



Measurement, methods, and divergent patterns: Reassessing the effects of same-sex parents [☆]



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ABSTRACT

Scholars have noted that survey analysis of small subsamples—for example, same-sex parent families—is sensitive to researchers' analytical decisions, and even small differences in coding can profoundly shape empirical patterns. As an illustration, we reassess the findings of a recent article by Regnerus regarding the implications of being raised by gay and lesbian parents. Taking a close look at the New Family Structures Study (NFSS), we demonstrate the potential for misclassifying a non-negligible number of respondents as having been raised by parents who had a same-sex romantic relationship. We assess the implications of these possible misclassifications, along with other methodological considerations, by reanalyzing the NFSS in seven steps. The reanalysis offers evidence that the empirical patterns showcased in the original Regnerus article are fragile—so fragile that they appear largely a function of these possible misclassifications and other methodological choices. Our replication and reanalysis of Regnerus's study offer a cautionary illustration of the importance of double checking and critically assessing the implications of measurement and other methodological decisions in our and others' research.

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1. Introduction

Research communities in the social sciences have long been aware that methodological decisions can potentially affect the inferences of survey research (Firebaugh, 2008). This threat to the validity of research inferences is particularly challenging for studies that focus on a very small group of interest, such as some racial minority groups, atypical families, and same-sex couples (Cheng and Powell, 2005, 2011). In such research, even a tiny percentage of measurement errors for the small subsamples could powerfully distort patterns from the surveys, and other methodological choices can similarly affect empirical results. When research findings from these analyses are used as policy guidelines, the threat goes even beyond scientific communities. It therefore is incumbent for scholars to critically assess the implications of these decisions in their own work as well as that of others.

In this paper, we use a recent article by Regnerus (2012a) in *Social Science Research* as an example to illustrate these points. In "How different are the adult children of parents who have same-sex relationships? Findings from the New Family Structures Study," Regnerus (2012a) introduces the New Family Structures Study (NFSS) and, with these data,

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compares the outcome profiles of 236 adult children whose parents reportedly had a same-sex romantic relationship with the profiles of those who grew up in other family types, including “intact biological families,” stepfamilies, and single-parent families. Examining 40 social, emotional, and relational outcomes, Regnerus concludes that adult children of same-sex parents generally fare less well than those from intact two-biological-parent families.

It is an understatement to describe this article as eliciting a great deal of interest. This is one of the most visible and controversial articles to appear in this journal—or, more broadly, social science journals—in recent history. It has been vigorously defended and critiqued in this journal (Amato, 2012; Barrett, 2012; Eggebeen, 2012; Gates et al., 2012; Schumm, 2012), other academic journals and forums (Perrin et al., 2013), the courts (Brief of Amicus Curiae American Sociological Association, 2013; Brief of Amici Curiae Social Science Professors, 2014), and the public sphere (Davidson, 2012; Gallagher, 2012; Luscombe, 2012). Defenders often point to what they see as the high quality of the data, which, they argue, “deserve[s] to be considered the gold standard in this field” (Sprigg, 2012). Osborne (2012), identified as “key collaborator” on the NFSS website, praises the study for being “one of the most comprehensive and rigorous studies that has been conducted in this field to date” (p. 779). In contrast, critics call into question, among other things, the study design, the quality of the data, review process, and even the motives of the author and funders of this project (Cohen, 2013; Perrin et al., 2013; Sherkat, 2012). Both sides of the debate often characterize the other side as non-scientific and overly political.

We take a different approach in evaluating the NFSS and the findings reported by Regnerus. We agree with Smith (2012) who, in challenging critics of Regnerus, contends that “science already has its own ways to deal with controversial research results. Studies should be replicated. Data sets should be made public and reanalyzed. . . . Eventually the truth comes out. By those means, Regnerus might be shown to have been wrong or perhaps be vindicated. That is how science is supposed to work.” To his credit, Regnerus has made his data publicly available and, in fact, notes that a goal of his original article is to “serve[s] as a call” (2012a, p. 766) to analyze NFSS. We have accepted this invitation to reanalyze these data. In this article, we report on the results of this reanalysis.²

The fact that Regnerus’s findings are so markedly different from those reported by previous studies suggests that scholars and policymakers should more carefully scrutinize his analysis before reflexively accepting—or rejecting—its conclusion. To explain his different findings, Regnerus suggests that, “[t]he answer lies in part with the small or nonprobability samples so often relied upon in nearly all previous studies—they have very likely underestimated the number and magnitude of real differences between the children of lesbian mothers (and to a lesser extent, gay fathers) and those raised in other types of households” (2012a, p. 756).

We are hesitant to accept this explanation without further examination of the data because, as others have noted in their reanalyses of other national surveys (Bearman and Parigi, 2004; Fischer, 2009), findings from empirical analyses often are also affected by other factors, including the conceptualization and operationalization of key concepts and other methodological decisions made by the researcher in the research process (Firebaugh, 2008). These considerations are directly relevant to the comparison of same-sex parent families and other family forms because analyses of small-population groups using large survey data are particularly sensitive to different analytical decisions (Black et al., 2007; Cheng and Powell, 2005, 2011; Gates and Steinberger, 2009; O’Connell et al., 2010). In the case of Regnerus’s study, the NFSS data are new, the measures of family types and respondents with same-sex parents are somewhat novel and potentially problematic, and the analytical decisions made by Regnerus arguably are not entirely consistent with the general practices in the field. In revisiting the Regnerus article and reanalyzing the NFSS, we ask one fundamental question: *To what extent are the patterns reported by Regnerus attributable to the conceptualization and operationalization of family types—in particular, gay/lesbian/bisexual families—and other analytical decisions?*

Our empirical reexamination of Regnerus’s analysis is designed to answer this question. More broadly, it underscores the importance of, in the words of Firebaugh (2008), “build[ing] reality checks into your research” (p. 64)—in particular, “internal reality checks” (p. 65), checks on “dubious values and incomplete data” (p. 65), and checks on “consistency in conceptualization and measurement” (p. 69)—and the serious implications of not attending to these concerns (Bearman and Parigi, 2004; Cheng and Powell, 2011; Fischer, 2009). In addition, it highlights the general challenges that social scientists continue to face in our examination of same-sex parent households and other emerging family forms using nationally representative datasets (Cheng and Powell, 2005).

Below, we first discuss the NFSS and Regnerus’s measures of family types using the data, and then highlight the difficulties in using the NFSS to accurately distinguish between family types, using adoptive households and intact biological families as illustrations. We then discuss the challenges in accurately identifying same-sex families. We follow this discussion with a closer look at the NFSS survey and demonstrate the potential for misclassifying a non-negligible number of respondents as having been raised by parents who had a same-sex romantic relationship. Finally, we assess the cumulative implications of these possible classification errors and other methodological considerations from various stages of the research process by reanalyzing the NFSS in seven steps.³

These reanalyses provide a “reality check” regarding the conclusions from the original Regnerus study. The patterns from these reanalyses offer evidence of the fragility of these conclusions—so fragile, in fact, that they are due primarily to the methodological choices made by Regnerus. Or to put it another way, when equally plausible and, in our view, preferred

² In the spirit of full disclosure: one of the authors declined an invitation by Regnerus to participate as a paid consultant on the NFSS sampling strategy and measurement.

³ For an insightful, complementary reanalysis that focuses primarily on same-sex couple households, see Rosenfeld (2012).

methodological decisions are used, a different conclusion emerges: adult children who lived with same-sex parents show comparable outcome profiles to those from other family types, including intact biological families. That this revised conclusion is consistent with those reported in most previous studies and inconsistent with Regnerus's findings illustrates how the accumulation of research decisions throughout the research endeavor—and, in particular, measurement decisions that overlook inconsistent information within the data—may lead to questionable conclusions, even with a population-based large sample.

2. The New Family Structures Study and measures of family types

As described in its website, the New Family Structure Study “is a comparative project which seeks to understand how young adults (~ages 18–39) raised by (our emphasis) same-sex parents fare on a variety of social, emotional, and relational outcomes when compared with young adults raised in homes with their married biological parents, those raised with a step-parent, and those raised in homes with two adoptive parents” (Regnerus, 2012b). The data collection was in two stages. In the first stage, a screener survey was used to identify family types that respondents were raised in, while in the second stage, a detailed survey was used to gauge, among other items, respondents' experiences in young adulthood. Regnerus differentiates this study from others in four regards: (1) it uses a national population-based sample instead of snowball or convenience samples; (2) it uses a larger sample than do most other studies of same-sex families; (3) it focuses on the current experiences and “lives of young adults between the ages of 18 and 39, but not about children or adolescents” (Regnerus, 2012a, p. 755); and (4) it includes a wide array of items intended to gauge “subsequent life outcomes for adult children” (Regnerus, 2012c, p. 1367).

In his first article using the NFSS data, Regnerus (2012a) notes that his study offers “statistical comparisons of them [respondents' adult outcomes] among eight different family structures/experiences of origin” (p. 755), including adoptive families, single-parent families, and stepfamilies. He focuses, though, on the distinction between “intact biological families” (IBF) and “lesbian mother” (LM) and “gay father” (GF) families. In multivariate analyses of 40 outcomes,⁴ he finds significant LM–IBF differences for 24 outcomes and GF–IBF differences for 19 outcomes. Differences in some of these outcomes are merely differences, not necessarily disadvantages: for example, whether the young adult identifies as entirely heterosexual and whether the young adult is in a same-sex romantic relationship. Recognizing that some of these outcomes may be more consequential than others, he emphasizes outcomes “that are obviously suboptimal” (p. 764), pointing to “education, depression, employment status, or marijuana use” (p. 764) as examples. Regnerus indicates that the goal of his article is not to identify the reasons behind the patterns he reports here. That said, he does express concern that families with two same-sex parents still exhibit “a diminished context of kin altruism (like adoption, step-parenting, or nonmarital childbirth), which have [sic] typically proven to be a risk setting, on average, for raising children when compared with married, biological parenting” (p. 765). In later writings, he, along with some fellow social scientists, refers to the “benefit from the unique parenting contributions of both men and women” (Brief of Amici Curiae Social Science Professors, 2014, p. 4).

The data collection efforts for this project are certainly impressive, especially with its large nationally representative sample,^{5,6} multiple outcomes covered, and attempts to identify different family structures. While some portray the NFSS and, in turn, the Regnerus analysis, as a gold standard in family research, we contend that a critical hallmark of any study is its ability to accurately measure its key variables of interest—in this case, the different family types.

In revisiting the NFSS, we were struck by the difficulties in unequivocally categorizing respondents by family type—or to put it another way, the challenges in developing valid measures of family type. For example, Regnerus relies on a screener survey to identify 101 adult respondents who were “adopted by one or two strangers at birth or before age 2.” The restriction to respondents who were adopted at such an early date presumably is to ensure that any documented patterns for this group can be attributed to having been raised in an adoptive family since early childhood. Regnerus also notes the presence of “calendar” data from each respondent about their relationship to people who lived with them in their household (for more than 4 months) from birth to age 18, as well as who has lived with them from age 18—after they have left home—to the present” (2012a, p. 757). Regnerus acknowledges that these data are “only sparingly used” in his analysis, but affirms that “... such rich data enables [sic] researchers to document who else has lived with the respondent for virtually their entire life up to the present” (2012a, p. 757).

These data are rich, and Regnerus deserves credit for collecting these complicated data. When we compare the responses from the calendar data to those from the screener survey, however, we notice that 9 of the 101 respondents report that they

⁴ Controls include respondent's age, gender, race/ethnicity, mother's education, family income while growing up, experience of being bullied in childhood, and state's legislative gay friendliness.

⁵ Others have questioned Regnerus's reliance on internet surveys collected by Knowledge Networks, now GfK (Sherkat, 2012); however, the quality of data collected from internet surveys completed by a nationally representative, probability-based survey web panel is comparable to that of other data collection efforts that also rely on random digit dialing (Chang and Krosnick, 2009). Despite some limitations to internet surveys of this type—or, for that matter, surveys in general—it bears pointing out these surveys have been productively used in sociological scholarship on family and relationships (Doan et al., 2014; Rosenfeld and Thomas, 2012; Seltzer et al., 2012).

⁶ Response rates from Knowledge Networks/GfK are similar to the industry norms. That said, the “65% within survey response rate” reported by Regnerus (2012a, p. 756), although technically accurate, might mislead readers into believing that the overall response rate for NFSS is very high. As reported by others who have analyzed data collected by Knowledge Networks/GfK, the cumulative response rate—which takes into account not only within survey response rate but also recruitment rate and demographic profile completion rate—typically is less than 15%, a rate that still is consistent with those from comparable data collection efforts (Rosenfeld and Thomas, 2012; Seltzer et al., 2012).

actually had only lived with their adoptive parents for a very short period of time: 4 for less than a year, 2 for one year, 1 for two years, and 2 for three or four years. In one case, for example, the respondent reports that she lived with her adoptive father for one year only (when she was 1) while also living with her biological mother from birth until she was 16 years old. In another case, the respondent reports having lived with his adoptive mother for three years and then his biological mother for the next 15 years. Technically speaking, these 9 cases may be consistent with Regnerus's description that the respondents were "adopted by one or two strangers at birth or before age 2"; however, if we are to take the responses seriously (an issue we return to shortly), then the inconsistencies between the screener survey and the calendar data should give us concern over whether these cases are accurately classified as "adoptive families."⁷ These concerns, however, are not limited to adoptive families, but also extend to the descriptions and classifications of stepfamilies, single-parent families, and, perhaps most importantly as we discuss shortly, "lesbian mother" and "gay father" families in the Regnerus analysis.

Even in the absence of inconsistencies in responses, there is the potential to miscode or misclassify family types. For example, Regnerus's operationalization of "intact biological families" is puzzling. Of the 2988 respondents included in Regnerus's analysis, 1195 answer "yes" to the screening question "Did you live together with BOTH your biological mother AND biological father the entire time from when you were born until age 18 (or until you left home to be on your own)?" (original emphases). Yet, Regnerus chooses to include only 919 of these respondents in the category of "intact biological family" (IBF). Excluded from this category are the 116 respondents whose parents were not married at the time of the interview, which he places in the category "divorced later or had joint custody."

To the extent that the NFSS is intended to, among other things, examine the outcomes of "young adults (ages 18–39) who were raised (emphasis ours) in different types of family arrangements" (Regnerus, 2012a, p. 752), the distinction between these two groups—which, in the absence of other information, appear to be virtually identical in structure during the respondents' childhood—cannot be reconciled with the goals of the project.^{8,9} Where these two groups may differ is in the quality or functionality of the marriage—characteristics that may affect the well-being of children. For example, parents who divorced later may have had an unhappy marriage but nevertheless stayed married until their children had left their home. If so, the decision to exclude this group from the category IBF could overstate the positive consequences of being raised in this family type, especially compared to being raised in other family types.

3. Challenges in identifying parents who had a same-sex romantic relationship

The preceding discussion underscores the challenges in identifying children who were raised in adoptive families and intact biological families. Our primary concern, however, is in regards to Regnerus's classification of same-sex parent families. Regnerus (2012a) identifies children raised by same-sex parents on the basis of responses to the question, "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?" If respondents responded affirmatively to this question, they were then asked, "Did you ever live with your mother/father while s/he was in a romantic relationship with another woman/man?"

Even if we are to accept Regnerus's position that these items accurately measure "LM (child of a lesbian mother), and GF (child of a gay father)" (2012a, p. 758)—a position that we challenge below—it is telling that these questions apparently were *not* asked of all respondents in the NFSS. Notably exempt from answering these questions, as indicated in the screener survey and the subsequent survey,¹⁰ were respondents who reported living "together with BOTH your biological mother AND biological father the entire time from when you were born until age 18 (or until you left home to be on your own)." In other words, Regnerus's analysis is based on the assumption that parents in intact biological families never have "romantic relationships with someone of the same sex" while the parents are married—an assumption that is highly difficult to defend.¹¹ To the extent that he equates lesbian and gay families with the parental relationship history (during the respondents' childhood), Regnerus underestimates the number of children from LM and GF households and, in turn, overestimates the number of children of IBF households.

⁷ We take a conservative approach in identifying these questionable cases of "adoptive families." Not included among these 9 cases, for example, is a respondent who claims to have always (from birth until leaving home) lived with her biological mother, adoptive mother, adoptive father, grandmother, grandfather, and foster parents.

⁸ Respondents who fall into these two categories apparently were not asked to complete the calendar data. Instead, the calendar data for these two groups are imputed solely on the responses to the screener survey: that is, as described in the NFSS survey, these respondents are "automatically assign[ed] 'always' to 'biological mother' and 'always' to 'biological father.'" In other words, in contrast to the respondents from other family types, there is no mechanism to check for consistency, or reliability, in responses for these two groups or to identify other adults who may have lived in the household.

⁹ Perhaps the only way to justify this distinction is to point to the problematic nature of the screening question. Although the question asks whether respondent lived "together with BOTH your biological mother AND biological father," it is possible that a small number of respondents might have interpreted this to mean being raised by each parent but in a joint custody arrangement. Surprisingly and unfortunately, the NFSS does not ask when respondents' parents were divorced. In the absence of this potentially confirming or disconfirming information, the most intuitive coding, especially given the phrasing of the question, is to include the 119 respondents in the IBF category.

¹⁰ It is possible that Regnerus was reluctant to ask this question in the screener survey because of concern that doing so would result in a lower response rate from the IBF sample. That said, as in the case of other potentially controversial items, it could have been asked in the subsequent survey or, following protocol regarding potentially controversial items, could have been included in the end.

¹¹ Nor does the screener survey or the latter survey ask whether parents in intact biological families ever had a romantic relationship with an opposite-sex partner other than one's spouse.

After examining the data, however, we see a potentially larger threat to validity at hand: that Regnerus also may have *overestimated* the number of children from LM and GF households and, in turn, misidentified respondents from other family types as coming from LM and GF households. In replying to critics, Regnerus (2012c) defends his reliance on respondents' reports of parental relationships, but acknowledges that these measures do not necessarily correspond with respondents' assessments of parental sexual orientation or parental assessments of their own behavior. What he does not acknowledge, however, is the possibility of inaccuracy even in respondents' reports of parental relationships. Like all studies that rely on surveys, Regnerus's study assumes that respondents interpret survey questions in the same way that the researcher intended them to be interpreted. Yet, there is ample evidence to question this assumption. In our own work, for example, we have documented the challenges that some individuals face and mistakes they make in understanding and interpreting questions regarding racial identification (Cheng and Powell, 2011), and, more directly relevant to the question of same-sex households, words such as heterosexual, bisexual, civil unions, and "two women (two men) living together as a couple" (Powell et al., 2010, 2015). Similarly, Savin-Williams and Joyner (2014) attribute the large number of "dubious" cases of gay, lesbian, and bisexual adolescents in the Add Health data set to confusion over the meaning of "romantic attraction"—as well as to mischievous jokesters who are not truthful or careful in their responses regarding their sexuality.

The potential for misconstrued interpretations—and, therefore, threats to validity—is among the reasons that so many scholars conduct cognitive interviews or pretesting before fielding their surveys. It is unclear, though, whether Regnerus followed this standard, or at least preferred, practice. Even if he did, it would be difficult to detect misinterpretations—or even careless or cavalier responses—made by a very small percentage of respondents. Fortunately, for most studies, these small errors likely have little impact on key patterns coming from surveys. Unfortunately, for studies focusing on a very small group of interest—in this case, people who were raised in same-sex parent households—even a tiny percentage of error could powerfully distort patterns from the surveys.

Researchers using nationally representative datasets to study same-sex parent families have routinely checked for potential coding errors or inconsistent cases in their data. For example, in their analysis of the National Longitudinal Survey of Adolescent Health (Add Health) (Harris and Udry, 2010), Wainright et al. (2004) identified 44 cases of adolescents in same-sex families—a small sample size that Regnerus critiques for minimizing the likelihood of finding significant differences between same-sex and other families. To their credit, though, they also minimized the number of misclassified cases of adolescents in same-sex families by checking for consistency in parental reports of their sex and family relationship. Doing otherwise would have markedly overestimated the number of same-sex parental households: as reported in the Add Health codebooks, a cross-check of parental responses identified 339 "male mother figures" and 45 "female father" figures.

Similarly, programmers of the Early Childhood Longitudinal Study-Kindergarten Cohort (ECLS-K) performed extensive triangulation checks to eliminate the inconsistent coding of same-sex parent families and other family types: for example, families with a "male mother" or a "biological mother over age 80" (Potter, 2012; Tourangeau et al., 2006). Several years ago, we too contemplated using the National Education Longitudinal Study of 1988 (NELS) to study children from gay/lesbian households. But in looking more closely at the parental and adolescent responses of the 69 cases that initially appeared to fit this description, we concluded that the high rate of discrepancies in responses (over 80%) presented an insurmountable problem of reliability and, in turn, of validity that effectively precluded any meaningful examination of same-sex families (Cheng and Powell, 2005).

In the most publicized example of the misidentification of same-sex households, the U.S. Bureau of the Census retracted its 2010 estimates of same-sex couple households. The initial estimates were derived from two questions from the Census: relationship to householder and the sex of each person. The Census Bureau subsequently discovered that the number of same-sex couple households estimated from these two questions could be greatly inflated if a very small fraction of opposite-sex couples checked the wrong box for the sex of one's partner or spouse (O'Connell, 2011). In correcting the errors,¹² the Census Bureau lowered its original estimate of 901,997 same-sex couple households to 646,464—a reduction of 28%. The drop in the number of same-sex married couples was even more dramatic, from 349, 377 to 131,729—a reduction of 62%.¹³

4. Revisiting the Regnerus categorization of LM and GF households

In light of these documented cases of errors that appreciably inflated the number of same-sex couple households, it is untenable to automatically assume that NFSS is immune from challenges to validity. Still, Regnerus does not check for, or apparently even consider the possibility of, inconsistent, uncertain, and unreliable cases in his data—even though some other items in the NFSS offer some limited means to assess this possibility. For example, Regnerus (2012c) acknowledges that, according to the aforementioned calendar data, over half of the respondents never lived with a parent's same-sex partner, but fails to mention that many respondents—approximately one-third—also *never lived with their same-sex parents or lived with them very briefly*. As seen in Table 1, of the 236 respondents classified as being raised by a gay father (GF) or lesbian mother (LM), 24 (15 GF, 9 LM) report they had never lived with the parent from birth to age 18, 34 (18 GF, 16 LM) report they had lived with the parent for a year, and 18 (9 GF, 9 LM) report they had lived with the parent for only two to four years.

¹² See O'Connell (2011) for discussion of the techniques used to correct for these discrepancies.

¹³ The error rate in overestimating same-sex couple households and married same-sex couple households for the 2000 Census is even higher: 40% and 83%, respectively.

Table 1

Numbers of years respondents reported living with a same-sex parent or same-sex parent's partner, NFSS.

	Gay father (GF)		Lesbian mother (LM)	
	N	%	N	%
<i>A. Number of years with a same-sex parent</i>				
Never	15	20.5	9	5.5
1 year	18	24.7	16	9.8
2–4 years	9	12.3	9	5.5
More than 4 years	31	42.5	129	79.1
Total	73	100.0	163	100.0
	Father's boyfriend		Mother's girlfriend	
	N	%	N	%
<i>B. Number of years with same-sex parent's partner</i>				
Never	56	76.7	82	50.3
1 year	12	16.4	29	17.8
2–4 years	3	4.1	32	19.6
More than 4 years	2	2.7	20	12.3
Total	73	100.0	163	100.0

Note—Analyses are restricted to the 236 LM/GF respondents identified in Regnerus (2012a).

While one should not discount the potential influence of non-residential parents and one should be cautious in identifying the exact number of years that a child needs to live with a parent to be considered raised by that parent, it is difficult to reconcile these patterns with Regnerus's assertion that the 236 respondents "were *raised* (emphasis ours) by parents that had a same-sex relationship." (2012a, p. 755).

Upon closer inspection of the calendar data and other responses, we discovered additional inconsistencies that call into question the coding of a sizeable number of the 236 LM and GF respondents. To identify the inconsistencies, each coauthor examined each case independently. A summary of our reanalysis, which displays only those cases in which the coauthors' ratings correspond, is provided in Table 2.

As a standard procedure of data analysis, we begin by first detecting 9 cases with highly unlikely or potentially unreliable and, in turn, invalid responses to other questions in the survey. The most blatant example of highly suspicious responses is the case of a 25 year-old man who reports that his father had a romantic relationship with another man, but also reports that he (the respondent) was 7-feet 8-inches tall, weighed 88 pounds, was married 8 times and had 8 children. Other examples include a respondent who claims to have been arrested at age 1 and another who spent an implausibly short amount of time (less than 10 minutes) to complete the survey.¹⁴ These cases are akin to the aforementioned jokesters in the Add Health data set (Savin-Williams and Joyner, 2014) and also are consistent with ongoing concerns regarding truthfulness and satisficing in internet surveys (Baker et al., 2014).

After identifying these 9 cases, we compare responses in the screener survey with calendar responses in the following survey and locate an additional 53 respondents who report that they lived with their lesbian mother or gay father for a year or less.¹⁵ We then find 20 other respondents whose answers in the calendar data and screener survey appear inconsistent or improbable.¹⁶ Among these are:

1. Four respondents who report that they lived with their biological parent, that parent's opposite-sex partner (i.e., respondent's stepparent), and that parent's same-sex partner in the same year.¹⁷
2. Eight respondents who report that they lived with mother's girlfriend or father's boyfriend while the mother/father was absent in the family. That the biological parent also lived with an opposite-sex (step)parent, never lived with both the respondent and alleged same-sex partner at the same time, and/or was absent in the family suggests a good possibility that the "boyfriend" or "girlfriend" figure is potentially a close, non-romantic friend rather than the parent's partner.
3. Eight respondents who show various degrees of inconsistencies in their responses to calendar data and in other survey questions. For example, one respondent reports having *always* lived alone but also claims to have *always* lived with mother, father, and two grandparents. Another two respondents report that during their childhood, their mother had a same-sex relationship but also report that they *always* lived with mother but *never* lived with the mother while she had a same-sex relationship.

¹⁴ According to the codebook, the median time to complete the main survey was 34 min.

¹⁵ Of these 9 cases, a few also report that they lived in a LM or GF household for a year or less, thereby explaining the seeming discrepancy between the 53 cases reported here and the 58 cases reported in Table 1.

¹⁶ It is possible, of course, that the respondents were inaccurate when completing the calendar data. This possibility, however, would place Regnerus in the paradoxical and indefensible position of defending the accuracy of one section of the NFSS by discounting the accuracy of another section.

¹⁷ There is a possibility that within the same year the parent switched partners; however, this possibility is slight given other problematic responses among these four cases (e.g., regarding income, household size, length of time to complete the survey). Of note, in each case the respondent reports living with the parent's same-sex partner in only one year.

Table 2

Potential unreliable, inconsistent, and uncertain cases, NFSS.

	N	%	Cum N	Cum %
<i>Unreliable and inconsistent cases</i>				
Unreliable responses	9	3.8	9	3.8
Lesbian mother (LM) in household for a year or less	23	9.8	32	13.6
Gay father (GF) in the household for a year or less	30	12.7	62	26.3
Inconsistent responses in screener survey and calendar data	20	8.5	82	34.7
<i>Uncertain cases</i>	6	2.5	88	37.3
Same-sex parent minor roles	15	6.4	103	43.6
Total	103	43.6		

Note—Analyses are restricted to the 236 LM/GF respondents identified in [Regnerus \(2012a\)](#).

Taken together, these 82 cases account for *over one-third* (34.7%) of the 236 respondents categorized by Regnerus as LM or GF. Although this figure might include isolated cases in which inconsistencies merely reflect very complex family situations, this slim possibility does not match up with available information in the NFSS. In fact, this figure may be a conservative estimate of the rate of misclassification of respondents. In 6 cases, for example, the responses are sufficiently problematic or inconsistent that without additional information that was unfortunately not provided in the survey it is difficult to conclude with confidence that they actually had lived in a LM or GF household.¹⁸ An additional 15 of the remaining respondents report having lived in a LM or GF household for only 2–4 years—such a short period of time that it is uncertain whether these respondents are most accurately defined as having been “raised” by parents that had a same-sex relationship. If one includes these cases, the rate increases to 43.6%—a figure that still is consistent with the error rates detected, and then adjusted for, in the Census and other national surveys mentioned earlier in this paper.

5. Implications of methodological decisions and alternative coding

We readily acknowledge that some of our coding decisions are open to different interpretations and can be debated. What cannot be debated, however, is that there are uncertainties and potential errors in Regnerus’s operationalization of LM and GF respondents and that there appears to be little attempt on Regnerus’s part to uncover these possible classification errors or, more importantly, to assess the implications of the inclusion of cases that at minimum are contestable. As scholars have suggested elsewhere, in the analysis of small-subsample groups, even a small number of misidentified cases may alter the conclusions researchers draw from their data. Below we evaluate the extent to which the patterns reported by Regnerus are contingent on his coding and other methodological decisions.

We begin by replicating [Regnerus’s analysis \(2012a\)](#), and then assess the implications of using alternatives to Regnerus’s analytical decisions—alternatives that are common practices in social scientific research. Next, we consider how the potentially miscoded cases affect outcome differences between children raised by same-sex parents and children raised by intact biological families. Finally, we repeat this analysis but restrict the sample to children from same-sex couple households—i.e., households in which the same-sex partner ever lived with the child. These results are summarized in [Table 3](#), in which we identify LM–IBF and GF–IBF differences that are significant at the .05 level at each step of our reanalysis. This table is restricted to the 32 of the 40 outcome variables that were significant at any point in Regnerus’s multivariate analysis or in ours.

5.1. Correcting analytical considerations

We begin with a baseline model that replicates [Regnerus’s \(2012a\)](#) original analysis. As others have noted, Regnerus did not report the regression coefficients or standard errors in the article ([Perrin et al., 2013](#)). Nevertheless, we were able to replicate the reported mean scores of the outcome variables for IBF, LM, and GF. Of the total 40 outcome variables (as noted above, only 32 are shown in [Table 3](#)), LM is significantly different from IBF in 24, and GF is significantly different from IBF in 19 ([Table 3](#), first row). Since our attempts to replicate are successful, this baseline model allows us to assess the implications of alternative analytical and measurement considerations and corrections.

In the second step, we adjust for four coding decisions that either are errors or have a plausible alternative:

1. In two binary outcomes, refusals to respond to the question were coded as “0” when they should be coded as missing.
2. For the question about voting in the last presidential election, respondents who were not old enough to vote at the time of the election were included in the analysis when they should have been coded as missing.
3. Several outcomes measures have identifiable units (e.g., household income in thousand dollars), but were coded as categorical and analyzed using OLS. We recode these variables by their identifiable units.

¹⁸ Among these are respondents who lived with both parents and then lived with their father and stepmother and with their mother and stepfather.

Table 3

Multivariate analyses of the outcome differences between children raised by same-sex parent families and those living with both biological parents until age 18.

		Currently cohabiting	Family received welfare growing up	Currently on public assistance	Currently employed full-time	Currently unemployed	Voted in last presidential election	Thought recently about suicide	Recently or currently in therapy	Identifies as entirely heterosexual	Is in a same-sex romantic relationship	Had affair while married/cohabiting	Has ever had an STI	Ever touched sexually by parent/ adult	Ever forced to have sex against will	Educational attainment	Family- of-origin safety/ security	Family- of-origin negative impact	Closeness to biological mother	CS-D depression index	Attachment scale (depend)	Level of household income	Current relationship quality index	Current relationship is in trouble	Frequency of marijuana use	Frequency of smoking	Frequency of watching TV	Frequency of having been arrested	Frequency pled guilty to non- minor offense	N of female sex partners (among women)	N of female sex partners (among men)	N of male sex partners (among women)	N of male sex partners (among men)	Total
		1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	
1	Replicate <i>Regnerus (2012a)</i>	LM GF	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	24 19
2	Adjust coding for outcome measure	LM GF	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	25 16
3	Expand IBF category	LM GF	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	20 12
4	Add supplementary controls	LM GF	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	12 12
5	Impute missing values in controls	LM GF	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	11 10
6	Add potential coding error controls	LM GF	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	6 3
7	Family type by calendar data	Same- sex couple	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	x x	4

Note—LM signifies lesbian mother; GF signifies gay father; same-sex couple signifies LM or GF households in which the respondent lived with both the parent and the parent's same-sex partner for at least one year. This table does not include 8 of the 40 outcomes in which neither [Regnerus \(2012a\)](#) nor we found significant LM or GF effects. "x" indicates that the difference from IBF (intact biological families) is statistically significant at the .05 level.

4. The coding of number of sexual partners was not consistent with other comparable items in the study, which Regnerus (2012a) coded as count variables. We correct for this inconsistency by recoding the number of sexual partners as count variables and using count models.

Coding decisions such as these underscore the importance of taking the time to double check all analyses and consider alternative coding of dependent variables. In this case, however, these adjustments have minimal effect on the outcomes. As shown in Table 3 (second row), these corrections actually increase the number of significant differences between LM and IBF from 24 to 25. They also, however, decrease the number of significant differences between GF and IBF from 19 to 16.

In the third step, we expand the category IBF to include all respondents who reported living together with both their biological mother and biological father from birth to age 18. As discussed earlier, Regnerus identified 116 respondents who fit this description but coded them as a separate category because their parents were no longer married at the time of survey. This coding decision is incompatible with Regnerus's goal of analyzing the influence of the family arrangements in which youths were raised and further complicates any causal claims regarding outcomes in adulthood. In fact, this coding decision appears to have artificially increased the LM–IBF and GF–IBF differences. As shown in Table 3 (third row), when the IBF category is expanded, the number of significant differences decreases (to 20 between LM and IBF and 12 between GF and IBF).

In the fourth step, we reconsider Regnerus's inclusion of controls in multivariate models. Regnerus commendably added controls (e.g., gender, age, level of mother's education) to make sure that any ostensible effects of family structure on "subsequent life outcomes for adult children" (2012c, p. 1367) are not a function of sociodemographic background of the respondent or respondent's family. We too add these controls but make two adjustments—one involving recoding and one involving the inclusion of other controls. We recode the measure of race/ethnicity so that instead of making a mere binary distinction between white and non-white respondents, we can distinguish among respondents who identify as white, black, Hispanic, multiracial, or other. As Regnerus's collaborator Osborne (2012, p. 780) recommended, "to be consistent with the rest of the analysis," the variable "family received welfare growing up"—which certainly is reflective of family socioeconomic status—should be counted as a control variable. Following this recommendation, we add this variable—along with biological mother's age and biological father's age at birth of child, region, and residential area (metropolitan = 1)—as additional controls.¹⁹ As shown in Table 3 (fourth row), doing so reduces the number of LM–IBF significant differences to 10, but does not change the number of GF–IBF significant differences.²⁰ This change suggests that Regnerus's analysis underestimates the effects of respondents' sociodemographic background, at least in regards to LM–IBF differences.

In the fifth step, we use multiple imputation techniques for missing values in control variables. There are different approaches to handling missing cases, but Regnerus's decision to delete missing cases contradicts his goal of maximizing the sample sizes of respondents who report being raised by same-sex parents.²¹ It also is at odds with the assessment of some applied statisticians that multiple imputation offers a more efficient use of existing data, produces more unbiased estimates in multivariate analyses, and thus is a preferred solution to the missing data problem (Accock, 2005; Allison, 2002; von Hippel, 2007). When the missing data are completely at random (MCAR) or at random (MAR), the use of multiple imputation increases the likelihood of significant patterns. This applies to the LM–IBF comparison, as the number of significant differences increases from 10 to 11 (Table 3, fifth row). When the MCAR or MAR conditions are not satisfied, however, listwise deletion could lead to biased estimates. In Table 3, we see that the use of multiple imputation actually further reduces the number of significant GF–IBF differences from 12 to 10, which suggests that the GF–IBF differences in the deleted cases are smaller than the differences in the other cases. Compared to Regnerus's original findings, the difference that these simple methodological modifications from these five steps—corrections that certainly are not atypical practices in the discipline—make is remarkable.

5.2. Controlling for misclassified and uncertain cases

As noted in our earlier discussion (and presented in Tables 1 and 2), we have strong reason to question Regnerus's classification of over one-third of the 236 respondents identified as living with LM or GF parents. Eighty-two (i.e., 34.7%) provided responses that appear unreliable, indicated that they had lived with their LM or GF for a very short period of time (i.e., one year or less), or offered other information that seriously undermines Regnerus's classification scheme. We also have some doubts about the classification of another 21 respondents (8.9% of the 236 cases) either because of insufficient information in the data set or because they reported living with their LM or GF parents for only 2–4 years. As a corrective, in the sixth step we keep Regnerus's measures of the 163 LM and 73 GF respondents in the model, but we also add two

¹⁹ Osborne (2012) also suggests that two other outcomes—whether one was "ever touched sexually by a parent/adult" or "ever forced to have sex against will"—could be considered candidates for additional control variables. Although the models reported in Table 3 do not include these controls, following her suggestion would further reduce the number of significant differences across family structure.

²⁰ Although Regnerus's use of "family received welfare while growing up" as a dependent variable is contrary to his stated purpose is to explore "the lives of young-adult children of gay lesbian parents" and "their experiences and accomplishments as adults" (2012a, p. 755), we retain this childhood experience as a dependent variable in Table 3 and include all other control variables in the multivariate models predicting this item.

²¹ In Table 1 of his original study, Regnerus (2012a) reports 27 missing cases in experience being bullied as a youth, 8% of missing cases in mother's education, and 22% of missing data in family income. The missing cases in mother's education and family income are kept in the analyses using dummy variables. This approach tends to result in biased estimates in multivariate analyses. To replicate the results in Regnerus (2012a), however, we also use a series of dummy variables (including dummy variables for missing cases) for mother's education and family income. We also found one respondent with missing value in gender, which is not reported in Regnerus (2012a).

dichotomous variables for the 82 and 21 cases to control for the potential confounding effects of these misclassified or uncertain cases, respectively.

As shown in Table 3 (row 6), the number of significant LM–IBF differences is reduced to only 6 of the 40 outcome measures, while the number of significant GF–IBF differences is cut to only 3.^{22,23} Additional sensitivity analyses suggest that, in the case of LM respondents, the significant effects for 3 of the 6 outcomes—i.e., family security, frequency of being arrested, and frequency of pleading guilty—are so fragile that they disappear simply by deleting 1 or 2 cases from the analysis.²⁴ With 163 respondents in the LM category, this is not an issue of statistical power.

These results suggest that 3 of the 6 significant coefficients are highly sensitive to 1 or 2 influential cases. The only three outcomes in which a significant LM–IBF difference remains are: (1) family received welfare assistance growing up, (2) self-identification as entirely heterosexual, and (3) had affair while married/cohabitating (#2, #9, and #11 in Table 3). Of these, receiving welfare assistance in childhood is an outcome that, as we noted earlier and others have articulated elsewhere (Osborne, 2012), more appropriately should be considered a control variable and certainly is not an indicator of the respondents' experiences as adult. Similarly, whether a respondent self-identifies as homosexual or heterosexual should carry no advantageous or disadvantageous implications as an outcome measure (i.e., neither outcome should be seen as, in Regnerus's term, "suboptimal"); moreover, this pattern already has been confirmed in other studies (Stacey and Biblarz, 2001). If these two variables are excluded from the list, only 1 coefficient for LM respondents is statistically significant and could conceivably be seen as a possible disadvantage to adult children from LM households.²⁵

5.3. Assessing the consequences of living in a two-parent LF or GM household

The above analyses focus on the experiences of respondents who report living in a household in which at least one parent had a same-sex romantic relationship. Of the 236 respondents identified by Regnerus (2012a) as living in a LM or GF household, we identify only 51 that can plausibly be coded as being raised for *at least a year in a same-sex couple household*.^{26,27} The other respondents are better characterized as living in other family types. In Table 3, we further examine whether and how the outcome profiles of the 51 adult children respondents from same-sex-two-parent households differ from the profiles of those from IBF households. Here we find only four significant differences, although the differences either are not indicative of any LM/GF disadvantage (i.e., sexual self-identification and having a same-sex romantic relationship) or do not gauge adult experiences (i.e., receiving public assistance in childhood and sense of safety and security while growing up). These patterns also are highly fragile and based in part on a couple of influential cases or outliers. Admittedly, even with a large overall sample, a subsample of 51 cases still limits the statistical power of the analysis. Still, the results are either inconclusive or suggestive that adult children raised by same-sex two-parent families show a comparable adult profile to their peers raised by two-biological-parent families.

6. Conclusion

The standard advice in survey research textbooks—and presumably in most courses on research methods—is that researchers should double-check their concepts, variables, and statistical analyses, and be initially skeptical of the results, even if they correspond with the researchers' expectations. Confidence increases if the patterns are sufficiently robust that they hold up with the use of different coding and control variables and additional analysis of potential outliers and influential cases. These "reality checks"—as recommended by Firebaugh (2008)—can both build up trust in the substantive conclusions and increase the credibility of our research community as a whole.

In this paper, we document the empirical implications of not following this recommendation by using Regnerus's recent article on adult children of same-sex parents as a case in point. Our reanalysis of the NFSS and the Regnerus study

²² Recognizing that there can be disagreement over which cases should be classified as misclassified or uncertain, we also considered alternative classifications that were more expansive or more restrictive. Models using these alternative classifications also resulted in a notable decrease in the number of significant LM–IBF and GF–IBF differences.

²³ Importantly, the controls for misclassified or uncertain cases are significant for 13 outcomes. For example, respondents in the misclassified category are significantly more likely to report they thought recently about suicide and were not close to their mother. They also indicated a higher frequency of drinking to get drunk. In addition, and perhaps even more importantly, they also rated higher on the CES-D depression scale, family-of-origin negative impact scale, and lower on the current relationship quality index. That this group significantly differs from others in the LM and GF sample, as well as others from the overall sample, offers further support for our contention that respondents in this group were misclassified by Regnerus.

²⁴ Residual analysis showed significant proportions of cases as potential outliers (i.e., standardized residuals greater than 2.5 standard deviations) in five of the six outcomes (approximately 3–4%). These large numbers of potential outliers may signal the abnormality of the data, the failure of the statistical models to capture the important characteristics of the data, or both. Because the patterns of outliers are likely to change with different model specifications, our sensitivity analyses are not restricted to outliers.

²⁵ As seen in Table 3, there are statistically significant GF–IBF differences for only three outcomes: whether the family received welfare at some point in the respondent's childhood, the number of male sexual partners among female respondents, and the number of female sexual partners among male respondents. As noted earlier, the first—whether the family received welfare at some point in the respondent's childhood—is more appropriately considered a control variable than as an adult outcome. The patterns regarding the other two outcomes suggest that respondents from GF households are more sexually active (i.e., more opposite-sex partners) than those from IBF households. The extent to which these differences imply a disadvantage or advantage is unclear.

²⁶ In the supplementary analyses, we also differentiated between respondents from LM-couple and GF-couple families. Given the small number of cases, however, the analysis in Table 3 is based on 51 respondents from either type of household.

²⁷ For a detailed analysis of NFSS that focuses on same-sex couple parents, see Rosenfeld (2012).

demonstrates how the accumulation of contestable research decisions—from the initial conceptualization and measurement in the questionnaires to inattention to inconsistencies in survey responses to coding, modeling and treatment of missing cases—can result in a notably ambitious study that still yields disputable patterns. The methodological problems we describe not merely are those of a given research question or one particularly flawed article, but pose a risk more generally to inference from social surveys.

Our primary concern regarding the NFSS in this paper, however, is in the measurement—or what we believe to be the mismeasurement—of same-sex families. Although the number of households headed by same-sex parents have rapidly increased over the past few decades, their proportion in the population remains very small (O'Connell, 2011; Rosenfeld, 2010; Stacey and Biblarz, 2001). Scholars have noted that the analysis of same-sex parent families is sensitive to researchers' analytical decisions, and even small coding errors can seriously compromise empirical conclusions from the research (Cheng and Powell, 2005; Gates and Steinberger, 2009). Our replication and reanalysis of Regnerus's study offer a cautionary illustration of this point.

Regnerus's analysis of the NFSS generated strong reactions—some laudatory, some scathing—from various stakeholders in debates regarding family structure and same-sex marriage, despite Regnerus's assertion in the article that “the study is intended to neither undermine nor affirm any legal rights” regarding same-sex marriage (2012a, p. 766). What the analysis did not generate, however, was much empirical analysis. In fact, in a subsequent amicus brief advocating for the “government to continue to recognize marriage as a man-woman union” (Brief of Amici Curiae Social Science Professors, 2014, p. 21), Regnerus and his coauthors note that “despite the attention and scrutiny, the study remains in print and subsequent analyses of the (now publicly-accessible) data have revealed no analytic errors” (Brief of Amici Curiae Social Science Professors, 2014, p. 19).

Our study is an exception. Taking seriously both Smith's recommendation to reanalyze the NFSS and Firebaugh's rule to “build reality checks” in social science research, we revisit Regnerus's analysis, identify serious problems in his decisions regarding measurement and models, and offer evidence that the empirical patterns showcased in his article are largely a function of these decisions. In reanalyzing the data, we find:

1. A non-negligible number of respondents were miscounted as having been raised in LM or GF households. The sources of these potential errors—which we estimate to exceed one-third of Regnerus's subsample of LM and GF—were the inclusion of individuals whose highly implausible responses to other questions call all of their responses into doubt, individuals who reported living in these households for a very short period of time, and individuals whose responses in the calendar data were incompatible with the original categorization of being raised in a LM or GF household.
2. A number of other methodological and modeling decisions made by Regnerus—decisions that have plausible alternatives that at minimum should be checked to assess the robustness of the patterns—appear to artificially inflate the differences between LM/GF and IBF households.
3. Once corrections to these potential coding errors and alternatives to these methodological choices are made, the putative disadvantage in the outcome profile of respondents from same-sex parent families (both single-parent LM and GF households and two-parent LM and GF households) decreases dramatically—with some of the remaining differences not “sub-optimal” (e.g., whether or not the respondent identifies as entirely heterosexual and the number of other-sex partners) or a function of one or two influential cases.

We do not claim that the coding we followed or other methodological choices we made are the only reasonable ones, but we do contend that for a pattern to be believable—especially those that are antithetical with the patterns found in nearly every other study on the same topic—it should hold up to empirical scrutiny and should withstand the use of different coding and alternative specifications.²⁸ Regnerus's analysis does not meet this core requirement. In turn, it does not provide sufficiently credible counterevidence to the longstanding body of scholarship that confirms minimal differences in the consequences of living with same-sex or opposite-sex parents.

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²⁸ We similarly believe that scholars must maintain equally high standards when assessing studies that reach different conclusions than Regnerus's. Reflecting this belief, we have been in the position of (successfully) recommending rejection of such studies that have relied on other data sets for the same concerns that we have outlined in this paper.

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