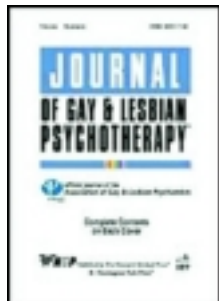


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Are Children of Parents Who Had Same-Sex Relationships Disadvantaged? A scientific evaluation of the no-differences hypothesis

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Are Children of Parents Who Had Same-Sex Relationships Disadvantaged? A scientific evaluation of the no-differences hypothesis

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Abstract

In a widely publicized and controversial article, Regnerus (2012a) seeks to evaluate what he calls the “no-differences' paradigm” with respect to outcomes for children of same-sex parents. We consider the scientific claims in Regnerus (2012a) in light of extant evidence and flaws in the article's evidence and analytical strategy. We find that the evidence presented does not support rejecting the “no-differences” claim, and therefore the study does not constitute evidence for disadvantages suffered by children of same-sex couples. The state of scientific knowledge on same-sex parenting remains as it was prior to the publication of Regnerus (2012a).

Keywords: discrimination, family, adolescent, children, parenting

In a widely publicized and controversial article, author Mark Regnerus (2012a) seeks to evaluate what he calls the “no-differences’ paradigm” with respect to outcomes for children of same-sex parents. Using a survey dataset collected for the purpose, Regnerus claims to provide the first systematic evidence that children of same-sex parents suffer disadvantages relative to children of “intact biological families.” Here we present a comprehensive scientific evaluation of the data, analysis, and conclusions of Regnerus (2012a), focusing on whether its main conclusions are supported by the evidence. We demonstrate that they are not.

Much of the controversy surrounding the Regnerus study involves its political ramifications and questions of the propriety in the review process. These are important considerations—within days of the article's release, Regnerus had provided a popular article opening the gates for political interpretations (Regnerus 2012b). The American College of Pediatricians, a small association formed in 2002 to pursue a socially conservative agenda, deployed the study in a legal brief supporting the federal Defense of Marriage Act on the grounds that gay marriage is harmful to children (American College of Pediatricians 2012). Over 200 scholars signed a letter to the editors of *Social Science Research* (SSR), which published the article, decrying the paper’s scientific flaws and political implications (Gates et al. 2012).

In addition, the timeline between data collection, analysis, article submission, and publication was extremely compressed relative to most social scientific studies, with the final paper accepted one month after the end of data collection (Cohen 2012). An internal audit

performed by a member of the editorial board of *Social Science Research*, which published the article, criticized its publication while recognizing the institutional pressures that led to publication (Sherkat 2012). And a journalist has presented evidence that one of the article's likely peer reviewers was also heavily involved in the political work of one of its funders as well as in the study's design and execution (Rose 2012) -- a conflict of interest that contradicts the article's claim that "the funding sources played no role at all in the design or conduct of the study, the analyses, the interpretations of the data, or in the preparation of this manuscript" (Regnerus 2012a) and should certainly have resulted in disqualification of one or more reviewers (American Sociological Association 1999; Resnick 2012).

In the present article, however, we do not take up these questions of publication integrity and political effects. Rather, we consider the scientific claims in Regnerus (2012a) in light of extant evidence and flaws in the article's sample, evidence, and analytical strategy. We find that the evidence presented does not support rejecting the "no-differences" hypothesis as it actually exists in the substantial literature prior to Regnerus (2012a). The study therefore does not constitute evidence for disadvantages suffered by children of same-sex couples.

The article proceeds in three parts. First, we review the origins and construal of the no-differences hypothesis and provide a specification for the hypothesis that is more faithful to the prior state of scientific knowledge that motivated the Regnerus study. Second, we outline flaws, mistakes, and omissions in the analytic strategy, the sum total of which requires that we not reject the no-differences hypothesis. Finally, we offer alternative mechanisms and hypotheses that provide a better explanation for the findings published in Regnerus (2012a). Despite numerous requests to Regnerus and *SSR* editors, neither supplementary analysis that Regnerus

stated was already available, diagnostic statistics (e.g., standard errors) nor the raw data had been provided as of this writing, so we were not able to evaluate the underlying quality of the data or assess these alternative mechanisms and hypotheses. Since the present article was submitted, the raw data have been uploaded to the Inter-University Consortium for Political and Social Research (ICPSR) so they can be analyzed by future scholars, but when we wrote the present article they were not yet available. Our assessment is therefore limited to the data and analyses as presented in Regnerus's original article.

The “no-differences” hypothesis

Regnerus states the no-differences hypothesis thus:

H1: children from same-sex families display no notable disadvantages when compared to children from other family forms.

Stacey and Biblarz (2001) offered a good criticism of this research program, arguing that the deficit model it ignores potentially important qualitative or positive differences between family structures. Another article in the same issue of *Social Science Research* (Marks 2012) provides an extended criticism of the scientific process that led to the adoption of a related statement from the American Psychological Association's 2005 brief report on same-sex parenting, which holds that (2005:15):

Not a single study has found children of lesbian or gay parents to be disadvantaged in any significant respect relative to children of heterosexual parents.

Marks takes this statement to task by evaluating the various studies examined for the brief and arguing that none of them is sufficiently scientifically rigorous to justify that summary claim.

Regnerus does similarly at the outset of his article:

Suffice it to say that versions of the phrase “no differences” have been employed in a wide variety of studies, reports, depositions, books, and articles since 2000.

Both Marks and Regnerus make a key epistemological error. The APA report and cited research conclude essentially that there is no evidence of systematic difference. In scientific terms, they test a different hypothesis:

H2: Children from same-sex families display notable disadvantages when compared to children from other family forms.

And, in general, they reject that hypothesis based on the evidence available. Marks and Regnerus treat this as if the reports conclude that there is conclusive evidence of lack of systematic difference: as if the null hypothesis (H1) were confirmed. But rejecting hypothesis H2 is not the same thing as proving the null hypothesis (H1), a key distinction present in the relevant literature, but one that Regnerus ignores. Since Regnerus (2012a) is motivated by the ubiquity of the “no-differences” hypothesis, and the lack of support for H2 is the actual state of the literature prior to the publication of Regnerus (2012a), adequate evidence to support H2 is the appropriate standard for rejecting the no-differences hypothesis. If there is sufficient evidence to support H2 with confidence, the “no-differences” hypothesis should be rejected; if there is not, the “no-differences” hypothesis stands as the current state of knowledge. In the next section we evaluate the evidence presented by Regnerus to determine whether it is sufficient to provide support for H2 and therefore to reject the no-differences hypothesis.

Study Design, Analysis, and Conclusions

Regnerus's article introduces a new dataset, the New Family Structures Survey (NFSS), which Regnerus collected for this purpose. The study uses a Knowledge Networks sample of about 3,000 respondents born between about 1981 and 1994. It asks about a variety of characteristics of interest as well as for information about the respondents' families of origin, including a diary of with whom the respondent lived for each four-month block during his/her childhood between birth and age 18 (these diary data are not analyzed in the article). It also screens participants in the panel by asking:

From when you were born until age 18 (or until you left home to be on your own), did either of your parents ever have a romantic relationship with someone of the same sex?

Respondents who answered “yes” were classified as children of lesbian mothers (LM, renamed to LM/MLR, for Maternal Lesbian Relationship, in Regnerus 2012c) or gay fathers (GF, FGR in Regnerus 2012c) (depending on which parent they recalled having such a relationship), and compared to respondents from “Intact Biological Families” (“IBF”), as well as to respondents from adopted, divorced, single-parent, and step-family environments. These categories were treated as mutually exclusive, with respondents coded into the LM or GF categories regardless of their logical status in other categories as well. This is despite the article’s acknowledgment that they are empirically not mutually exclusive. People categorized as LM or GF may quite plausibly have been in any one of the other categories as well, and indeed most of them probably were. So treating them as mutually exclusive amounts to deciding *a priori* that a parent's having had a same-sex relationship is causally more important than the other measures that capture

actual family structure. No theoretical or empirical defense is offered for this decision, although in interviews after its publication Regnerus has attributed it to the need for a sufficient sample size of LM/GF family respondents.¹

The self-report methodology requires, then, that one (or both) of the respondent's parents had a romantic relationship with someone of the same sex, and that the respondent knew about and later remembered that relationship. Recall bias is therefore a concern (Amato 1991), as respondents with an unfavorable view of their childhood may be more likely to recall parents' extramarital affairs, and in particular same-sex affairs. This is particularly true during the time period considered: the respondents were children between 1981 and 2012, so during much of this period same-sex relationships remained taboo for much of mainstream society. There is also the potential for selection bias, as Knowledge Networks screened members of their existing panel using the same-sex-relationship question above. Since members of the panel are paid for participation, they may have overreported such relationships in the hope of being selected for the panel. Both of these sources of bias are exacerbated when identifying relatively rare behaviors,

¹ In a “Q&A” with himself, Regnerus (2012d) wrote: “One of the key methodological criticisms circulating is that—basically—in a population-based sample, I haven’t really evaluated how the adult children of stably-intact coupled self-identified lesbians have fared. Right? Right. And I’m telling you that it cannot be feasibly accomplished. ... My team of consultants elected to go with the screener questions (including the one about same-sex relationships) that we did, anticipating—accurately, too—that there would be no way of generating ample sample size if we narrowed the criteria (for who counts as a lesbian parent) to the sort that critics are calling for.”

such as those considered in the article, since small proportions are particularly vulnerable to such errors.

The bigger problem with this definition, though, is neither recall bias nor selection bias. The single biggest weakness of the article is the conceptual definition of same-sex parents, as a very large proportion of these parents was certainly not raising children in a same-sex household. Indeed, just over half (52%) of respondents who responded that their mothers had had a lesbian relationship had *ever* lived in the same household with both the mother and her lesbian partner, and 60% of these did so for 2 years or less. Thus the conceptual definition is certainly invalid, since the group of people categorized as LM or GF simply does not match the question of theoretical interest: those who were actually raised in same-sex-parent families. Leaving aside the questions of the quality and reliability of the data and analysis, Regnerus provides an answer to a different question from the one motivating the no-differences hypothesis and one which is, therefore, irrelevant to evaluating that hypothesis. One alternative, which Regnerus does not pursue, would be to compare respondents whose parents had homosexual extramarital affairs with those whose parents had heterosexual extramarital affairs as the comparison case varying only the sex of the extramarital partner.

The Regnerus article finds substantially worse outcomes among LM and GF respondents than among those in the other groups. In particular, a lot is made of the relationship between LMs and receipt of public assistance, both in their family of origin and as adults. The implied claim is that growing up in an LM household leads to greater receipt of public assistance, but the causal direction is not investigated. But if women fleeing abusive husbands, for example, sometimes enter romantic relationships with other women, or if lesbian working-class

households have a harder time because of women's lower wage-earning power, these could explain the observed relationship without resort to the implied causal claim. An interesting question is the likelihood of a respondent whose family of origin received public assistance continuing to receive it during adulthood. That likelihood decreases by 41% for IBFs (from 17% to 10%) but by 44% for LMs and by nearly 60% for GFs, suggesting that poverty is a more temporary condition for these respondents than for IBFs.

The “Summary of Differences” section of the article attempts to catalog the number of between-group differences found, out of a total of 279 possible such differences. The article offers a count of how many such differences are found (at $p < .05$) between GFs and everyone else, and LMs and everyone else. However, since no confidence intervals or standard errors are provided, and no correction made for the very large number of dependent variables, some significant number of these differences is likely due to chance alone (Benjamini & Hochberg 1995).

Additionally, there are several other errors in the data preparation and presentation that call into question the main findings. For example, there are at least three errors in the tables. The standard deviations for “Father had same-sex relationship (GF)” and “Adopted age 0–2” from Table 1 are both listed as .75, but they are likely .075. The stated numbers are simply not plausible standard deviations for binary variables with a mean of less than .01. Additionally, for the “Adopted by strangers” category in Table 2, the mean of “Identifies as entirely heterosexual” is .83, which is incompatible with the reported mean of “Is in a same-sex romantic relationship”: .23. This might be an error in the table, or might be related to the difficulties associated with getting reliable estimates from very small sample sizes.

It is also not clear how Regnerus handles missing data. He writes, “The regression models exhibited few ($N < 15$) missing values on the covariates.” This statement is at odds with the cross-tabs provided on the author’s website. For income growing up (Q35), 24 people refused to answer the question and 615 didn't know. While it is likely that the “don't knows” were included as a categorical variable in the models, that data is still missing, and 24 is greater than 15. There is a similar story with maternal education where the number of missing values is greater than 15, although it is trickier to figure out based on the crosstabs because the measure might be based on two questions (maternal education Q23_1 and female parent education Q24_1). These cases might also be missing on the outcome measure so are not included in the regression analysis. However, if they are missing both the dependent and independent variables, they should not be included in the overall sample size.

Finally, it is unclear how Regnerus handled extreme values. The cross-tabs suggest that some number of respondents were likely just having fun filling out the survey. For almost every open-ended question, some people took the opportunity to give implausible answers. For example, two respondents had mothers more than eighty years old at the time of birth (Q3); four respondents have had eight or more spouses (Q12); nine respondents were first arrested prior to the age of four (Q87); ten respondents had been pregnant a dozen or more times (Q132); and fifteen respondents had had sex more than thirty times in the last two weeks (Q135). While some of these answers are possible, the volume of them is incredibly unlikely in a sample this size. It was not clear what data cleaning process was used for such cases. Standard analytic procedures would suggest that they be either (1) grouped with the less extreme cases; (2) excluded from the analysis; or (3) analyzed to see if there was a systematic pattern of particular people providing

specious answers. Regnerus does not state in the text how these issues were handled, so we assume that they were included in the analysis without any adjustments. This is bad practice in general and particularly likely to bias the estimates here as the dependent variable of interest is so rare. (We contacted Regnerus for clarification on these specific issues, but he did not respond.)

Alternative Mechanisms and Hypotheses

Our discussion above provides some alternative hypotheses with respect to Regnerus's findings of disadvantage. These include recall and selection bias as well as confounding effects of gender and heterosexism on social outcomes. Each of these constitutes a plausible mechanism by which categorization in LM or GF groups might be associated with adult disadvantage. The mechanism Regnerus proposes is “diminished kin altruism:” the principle that parents will show less care toward their children when those children are not biologically kin. While this is a plausible mechanism, it is not a widely accepted cause for family outcomes, and is not mentioned even in the reference Regnerus provides for the concept (Miller et al. 2000). It is also rendered less plausible by the fact that children of adoptive parents showed *less* negative outcomes than those from intact, biological families – which is consistent with other research showing higher-than-average parental investment in adopted children in the United States (Hamilton, Cheng & Powell 2007). Thus the only other family structure characterized by “diminished kin altruism” did not follow the same pattern. In short, there is little reason to suspect that “diminished kin altruism” drives Regnerus’s principal findings..

Given the extant literature, the most important alternative hypothesis is that the observed effects on LM and GF respondents are due, at least in part, to increased family instability during

childhood. Particularly during the period when many of the respondents were children, same-sex relationships may well have resulted in greater family instability due to cultural and legal constraints. Indeed, Potter (2012) shows that lower academic achievement by children of same-sex parents (measured more directly than in Regnerus) was explained thoroughly by the greater number of family transitions experienced by such children.

Even though NFSS, as described by Regnerus, has the capacity to document and analyze family transitions and, therefore, to evaluate the potential mediating effect of family instability in the relationships studied, that analysis was not included, so it is impossible to evaluate whether categorization in the LM/GF groups has an independent effect on the outcomes studied, or alternatively, whether these categories represent higher risk for family transitions in this historical moment. That could, in turn, predict differential outcomes. This is an essential point, both because mediation through family instability would offer a more plausible mechanism for any differential outcomes and because the individual and policy interventions appropriate to preventing disadvantage would be dramatically different from those implied by a direct effect of same-sex relationships on children's well-being.

Conclusion

Regnerus (2012a) spurred large amounts of political, academic, and scientific controversy following its publication. The article claims that “sexual orientation or parent sexual behavior...may affect the reality of family experiences among a significant number,” and that therefore “the empirical claim that no notable differences exist” for children in “lesbian and gay families...must go.” The article claims sufficient evidence, that is, to confirm hypothesis H2. In fact, due to major deficiencies of the data, significant untested assumptions, poor data analysis,

unmeasurable recall and selection bias, and lack of consideration of appropriate alternative hypotheses, there is insufficient evidence to confirm this hypothesis. Rengerus (2012a) fails to demonstrate that children from same-sex families display disadvantages. Thus the state of the science remains as it was prior to publication of Rengerus (2012a): there is no systematic evidence demonstrating that children from same-sex households suffer disadvantages relative to appropriate comparison groups from opposite-sex households.

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