



Summary of Douglas W. Allen, Catherine R. Pakaluk, and Joseph Price's

"Nontraditional Families and Childhood Progress through School: A Comment on Rosenfeld"

Demography 50 (June 2013): 955–961,
<http://dx.doi.org/10.1007/s13524-012-0169-x>

and

"Normal Progress through School: Further Results"

white paper (June 2, 2014)

The authors begin by noting the great challenge it is to study same-sex-parented families and child outcomes. At the time of the article's publication, there was only one applicable nationally representative data set available in the United States: the 2000 Census 5-Percent Public Use Microdata Sample.¹ Dr. Michael Rosenfeld used this data set to study children's progress through school and concluded that there were no differences among the children: "Children of same-sex couples cannot be distinguished with statistical certainty from children of heterosexual married couples."²

After a careful examination of his study, the authors argue that Rosenfeld made a series of methodological decisions that led to his conclusion of "no differences."³ First, he omitted from study two major groups of children: those who had experienced a residential move in the previous five years, and those not biologically related to the *head* of the household. By omitting these groups of children, he significantly reduced his sample size, creating a situation in which the outcomes of the children of same-sex parents appeared indistinguishable from those of any other children. Second, he failed to reduce the measurement error in his child-outcome measure, which resulted in another bias toward statistical insignificance.

Progress through school is an important signal of child welfare, because children who are held back in school are at much higher risk of dropping out of high school, which is correlated with lower financial earnings, higher unemployment rates, lower self-esteem, and higher mortality rates.⁴ Moreover, studies have found that effective parenting plays an important role in school performance.⁵ Therefore, it matters for children whether different parenting structures are correlated with normal or suboptimal progress through school.⁶

In the first of these two papers, the authors explain why Rosenfeld's decisions to reduce the sample led to a finding of statistical insignificance. They then replicate his procedures while correcting his sample restrictions and discover statistically significant differences between children of same-sex parents and children raised by a married mother and father.

In the second paper, they take their research a step further and distinguish between the gender distributions of the same-sex households (i.e., boys and girls in lesbian- versus gay-parented homes). They find that not only are there significant differences between the children of different family structures, but there are differences in child outcomes among the same-sex families themselves. Finally, they show that simply eliminating the measurement error in the “normal progress” outcome measure also leads to statistically significant differences across the different households.

Critique of Rosenfeld

The initial Allen, Pakaluk, and Price critique of Rosenfeld has two parts. The first is to show that the means Rosenfeld chose (dropping groups of children from the sample) to meet his intended ends undermined his intention. The second challenges Rosenfeld's intention of studying only children that meet the criteria of having family stability and biological relatedness to a parent.

Rosenfeld's intent behind dropping the first group of children, the children who had moved within the previous five years, was to study only children from relatively stable families. Rosenfeld reasoned that children with instability at home would have causes that affect their performance at school and, since same-sex households are likely more unstable, it would be unfair to compare these children to children from more stable, opposite-sex homes.

However, Allen, Pakaluk, and Price argue that dropping these children from the sample was an inadequate method to select for family stability, because a change in *residence* does not necessarily betoken a change in *family structure*. Families often move for reasons unrelated to a change in family structure (such as moving for a new job, for school, or to be closer to extended family). Indeed, when Allen et al. looked at the data, they discovered that 47.6 percent of the children dropped from the sample were from families headed by a married mother and father, the family form that experiences higher levels of stability. This is to say that Rosenfeld greatly reduced his sample size (which meant a loss in precision and a greater chance of finding no statistical differences when there might truly be some) while likely excluding a great many stable families (defeating his goal of comparing children from stable families). Allen et al. argue that it would have been more reasonable to include these children in the sample but to *control* for whether or not they had changed residences in the previous five years. This method has the benefit of accounting for family movement while still maintaining a large sample.

The second group of children Rosenfeld omitted were those not biologically related to the household head, because he reasoned it was unfair to compare children who were biologically related to their parents (such as children from a married-mother-and-father family) to children who were not biologically related (such as adopted children or those in foster care). Again, Allen and colleagues note that by dropping these children, Rosenfeld reduced his sample size.

The problem with this second sample restriction, the authors point out, is that in the case of same-sex families, the designation “household head” is arbitrary with respect to the biological connection with the children.⁷ Indeed, when they look at Rosenfeld's data, the authors find that 17.7 percent of the children raised by same-sex couples were dropped from the sample, even though 75.1 percent of these *were* biologically related to the *second* partner, the one not considered the household head.⁸ Although this large group of children had biological relatedness to one of their parents—meeting the criteria of biological relatedness Rosenfeld sought—Rosenfeld dropped these children from study. Allen and colleagues argue that it would have been more prudent to include all of these children in the sample and to *control* for whether the children were biologically related to one or more of their parents.⁹

The Allen, Pakaluk, and Price paper further argues that it is important to include all children. It is not simply that Rosenfeld's methods failed to meet his stated ends; his goal of studying only certain children from relatively more stable and biologically related households was off the mark as well. The researchers point out that it is important to include these children in the sample for the very reason that family instability and loss of a biological parent are likely to be the very channels, or mechanisms, by which the negative effects of new family structures are often caused.¹⁰

To look at the issue of channels by an analogy, imagine that a group of doctors would like to study whether adults in urban areas have better or worse sleep quality than adults in rural areas. The doctors take a very large, random sample of citizens, distinguish them according to urban or rural residence, and look at the responses related to quality of sleep. However, the doctors decide to exclude from the sample any adults who drink coffee after 3:00 p.m. or are exposed to bright light before bedtime, as they feel it's unfair to compare residents with this handicap to those without it. As it happens, this decision causes them to drop from the sample half of the adults from urban areas. Then, the doctors run the numbers and find no statistical differences in quality of sleep between those who live in urban areas and those in rural areas.

We would rightly question whether these doctors have removed from the sample the very channels by which poor sleep is caused. Urban life tends to revolve around densely populated areas where drinking coffee is a frequent occurrence and bright light is the norm at night. We might even say that the researchers are misrepresenting the very experience of urban living by removing these adults from the sample.

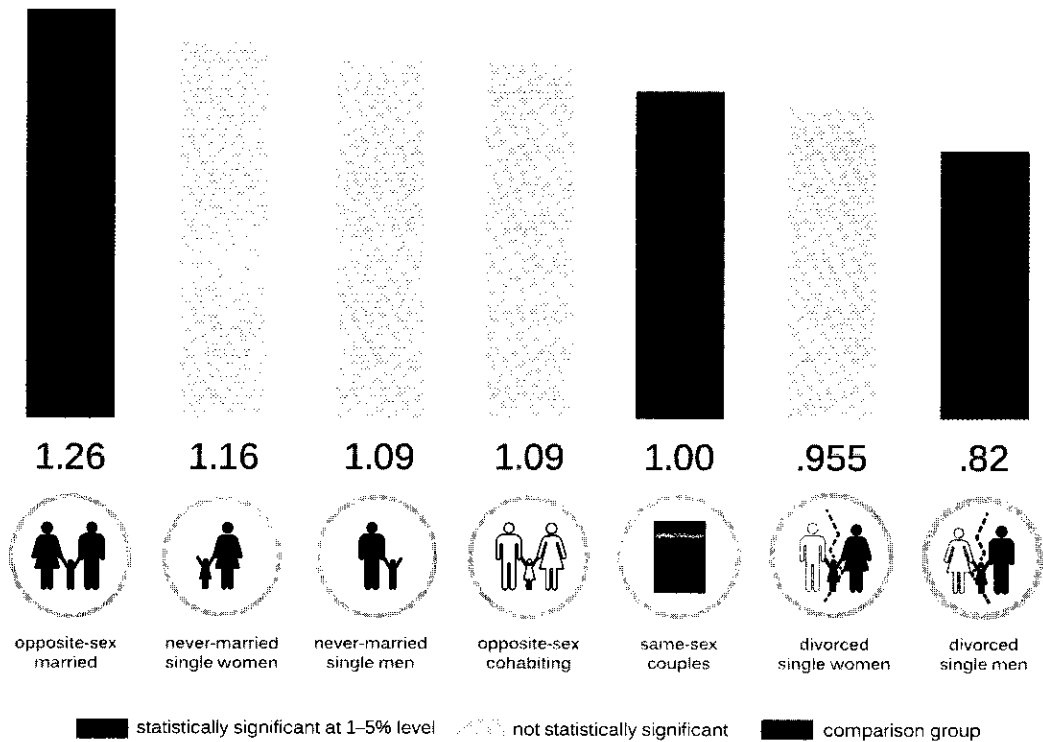
Likewise, Allen, Pakaluk, and Price point out that scholars have identified channels by which children's outcomes are influenced—two of these channels being family instability and the loss of one or more biological parents.¹¹ Same-sex families are—by definition—families where a child has experienced a separation from one or both biological parents. Moreover, same-sex couples have demonstrated greater levels of instability and shorter duration of cohabitation than have opposite-sex couples.

So when Rosenfeld decides to drop from the sample the children who are not biologically related to a household head or who have experienced a residential move in the previous five years, he takes measures to remove the children who experience the channels by which negative child outcomes often arise. One might say that he misrepresents the very experience of same-sex parenting for children. Allen, Pakaluk, and Price therefore challenge Rosenfeld and other scholars to include these children in the sample to gain a more complete understanding of what children from new family structures experience.

Doing It Right: Including All the Children

Allen and colleagues restore the omitted groups of children one group at a time, first by including the children who had changed residences in the previous five years. By returning these children, the sample size increases by more than 80 percent, and the researchers find that the children of married heterosexual parents are 26.1 percent more likely to make normal progress through school than are the children of same-sex parents, a statistically significant difference.¹²

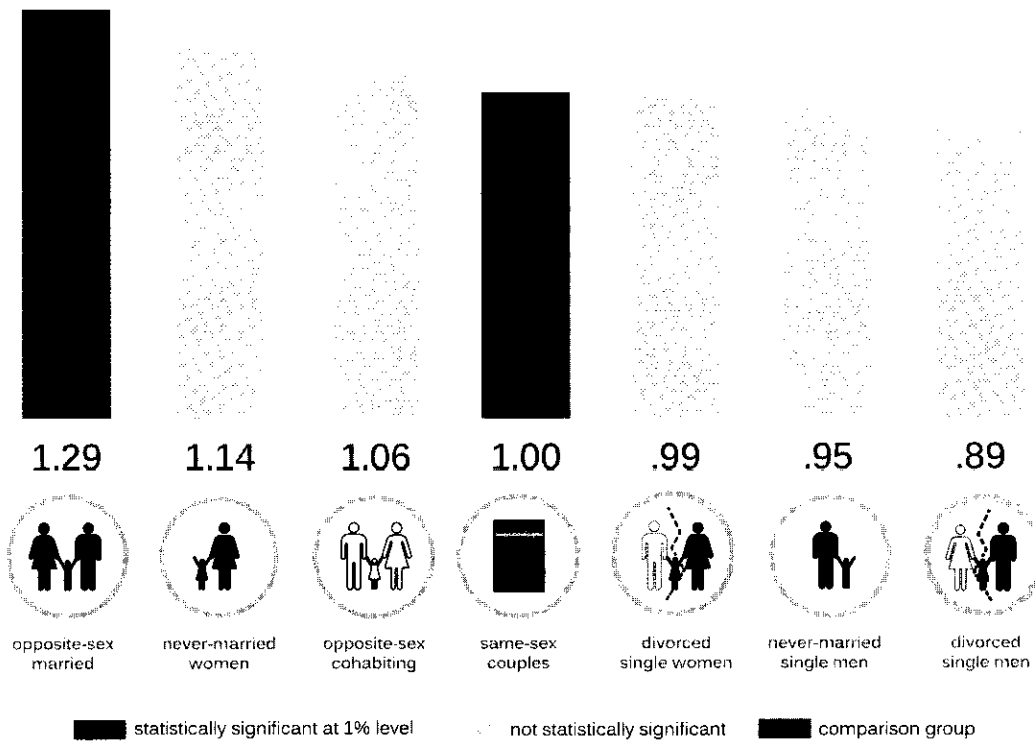
Odds Ratios of Children's Normal Progress through School, Including Children Who Moved in Previous Five Years (with Controls)



Source: Allen, Pakaluk, and Price, "Nontraditional Families," table 3, column 2.

Next, the authors return the children who were not biologically related to the household head to the sample and drop the first omitted group of children. In this scenario, the sample size increases a bit, and the researchers find that the children of heterosexual married families have 29-percent-better odds of making normal progress through school than do children from same-sex homes, again a statistically significant difference.

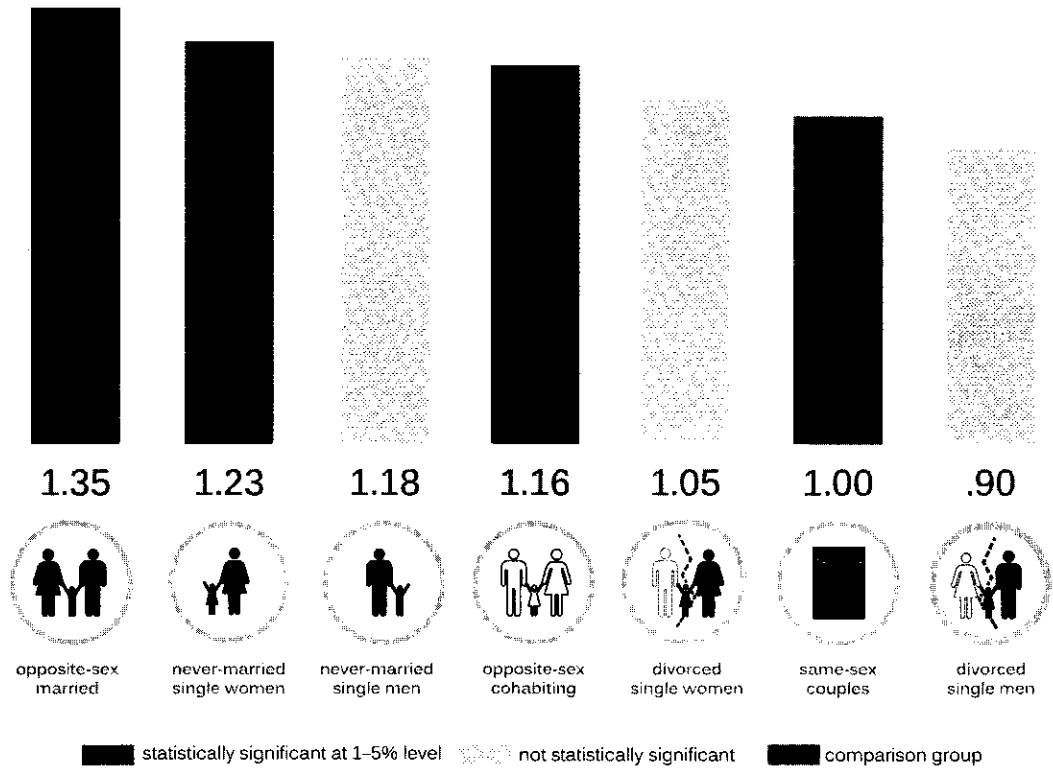
Odds Ratios of Children's Normal Progress through School, Including Children Not Biologically Related to Head of Household (with Controls)



Source: Allen, Pakaluk, and Price, "Nontraditional Families," table 3, column 3.

Finally, the researchers return both groups of children to the sample, restoring the original sample of 1.6 million children, and add controls. In this scenario, shown in the graph on the following page, the children of heterosexual married couples are 35 percent more likely to make normal progress through school than are children in same-sex families, again a statistically significant difference.

Odds Ratios of Children's Normal Progress through School, Unrestricted Model Including All Children (with Controls)



Source: Allen, Pakaluk, and Price, "Nontraditional Families," table 3, column 4.

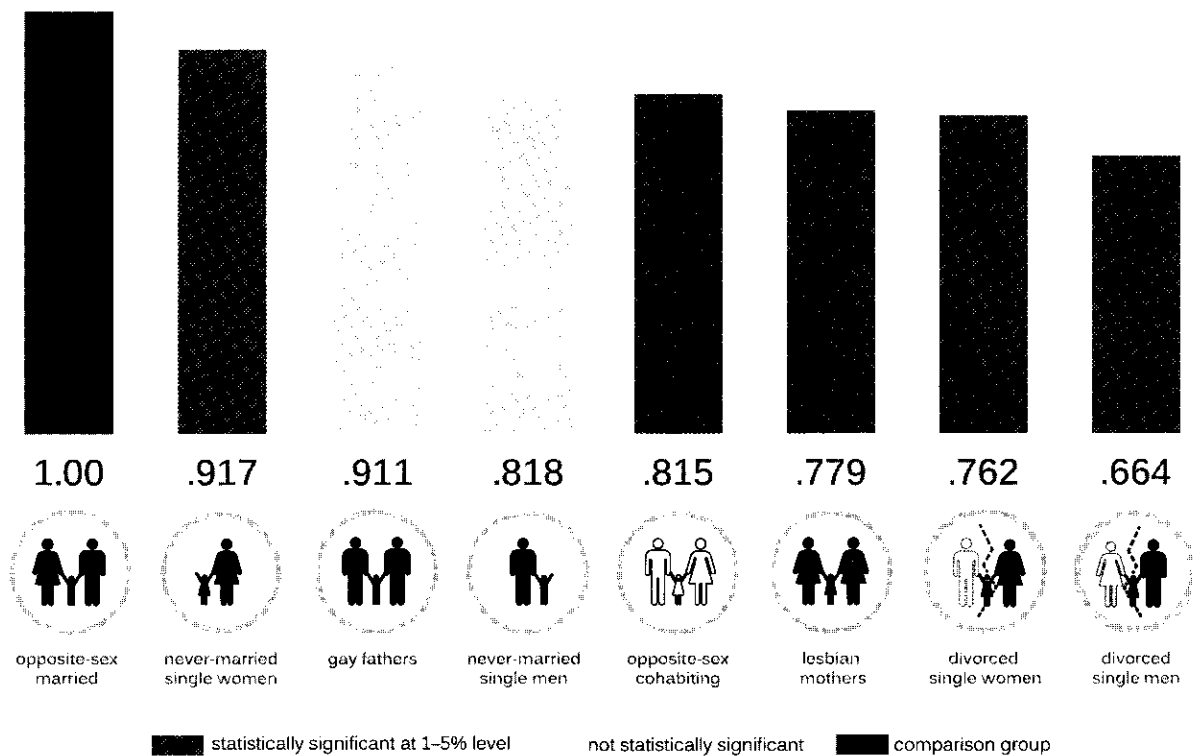
The conclusion of Allen, Pakaluk, and Price's first paper is that Rosenfeld made a series of questionable decisions about what children to include in the sample for study, decisions which yielded a conclusion of "no statistical differences" among the households. By increasing the sample size and controlling for Rosenfeld's concerns, the authors find noteworthy and statistically significant differences.

Further Results: Making Gender Distinctions

In their second paper, "Further Results," the researchers begin with the full, unrestricted sample of children and ask whether the gender of the children in same-sex families might make a difference in progress through school. They discover that girls in same-sex-parented homes have better odds of making normal progress through school than do boys in same-sex-parented homes.¹³

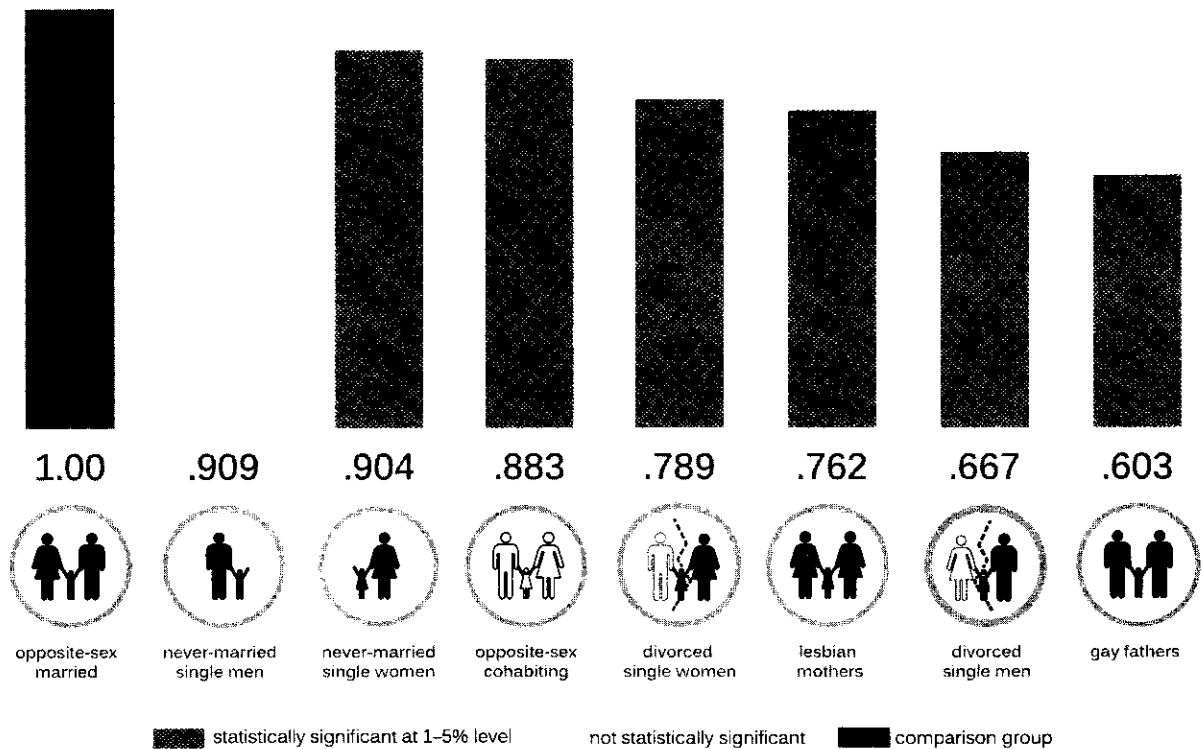
Then the researchers separate the lesbian households from the gay households, while keeping the groups of children segregated by gender, and discover additional differences between the boys' and the girls' outcomes. They find that while the girls are more likely to make normal progress in school than are the boys, the boys have the lowest of *all* odds of making normal progress through school when they are from gay-parented homes.

Odds Ratios of Girls' Normal Progress through School
(with Controls)



Source: Allen, Pakaluk, and Price, "Nontraditional Families," table 2, column 2.

Odds Ratios of Boys' Normal Progress through School (with Controls)



Source: Allen, Pakaluk, and Price, "Nontraditional Families," table 2, column 3.

In the case of both genders, then, children are more likely to make normal progress through school in the home of a single mother or with cohabiting parents than in the home of two same-sex parents, with boys performing less well than girls in same-sex homes, and especially poorly in gay-parented homes.

Interestingly, this latter result is the opposite of what Allen found in 2013 with the Canada Census data, where girls raised by gay fathers showed the poorest school performance of all other children.¹⁴ Allen notes that this difference in findings is probably due to a number of factors, including the rarity of children in same-sex homes and measurement errors in the U.S. Census. "The reasonable conclusion to draw is that gender matters. Prudence would suggest less confidence in how much and in what direction."¹⁵

Conclusion

Allen, Pakaluk, and Price critique Rosenfeld's decision to exclude certain children from the U.S. Census sample on the grounds that his decisions failed to adequately accomplish his intended goal of studying children from more stable families and with a biological relationship to a parent. They further challenge Rosenfeld (and other scholars) to not overlook children in these more challenging situations, suggesting that the link between same-sex parenting, biological-parent loss, and family instability is more relevant to children's outcomes than researchers let on. The authors also conclude that *gender matters*. Scholarship should study the link between gender distribution in each family type and children's outcomes, and not group all same-sex families or all children into one category.

When Allen, Pakaluk, and Price include all the children in the sample and add controls, they find notable and statistically significant differences in child outcomes: the odds of children from married-mother-and-father families making normal progress through school are 35-percent higher than those of children from same-sex-parent families. The odds of girls in lesbian-parent homes making normal progress are only 78 percent as high as those of girls in married-mother-and-father homes. For boys, the odds are 76 percent as high when in a lesbian home and 60 percent as high when in a gay-parented home, compared to married-mother-and-father homes. All of these are statistically significant differences between the groups of children. Given the ripple effects of falling behind in school, these are differences to notice.

Notes

1. Rosenfeld wrote his article prior to the release of the *New Family Structures Study*, the only other U.S. nationally representative data set available that includes same-sex-parented children.
2. See Appendix: Michael J. Rosenfeld, "Nontraditional Families and Childhood Progress through School," *Demography* 47 (August 2010): 755–775, p. 770.
3. Allen, Pakaluk, and Price also point out that Rosenfeld confuses "no differences" with "statistical insignificance." He actually found the latter but claimed to find the former.
4. See Rosenfeld, 758 (citing Alexander, Entwisle, and Horsey, 1997; Guo, Brooks-Gunn, and Harris, 1996; Moller et al., 2006; Roderick, 1994; Rumberger, 1987; Tillman, Guo, and Harris, 2006; McLanahan, 1985).
5. See Rosenfeld, 758 (citing Brooks-Gunn and Markman, 2005).
6. Allen, Pakaluk, and Price explain that the 2000 U.S. Census is a limited tool for measuring "normal progress through school" because of measurement errors. See their "Further Results," this volume, 8–10.
7. "The exact question on the census form asks whether the child is the 'natural-born son/daughter' of the household head. Of course, in a same-sex household, only one parent, at most, can be biologically related to the child and there is no reason why this parent must be the household head for census purposes" (Allen, Pakaluk, and Price, "Further Results," 3).
8. Allen, Pakaluk, and Price, "Further Results," 3.
9. Still, even when the children who are not biologically related to either parent are kept out of the sample but all of the children biologically related to *either* parent are placed back in, the authors find a statistical difference in child outcomes across the different households.
10. Allen, Pakaluk, and Price, "Further Results," 4.
11. "Necessarily, all of the children of same-sex couples will experience some degree of loss of relation to a parent: either they will be related to only one parent in the home (and stepchild to the other parent) or unrelated to both parents (adopted or foster). Such experiences could plausibly be the very channel through which negative effects might operate" (Allen, Pakaluk, and Price, "Further Results," 4). "There is also evidence that same-sex partnerships are less stable than heterosexual partnerships (both cohabiting and married) and this finding has a high degree of robustness (see Kurdek, 1998; Lau, 2012; Andersson et al., 2006; Kalmijn et al., 2007). One might surmise that stability is an important channel through which the child outcomes are produced. It is important that this channel be recognized in the data" (Ibid., 5).
12. Allen, Pakaluk, and Price, "Nontraditional Families," 955.
13. See Allen, Pakaluk, and Price, "Further Results," 6 (table 1, columns 2 and 3).
14. See Douglas W. Allen, "High School Graduation Rates Among Children of Same-Sex Households," *Review of Economics of the Household* 11 (December 2013): 635–658, included in this volume.
15. Allen, Pakaluk, and Price, "Further Results," 11.

Nontraditional Families and Childhood Progress Through School: A Comment on Rosenfeld

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Abstract We reexamine Rosenfeld's (2010) study on the association between child outcomes and same-sex family structure. Using the same data set, we replicate and generalize Rosenfeld's findings and show that the implications of his study are different when using either alternative comparison groups or alternative sample restrictions. Compared with traditional married households, we find that children being raised by same-sex couples are 35 % less likely to make normal progress through school; this difference is statistically significant at the 1 % level.

Keywords School progress · Gay and lesbian families

Introduction

Although there has been considerable research on the effect of family structure on child outcomes, almost none of the research using nationally representative samples has included same-sex parents as part of the analysis. The main barrier is the lack of a nationally representative data set with enough children being raised by same-sex couples to provide a basis for meaningful statistical inference. The one U.S. data set that currently provides enough data to conduct this analysis is the U.S. Public-Use Microdata Sample of the decennial census.

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Rosenfeld (2010) used data from the 2000 census 5 % Public-Use Microdata Sample to examine the association between same-sex parenting and child outcomes (as measured by normal school progress). His study concluded that the outcomes of children raised by same-sex couples “cannot be distinguished with statistical certainty from children of heterosexual married couples” (p. 770). However, our findings suggest a reevaluation of this conclusion. We present an alternative approach that updates the original study with important estimates of the magnitude of the effect sizes and their corresponding confidence intervals. We also provide a model that allows for robustness checks to the sample restrictions employed in the original study.

Our analysis proceeds in two parts. First, we show that the omitted group chosen for the regression itself informs the interpretation of the data because statistical precision is low. An alternative omitted variable leads to the finding that the outcomes of children raised by same-sex couples cannot be distinguished with statistical certainty from almost any other family structure, including well-documented disadvantaged groups, such as children being raised by a never-married mother. We also update the original study by providing standard errors and odds ratios for each of the coefficients. These two additions to the original study provide insight into the magnitude of the differences between groups and the size of the confidence intervals around these differences.

Second, we present an unrestricted model as a robustness check on the original findings by incorporating controls for the subgroups that Rosenfeld identifies as potential confounders. These subgroups are those in which the children in the sample are not biologically related to the household head (e.g., adopted children and stepchildren) and those in which the children and parents have not lived in the same residence for the previous five years. However, while we share Rosenfeld’s concern that these groups are potential confounders, the use of a restricted model both eliminates two of the channels through which family structure might actually affect child outcomes and reduces the sample by 55 %, thus making the standard errors sufficiently imprecise to detect a statistically significant difference. We present an alternative approach that accommodates the various concerns by incorporating controls for the subgroups into an unrestricted model. With this approach, we recreate the original results on the restricted sample and compare them to the more precise results from the unrestricted model with controls.

Alternative Comparisons and the Magnitude of the Effect Sizes

Table 1 provides descriptive statistics for the children in the sample by family structure type. The outcome variable is whether the child is making normal progress through school and is based on a comparison of the child’s age and current grade in school. This measure has been used in past studies because it is the only measure of child outcomes in the U.S. census data (Cáceres-Delpiano 2006; Conley and Glauber 2006) and it is correlated with other outcomes later in life, such as dropping out of high school (Eide and Showalter 2001). The fraction of children who are not making normal progress in schools varies from 1.83 % to 3.08 % across the different family structure types.

Table 1 provides the sample means for each of the control variables used in the regressions for each of the family structure types. The variables for "own child" (R_1) and "same location for 5 or more years" (R_2) are used to model the effects of the subset restrictions of interest.

Table 2, column 1, merely replicates the findings from the original study when the omitted group is children being raised by heterosexual married couples. Thus, all coefficients in column 1 provide inferences about the difference in outcomes between children being raised by heterosexual married couples and those in the other six family types. These results provide the basis for the original study's conclusion that children being raised by homosexual partners cannot be distinguished with statistical certainty from children of heterosexual married couples. However, there are good reasons to reevaluate this finding.

First, the magnitude of the coefficient for children raised in same-sex households is similar to that of never-married women, a family structure type which is documented to have worse child outcomes relative to married heterosexual couples. These alternative comparisons are difficult using the results of the original study because the main regression tables do not include the standard errors on the regression coefficients. One way to make these comparisons more transparent is to have the main group of interest (children of same-sex couples) be the omitted group in the regression, which is what we do in the second column of Table 2. The first coefficient is the same in column 1 and 2, since this particular comparison is the same when we switch which of the two is the omitted group. However, the rest of the coefficients now provide inferences about the difference between the reported group and same-sex households. These results highlight that an equally valid interpretation of the original study is that the outcomes of children in same-sex households cannot be determined with statistical certainty from those of children in almost any other family type (except divorced men).

Table 1 Summary statistics for households with children (2000 census)

	Heterosexual Married	Same-Sex Couples	Heterosexual Cohabiting	Divorced Women	Never- Married Women	Divorced Men	Never- Married Men
Median Income (\$)	58,000	48,820	40,250	24,000	15,900	35,800	29,000
% of Children Who Are:							
"Own" children (R_1)	85.7	82.3	83.1	91.7	97.8	87.8	90.9
Stepchildren	7.27	8.32	12.37	0.26	0.22	2.08	5.41
Foster children	0.32	0.81	0.97	0.76	0	0.35	0
Adopted	2.73	3.58	1.45	2.25	1.96	2.47	3.70
Other relation to head	4.03	4.95	2.14	5.02	0	7.32	0
Same location 5+ years (R_2)	52.4	42.2	21.3	39.5	35.3	47.0	37.2
Hispanic	15.8	23.9	23.2	16.7	16.4	12.4	21.2
Non-Hispanic black	8.21	16.6	22.0	24.1	61.8	13.0	34.5
Held back in school	1.83	2.81	2.56	2.76	2.80	3.08	2.59
<i>N</i>	1,189,833	8,632	66,642	215,021	77,879	46,654	6,153

Notes: The unit of observation in this table is the child. All of the results are weighted using the census sampling weights.

Table 2 Association between family type and making normal progress through school (alternative comparison groups)

	Logistic Regression Coefficients		Odds Ratios	
	(1)	(2)	(3)	(4)
Same-Sex Couples	-0.142 [0.125]	—	0.867 [0.108]	—
Heterosexual Married	—	0.142 [0.125]	—	1.153 [0.144]
Heterosexual, Cohabiting	-0.237** [0.057]	-0.094 [0.136]	0.789** [0.045]	0.910 [0.124]
Separated/Divorced/Widowed Women	-0.315** [0.026]	-0.173 [0.127]	0.730** [0.019]	0.841 [0.107]
Never-Married Women	-0.151** [0.041]	-0.008 [0.130]	0.860** [0.035]	0.992 [0.129]
Separated, Divorced, Widowed Men	-0.399** [0.046]	-0.257 [†] [0.132]	0.671** [0.031]	0.773 [†] [0.102]
Never-Married Men	-0.357* [0.123]	-0.215 [0.175]	0.700* [0.087]	0.807 [0.141]
Unweighted <i>N</i>	716,740	716,740	716,740	716,740

Notes: Each regression includes controls for disability, race, logged income, highest education in household, birthplace, metropolitan status, private school attendance, and state fixed effects. Standard errors are provided in brackets.

[†]*p* < .10; **p* < .05; ***p* < .01

The second reason to reevaluate the conclusions of the original study is that the effect sizes are quite large in magnitude despite falling short of statistical significance. The final two columns of Table 2 report the odds ratios that correspond to each of the coefficients from the first two columns. These odds ratios measure the percent difference in likelihood of a child making normal progress in school between children in the omitted group and each of the other groups. For example, column 4 of Table 2 indicates that children in traditional homes are 15 % more likely to make normal progress than children raised in same-sex households. Although this estimate is not statistically significant, the corresponding 95 % confidence interval for the difference ranges from -10 % to +47 %. Thus, while we are unable to reject the hypothesis that there is no difference, this approach also makes it impossible to rule out even very large differences (nearly 50 %).

Robustness Checks and the Unrestricted Model

The original study used a sample in which the children are biologically related to the household head (R_1) and in which the children and parents have been living at the same address for the past five years (R_2). Table 1 provides the fraction of children who are affected by each of these restrictions for each family structure type.

The motivation for the restrictions is to avoid a measurement error that would occur if a child's family structure is based on a current household composition that is different from what the child had experienced when his or her progress in school was being affected. However, these sample restrictions also cut off two of the channels through which family structure can influence child outcomes: biological relatedness and household instability. The sample restrictions also reduce the sample size by more than one-half, decreasing the precision of the estimates and making detection of a true difference in outcomes more difficult. In the alternative approach adopted here, we test an unrestricted model with the full sample of children but include controls for the important subgroups R_1 and R_2 .

To provide a baseline, column 1 of Table 3 presents the same restricted model used in column 4 of Table 2. In this model (as in the original study), both restrictions R_1 and R_2 are in place, but we present the coefficients as odds ratios for ease of interpretation. Column 2 presents a model in which R_1 is left in place while R_2 is relaxed to allow for children to be included who have not been in the same location for five years, though an indicator variable for the subgroup is included in the regression. By relaxing R_2 alone, the sample size increases by more than 80 %, from 716,719 to 1,397,144. The differential for children in heterosexual married households is +25.8 %, which lies squarely within the 95 % confidence interval from the

Table 3 Association between family type and making normal progress through school (varying the sample restrictions)

	(1)	(2)	(3)	(4)
Married Heterosexual	1.153 [0.144]	1.258** [0.097]	1.295* [0.137]	1.354** [0.089]
Heterosexual, Cohabiting	0.910 [0.124]	1.086 [0.088]	1.057 [0.123]	1.156 [†] [0.081]
Divorced Women	0.841 [0.107]	0.955 [0.074]	0.993 [0.107]	1.053 [0.070]
Never-Married Women	0.992 [0.129]	1.161 [†] [0.093]	1.140 [0.127]	1.232** [0.085]
Divorced Men	0.773 [†] [0.102]	0.821* [0.067]	0.895 [0.101]	0.900 [0.064]
Never-Married Men	0.807 [0.141]	1.096 [0.124]	0.952 [0.152]	1.183 [0.122]
Sample Restriction				
Own child (R_1)	X	X		
Five-year resident (R_2)	X		X	
Unweighted N	716,740	1,397,188	792,294	1,610,880

Notes: Each cell reports the odds ratio from a logit regression with the omitted group being children being raised by same-sex couples. Each regression includes controls for disability, race, logged income, highest education in household, birthplace, metropolitan status, private school attendance, and state fixed effects. In column 2 we also control for whether the household moved; in column 3 we control for whether a child is the parent's own child; and in column 4 we control for both. Standard errors are provided in brackets.

[†] $p < .10$; * $p < .05$; ** $p < .01$

restricted model (column 1), but the point estimate is larger in magnitude and is statistically significant at the 1 % level.

Column 3 presents a symmetric model in which we reinstitute the five-year location restriction (R_2) but relax the “own-child” restriction (R_1). This restriction removes not only adopted and foster children but any child who is not biologically related to the household head (the majority of which are biologically related to the spouse or partner of the household head). Relaxing R_1 increases the sample size more modestly, from 716,719 to 792,260. In this model, there is a differential of +29 % for children in heterosexual married homes as compared with those in same-sex households. This estimate is statistically significant at the 1 % level.

One potential concern with this specification is that children who are adopted by heterosexual married couples may have a different profile than children who are adopted by same-sex couples. When we exclude adopted children from the regression in column 3, the sample size drops to 770,325 with an estimated differential of +24 %, which is statistically significant at the 5 % level.

The final column of Table 3 presents the unrestricted model in which both sample restrictions are relaxed. This provides estimates utilizing the full sample of 1.6 million children.¹ In this specification, we include controls for the important subgroups, as in the previous two regressions. Here, the differential in the likelihood of making normal school progress is +35 % for children in heterosexual married households, which is statistically significant at the 1 % level.

Two alternative comparisons in this final specification are also illuminating. First, some have suggested that the appropriate comparison for children being raised in same-sex households would be heterosexual cohabiting partners because, at the time of the 2000 census, same-sex couples were unable to obtain a civil marriage. Under this comparison, children who are being raised by a heterosexual cohabiting couple are about 15 % more likely to be making normal progress in school than children being raised by same-sex couples, and this difference is statistically significant at the 5 % level. Second, the three family types that cannot be distinguished with “statistical certainty” from same-sex households—even with the full sample—are divorced men, divorced women, and never-married men.

Conclusion

Together, these findings are strikingly different from those of the original study—and the differences are large enough to be noteworthy. With respect to normal school progress, children residing in same-sex households can be distinguished statistically from those in traditional married homes and in heterosexual cohabiting households. The magnitude of the differences is large enough to be relevant for current and future policy debates, as well as to indicate a real need for more research into the channels through which family structure affects child development and educational outcomes.

¹ A careful reader might notice that the sum of the increase in the sample size in columns 2 and 3 (680,448 and 75,554) is less than the increase when we relax both restrictions (894,140). The additional 138,138 children are those for whom both restrictions apply.

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