestion of Dr. David Brady of Duke University), I have independently reviewed the submission and review process for both papers, and I report on that process from a perspective of editorial realism. I do this with an eye towards explaining how available and customary procedures can lead to substandard outcomes. I will leave the more detailed critiques of Regnerus' and Marks' studies to other scholars.

The publication of the Regnerus paper raised red flags for many scholars, including myself, and the timeline of the data collection, article submission, and acceptance led many to assume that *Social Science Research* colluded with the author and violated standard editorial policy as well as ethical standards. Indeed, the timeline of Regnerus' data collection effort and paper submission is highly unusual—Regnerus wrote and submitted the first draft of the paper before the data collection had been completed. Regnerus did not indicate that data collection was ongoing in the first draft of his paper. The paper was reviewed, revised, and accepted in only 6 weeks. Normally, an editor would be praised by the scientific community for accomplishing such a miraculous turnaround time for manuscript reviews, but in this case many questioned Wright's actions and implied a nefarious motive. Wright provided me with all of the reviews and reviewer information, along with the timeline of correspondence, and I will summarize the review process and how I see it in terms of the normal realities of editing a major journal like *Social Science Research*.

This incident provides us with an opportunity to reflect on the peer review process, and how we critically evaluate research. There are new political realities which social scientists, perhaps especially sociologists, have not previously had to engage. And there are also constant issues of conflicts of interest that need to be better addressed in the editorial process—and here the onus lies as much on reviewers as on journal editors to admit when one is too close to an author or an issue to make a valid judgment about the worth of the research.

1. Editing is hard, and peer review is a crap shoot

Most scholars give little reflection to what it takes to edit a scholarly journal, and very few will ever take up the task. *Social Science Research* has received a record-breaking number of submissions each year for the last 3 years, and is on track to process more than 350 new submissions this year (plus a hundred or so revisions of papers originally submitted in previous years). Wright processes these manuscripts with one assistant and a student worker. For each manuscript at least three reviewers have to be found to provide reviews in a timely fashion, and revised manuscripts and conflicting reviews often require soliciting additional reviewers. *Social Science Research* has one of the best turnaround times among the top-tier in the social sciences. Social scientists are becoming increasingly uncooperative as reviewers in an era of increasing scholarly

[☆] Comments and information from Neal Caren, Phillip Cohen, Lisa Keister, Shamus Khan, Debra Umberson, and Rhys Williams were helpful. Thanks also to James D. Wright for providing a thorough accounting of the editorial history of the two articles.

E-mail address: Sherkat@SIU.edu

output, and finding three willing reviewers usually takes six or more solicitations. Who cooperates and when is an issue of concern and structural features like editorial boards help but do not guarantee that reviews are produced. The result is that the review process may not always be as thorough or as objective as one would hope.

Like many journals, *Social Science Research* relies heavily on its editorial board for manuscript reviews, and members of the board doing research on popular topics are often called upon to review manuscripts. Wright also consults board members regarding potential reviewers. Given the frantic pace of scholarly output and paper submissions, imperfections can arise from several quarters. Both of the articles in question fit squarely in the area of family and sexuality, and having two papers on a similar topic in the review process at the same time crowds out potential cooperative reviewers. There has been a marked increase in research on sexuality and the family, and because of this many scholars are up to their ears with manuscript reviews. As is normal, Wright turned first to two editorial board members who work on topics related to the papers—and one of these board members reviewed both papers. Wright attempted to get five reviews for the Regnerus paper and he secured three reviewers, while all four scholars who were asked agreed to review the Marks paper (which is unusual). Two of the reviewers indicated that they had a potential conflict of interest related to consulting on the Regnerus paper but both averred that this consulting relationship would not preclude an objective, critical assessment; another reviewer reported that he had read a previous draft of the Marks paper (the reviewer claimed that he did not otherwise know Marks and had never met him in person). All of the reviewers provided quick feedback, and all of the reviews were positive. The editor required revisions in both articles as a result of the reviews, and the revisions were completed quickly and successfully with the guidance of the reviewers and the editor.

Who is asked and who cooperates to do a review is a somewhat random feature of academic publication. In a general-interest journal like *Social Science Research*, the editor cannot possibly know of the conflicts of interest or particularities of controversies in subfields far beyond his own. *Social Science Research* is a quantitative journal, and the editorial board and reviewers reflect that orientation. On the issue of sexuality and family, most studies have been smaller scale and qualitative; and scholars doing this sort of research would be unlikely to review for a quantitative journal like *Social Science Research*. Quantitative family scholars tend to be conservative, and three of the six reviewers for these two articles are bone fide conservatives—scholars who are on public record opposing marriage rights for LGBT persons. Wright indicated to me that he knew that one of the three conservative scholars held these commitments, but he did not know of the religious/political activism of the other two. This puts Wright even with me—I knew that one of the three was a religious and political conservative, but I had to search for the public views of the other two (and I did not know of the conservatism of the scholar Wright was familiar with). Five of the reviewers are very regular, reliable, *SSR* reviewers, and all six were notable scholars. Indeed, the three scholars who are not publicly conservative can accurately be described as social science superstars. I should also note that none of the reviewers were female, and all but one was over 50 years old.

The reviewers are not without some connection to Regnerus. Two admitted to being paid consultants on the Regnerus study, and it would have been ideal to solicit an additional review. However, at that point, Wright was sitting on three completed reviews by distinguished scholars and Editorial Board members—all of them advocating publication. I do not know which of the six reviewers reviewed which of the two manuscripts; however, two of the six reviewers are former coauthors with Regnerus (though neither has written with him in a decade). In any case, with two exceptions the reviewers certainly did not mention their conflicts of interest, and the editor could not have known. Can you make an unbiased decision about research 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.” More people should turn down reviews because they have conflicts of interest. Both Regnerus and Marks got a lucky roll of the dice—in large part because the *SSR* die are loaded in favor of conservatives in the area of family, and because scholars who should have known better failed to recuse themselves from the review process. It can be argued that Wright should have been more aggressive in asking about such conflicts but this is probably unrealistic given the large number of papers *SSR* processes.

2. Glad-handing reviews and the editor's echo chamber

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).

I was very disappointed to see a paper like the Marks paper published in *SSR*. While it appears to conduct a lowbrow meta-analysis of studies of the effect of same sex parents on children, no original data were collected or analyzed, nor was a systematic meta-analysis conducted. It is an argumentative review paper trying to make a case against a particular conclusion in an APA brief. Reviewers should have known that this was inappropriate for a journal that publishes original quantitative research. Indeed, the reviewers seemed so enamored with the basic argument in the Marks paper that they failed to notice that it does not fit the aims of the journal. Three reviewers voted “accept but suggest revisions”, and one reviewer suggested “revise and resubmit”. The manuscript was revised, resubmitted, and reviewers agreed it should be published.

The Regnerus paper received even more favorable reviews. Reviewers uniformly downplayed or ignored the fact that the study did not examine children of identifiably gay and lesbian parents, and none of the reviewers noticed that the data were inappropriate for a top-tier social scientific journal, particularly given the marginal population to which these estimates pertain. Throughout the paper, Regnerus points to the social and psychological deficits of children of gays and lesbians; yet, the study found only a handful of children of gays or lesbians. This is not a small semantic issue, since it cuts to the heart of what Regnerus has argued in his paper and in the media. Regnerus' peculiar definition of "gay" and "lesbian" also guided his selection of respondents. Regnerus' reviewers are effusively positive and point to ways to further bolster his argument and justify his use of a very bad measure on flawed data. What was the vote count? Two "accept with suggested revisions", one "accept with revisions". The manuscript was revised, and reviewers thought it worthy of publication.

If I were in Wright's shoes, I may well have made the same decisions. I might have desk-rejected the Marks paper, but I may not have noticed that the research was not real (it does appear to have tables). I might have read the Regnerus paper and realized it was substandard, but how many of those 350 manuscripts a year are you going to read with full reviewer goggles? And, it is unfair to expect Wright to hear the warning sirens when none were sounded by the reviewers. Of course, I also might also have noticed the political bent of the two papers and made sure that the reviewers were more diverse in their perspectives. One thing that was sounded by all the reviewers was that these papers will stimulate enormous interest. This is something that has helped bolster research on sexuality in the last few years (left, right, and center), and it is well known in social scientific publishing circles that sexuality is the hot-button download ticket. Indeed, last year the top two downloaded papers in *Social Science Research* were on homosexuality and written from a pro-civil rights perspective; and I was a coauthor of one of them (Sherkat et al., 2011). Our paper was accepted by Wright over the objections of two conservative reviewers—out of three reviews, two were certainly voting "reject". The accusation that Wright was somehow part of a conservative conspiracy to undermine civil rights for LGBT persons is ludicrous, and I know this from his prior actions.

3. Data quality, political interests, and scientific research

The reviewers and the commentaries on the articles uniformly failed to attend to a key problem with the Regnerus study—it estimates population parameters based on a sample from a panel maintained on-line by a research firm. The Regnerus data were financed by two extremely conservative foundations (the Bradley Foundation and the Witherspoon Institute), who contributed \$795,000 to pay for this research (according to Regnerus' vita). Notably, these conservative institutions are not well known—and the editor was unaware of the political bent of these foundations. There should be reflection about a conservative scholar garnering a very large grant from exceptionally conservative foundations to make incendiary arguments about the worthiness of LGBT parents—and putting this out in time to politicize it before the 2012 United States Presidential election.

The data were derived from a panel survey constructed from a national probability sample of households and also from a random digit dialing survey (the percentage of panelists recruited using each method is unclear). Panel members were recruited using monetary incentives and free internet and computer hardware, and panel members are expected to complete 2–4 surveys per month. There is no indication from Regnerus on the percentage of randomly targeted individuals who agreed to participate in the panel, or on the initial response rate of the RDD survey. Given the standards that prevail, it is likely that the recruitment rate is extremely low for both the RDD and "address based" sampling. I am unclear on the basis for the national representativeness of the study, particularly given the skewed distribution on gender, religiosity, and other measures. And, given that only 1.7% of respondents were (mis)classified as children of "gays" or "lesbians", these data are certainly not up to the task of adequately informing our understanding of same sex parenting. Regnerus' web page shows that the panel has suffered 34% attrition (what are called "withdrawn panelists"), and only 61.6% of the current panel responded to the Regnerus survey. Regnerus uses withdrawn panelists (only 21.6% of whom responded) to sample people who claim that a parent had a "romantic relationship" with someone of the same sex, but withdrawn panelists are not used for the sample of respondents with biological, intact, "heterosexual" parents. Given that withdrawn respondents were likely withdrawn because of concerns about their reliability as members of the data panel—it is inappropriate to have 11% of the fictive children of "gays and lesbians" recruited from these withdrawn panelists.

Predictably, there are several red flags in these data. The "nationally representative" panel is 32.7% male and 67.3% female. Respondents who claimed that their mother had a romantic relationship with another woman were disproportionately minority: 45% were white; 26% African American; 17% Hispanic; and, 12% other. The Regnerus codebook also reveals numerous unlikely responses, for example: 26 respondents had vaginal sex before the age of 8–10 of them at age 0; 20 male respondents have had sex with more than 100 women, while 16 female respondents have had sex with more than 100 men; Two respondents were pressured into have sex with their parent/adult caregiver for the first time after the age of 30. Ten respondents have been pregnant a dozen or more times; and, 15 respondents had sex more than 30 times in the last 2 weeks. In the rush to complete this paper before the data were even fully collected, data cleaning was apparently not something in the research agenda. Yet, none of these problems were transparent to either the reviewers or the editor, and would only be revealed by a careful analysis of additional materials from Regnerus' website.

Data quality has declined over the last 30 years, and much of the social scientific response to this decrease in quality has been resignation and a revision of scientific standards. The fact that similar data have been used in other peer-reviewed studies should not be used to justify Regnerus' use of marginal data to study an important and contentious issue. Nobody should

expect to publish a paper in a journal of the tier of Social Science Research on crucial questions using data collected in this manner. Indeed, the “gold standard” of research on family outcomes would require a randomly drawn sample of parents and children followed longitudinally and interviewed by a human. Commenting on the specific parameter estimates produced by such a non-scientific study merely privileges this “research” and enables it to inform public debate on consequential issues regarding sexuality and civil rights. The “debate” begins with a study with a questionable sample and inappropriate measures commissioned by an activist–scholar with funding from conservative foundations; but science is not a debate. It is notable that the day after publication of the Regnerus study it was cited in an *amicus curiae* brief by a conservative Christian political organization to justify denying marriage rights to same sex couples (http://www.ca9.uscourts.gov/datastore/general/2012/06/11/12-15388_Amicus_brief_American_College.pdf). Thankfully, other scholars and scholarly associations (including the American Psychological Association) have filed amicus briefs countering the claims made in Regnerus' severely flawed study and by the anti-LGBT activists who support it (http://www.ca9.uscourts.gov/datastore/general/2012/07/10/12-15388_Amicus_Brief_Psychological.pdf).

4. The numbers game and scientific priorities

My review of the editorial processing of the Regnerus and Marks papers revealed that there were no gross violations of editorial procedures—the papers were peer reviewed, and the “peers” for papers on this topic were similar to what you would expect at Social Science Research. Obviously, the reviewers did not do a good job—because of both ideology and inattention—but the clear signal to the editor was “publish these papers”. Still, once they were accepted there was an unseemly rush to publication (at least for the Regnerus paper), and that was justified based on the attention that these studies would generate. The published responses were milquetoast critiques by scholars with ties to Regnerus and/or the Witherspoon Institute, and Elsevier assisted with the politicization by helping to publicize the study and by placing these papers in front of the pay wall.

Because of the race to get these into print—certainly to boost the number of hits on Social Science Research's meter—Wright picked people he knew would write something in a timely fashion. It is easy to produce a quick response when you are a paid consultant and are already familiar with the project. All three of the respondents to these papers have ties to the Witherspoon Institute: Professor Osborne is a “key collaborator” on the Regnerus study, Professor Amato was a consultant, and Professor Eggebeen is a signatory of a Witherspoon declaration that limits marriage to heterosexual couples (“Marriage and the Public Good: Ten Principles” [http://www.winst.org/family_marriage_and_democracy/WI_Marriage.pdf]). Notably, unbeknownst to the editor, Eggebeen has also been a vocal opponent of same sex marriage who testified before the Hawaii Supreme Court. Wright did try to get Professors Charlotte Patterson and Michael Rosenfeld to be respondents, but he could not secure their cooperation. It seems possible that if these papers were held up for a more normal “backlog” of publication several distinguished and critical reviewers would have happily responded.

Controversy over sexuality sells and in only a week after publication these papers have already skyrocketed to the most downloaded papers published in Social Science Research. But neither paper should have been published, in my opinion. Undoubtedly, any researcher doing work on same-sex parenting will now have to address the Regnerus paper, and these citations will inflate the all-important “impact factor” of the journal. It is easy to get caught up in the empirical measures of journal success, and I believe this overcame Wright in driving his decision to rush these into print. The fetishism of the journal impact factors comes from the top down, and all major publishers prod editors about the current state of their impact factor. Elsevier is particularly attentive to this and frequently inquires about what Wright is doing to improve the already admirable impact factor of Social Science Research. As social scientists, popularity should not be the end we seek, and rigorous independent evaluation of these manuscripts would have made Social Science Research a less popular but better journal.

There are a few things that might help prevent papers like these from falling through the peer review process at Social Science Research. First, the Editorial Board needs expansion and diversification. Given the number of manuscripts being processed, the Board is too small, and perhaps a bit too old, straight, white, and male. Second, Social Science Research should begin the review process with a series of prompts about the author and the study (SSR is a single blind journal, so reviewers know who wrote the paper) to ascertain whether reviewers may have a conflict of interest. Third, the comments for the author/editor form should require that reviewers directly assess the quality of the data, measures, and analysis relative to the standards expected in a substantive area. This is especially important since reviewers may not recall that Social Science Research only publishes original research (not literature reviews, essays, or theoretical papers) and is a top-tier quantitative journal where data, measures, and analytic methods should be of highest quality.

Reference

Sherkat, D.E., Melissa, P.-W., Gregory, M., Kylan de Vries, 2011. Religion, politics, and support for same-sex marriage in the United States, 1988–2008. *Social Science Research*. 40, 167–180.