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Supplementary Information for

Differential fertility makes society more conservative on “family values”

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This PDF file includes:

Supporting Information Text

A. Within-Cohort Composition Effect

As shown in Fig. S1, conservatism and sibship size vary across cohorts. The composition effects estimated in the main text reflect both between- and within-cohort variation, but one may want to focus exclusively on the within-cohort component. In this appendix section, we estimate within-cohort composition effects to complement our main results.

We define the within-cohort absolute and relative composition effects against a counterfactual in which the prevalence of traditional-family conservatism is deweighted within each cohort, but the cohort composition of the sample is held fixed. To do so, we extend the notation from *Materials and Methods* to let π_k^c be the share of individuals with sibship size k from cohort c that are opposed to abortion or same-sex marriage; let η_k^c and $\tilde{\eta}_k^c$ be the actual and deweighted shares of cohort c with sibship size k, respectively; and let ω^c be the share of the sample from cohort c. Then the within-cohort absolute composition effect is:

$$\Delta^{\dagger} = \sum_{c=1915}^{1990} \sum_{k=1}^{14} \omega^c (\eta_k^c - \tilde{\eta}_k^c) \pi_k^c, \qquad [3^{\dagger}]$$

and the within-cohort relative composition effect is:

$$\delta^{\dagger} = \frac{\Delta^{\dagger}}{\sum_{c=1915}^{1990} \sum_{k=1}^{14} \omega^c \tilde{\eta}_k^c \pi_k^c}. \qquad [4^{\dagger}]$$

As elsewhere, we use 5-year birth cohorts.

Fig. S11 compares the new within-cohort composition effects with the overall composition effects from the main analysis. For abortion, the within-cohort and overall effects are virtually identical. For same-sex marriage, the within-cohort effects are slightly smaller but similar in trend.

B. Accounting for Childlessness

The composition effects estimated in the main text rely on a counterfactual in which sibship size is independent of traditional-family conservatism, which effectively limits them to the effects of differential fertility across *parents*. As a result, they ignore selection into parenthood. In this appendix section, we estimate lower bounds for composition effects that include non-parents in the counterfactual. The counterfactual now asks: what if *all* members of the previous generation (not just parents) had the same number of children?

In *Materials and Methods*, the parents-only composition effects in equations (3)-(4) summed over sibship sizes $k = 1, \cdots, 14$. We rewrite these equations to include sibships of size $k = 0$, i.e., the potential children of childless individuals. The absolute composition effect becomes:

$$\Delta^{\star} = \sum_{k=0}^{14} (\eta_k - \tilde{\eta}_k) \pi_k, \qquad [3^*]$$

and the relative composition effect becomes:

$$\delta^{\star} = \frac{\Delta^{\star}}{\sum_{k=0}^{14} \tilde{\eta}_k \pi_k}. \qquad [4^*]$$

As before, π_k denotes the conservative share of individuals from sibships of size k, η_k denotes their population share, and $\tilde{\eta}_k$ denotes the population share of their parents.

$\Delta^\star$ and $\delta^\star$ incorporate three new quantities: π_0, η_0, and $\tilde{\eta}_0$. Of these three quantities, one is known, one is estimable in existing demographic datasets, and one is neither known nor directly estimable. The known quantity is η_0, which equals 0 because individuals from sibships of size 0 do not exist.

The estimable quantity is $\tilde{\eta}_0$. We estimate it using GSS, census, and vital statistics data, highlighting a striking fact: for all cohorts of the 20^{th} century that have completed childbearing, the number of childless individuals was approximately equal to the number of individuals with exactly one child. Fig. S12 first documents this fact in the GSS (1), using all survey waves (1972-2018) to plot histograms of the number of children among adults 40-60, by decadal cohort and overall. Although the shape of the histogram above 1 varies substantially across cohorts, it is flat between 0 and 1 within every cohort and in the full sample. The ratio of childless individuals to one-child individuals in the full sample is 1.036.

Fig. S13 does the same for women ages 40-60 in pooled 1% samples from the 1940-1990 US Censuses (2) and cohort fertility tables from the National Vital Statistics System (3, 4).* Here again, the distribution evolves above 1 but stays flat between 0 and 1, for all cohorts except 1880-9, 1890-9, and perhaps 1900-9.

Individuals in the main GSS sample are unlikely to have mothers from these exception cohorts. Data on parental year of birth are unavailable in most GSS waves, so we use data on children 0-10 in 1% samples from the 1920 to 2000 US Censuses (2) to estimate the distribution of mothers' birth years for each birth year among children. We weight child birth years according to the distribution of cohorts in our main GSS sample and plot the histogram of mothers' potential birth years in Fig. S14. For the full GSS sample, the 5th percentile of mother's potential birth year is 1903, the 95th percentile is 1960, and the interquartile range is 1922-1949. For the subsample of respondents with no siblings, the 5th percentile is 1901, the 95th percentile is 1960, and the interquartile range is 1918-1948.

These distributions suggest that in the parent generation to our GSS sample, childless women numbered as many as one-child mothers. Indeed, if we average across the histograms in Fig. S13, weighting by the distribution in Fig. S14, we find that the ratio of childless women to one-child mothers is 1.006-1.013 (Fig. S15). Based on the results in Figs. S12-S15, we conclude that the previous generation had as many childless members as one-child members, so we set $\tilde{\eta}_0 = \tilde{\eta}_1$.

After setting $\eta_0 = 0$ and $\tilde{\eta}_0 = \tilde{\eta}_1$, we are left with one unknown: the conservative share of the potential children of the childless, π_0. This quantity is unknowable, but Fig. 1 suggests that it would be no higher than the conservative share of only children, π_1. We proceed with the assumption that $\pi_0 \leq \pi_1$ to compute lower bounds on $\Delta^\star$ and $\delta^\star$. Fig. S16 compares the parent-only composition effects from the main analysis with lower bounds that account for childlessness. For both abortion and same-sex marriage, the lower bounds that account for childlessness are similar in levels and trends to the parents-only composition effects estimated in Fig. 4.

C. Differential Mortality

We have focused on the effects of differential fertility, but differential mortality may also reweight public opinion, potentially offsetting the composition effect of differential fertility. In this appendix section, we investigate how mortality differences by traditional-family conservatism contribute to the reshaping of the population.

One easy approach to assessing whether differential mortality exacerbates or mitigates the

*Later censuses did not collect data on children ever born, and the vital statistics system has incomplete childbearing histories for earlier cohorts.

composition effect of differential fertility is to focus on ages at which mortality is minimal. Fig. S17 plots age-specific mortality rates from 2010-2016 for the United States (5). Rates fall with age in early childhood and then rise exponentially with age in adulthood, with no obvious threshold age at which rates rise disproportionately. However, the conventional age-65 "elderly" cutoff is convenient. Age-specific mortality rates never exceed 15 deaths per 1000 below this age. Fig. S18 re-estimates composition effects of differential fertility on the sub-sample of GSS respondents below age 65 and compares them with the full-sample estimates from Fig. 4. Levels and trends are similar to our main results.

A more involved approach is to directly estimate mortality differentials by traditional-family conservatism and to simulate the consequences for public opinion. We estimate mortality differentials using Muennig et al.'s (6) linkage of the 1978-2010 General Social Surveys with the National Death Index through 2014. This linkage leads to a sample size of roughly 5,000 observations, depending on the outcome, for our post-2004 study period. Because the small sample size limits power to detect associations for a rare outcome like mortality, we supplement the post-2004 sample results with complete sample results going back to 1978. The GSS has only asked consistently about same-sex marriage since 2004, so we also analyze respondents' opinions on whether "sexual relations between two adults of the same sex" are "always wrong," available in the linked dataset since 1980.

We estimate mortality differentials by running a separate Cox proportional hazard regression of annual mortality on each opinion, controlling for 5-year birth cohort and 5-year age group. The covariates purge our estimates of associations stemming from the higher conservatism and higher mortality of the elderly, for example. We view this generational phenomenon as inherent to the process of cohort replacement and therefore not in the spirit of our focus on cross-sectional mortality differentials.

Table S1 reports hazard ratios based on the Cox estimates. Opposition to abortion is not significantly associated with mortality risk in either sample (cols. [1]-[2]). In contrast, individuals opposing same-sex sexual relations face a 9% higher annual risk of mortality ($P < 0.01$) in the 1980-2010 sample (col. [3]). In the post-2004 sample, this opinion is associated a similar 12% rise in risk (col. [4]), but it is not statistically significant due to lower power. Opposition to same-sex marriage is also associated with a non-significant 12% increase (col. [5]) in the post-2004 sample. Taken together, these results suggest that conservative opinion on homosexuality, but not abortion, is associated with elevated mortality risk, a finding consistent with (7).

We simulate the extent to which this mortality gap affects the prevalence of conservative (anti-gay) sentiment in each decadal birth cohort for which the National Vital Statistics System has mortality data going back to infancy. Given varying assumptions about the initial share of conservatives in the cohort, we solve for conservative and non-conservative mortality rates that maintain a 9.4% mortality differential and match the cohort's overall survival experience, as documented in Fig. S19 (5). We then compute the absolute composition effect of differential mortality at a given age as the simulated conservative share at that age minus the initial conservative share.

Fig. S20 displays these simulated composition effects over the lifecycle by cohort, finding that they are small relative to the composition effects of differential fertility. Differential mortality decreases the prevalence of anti-gay sentiment, but for most cohorts at most ages, the effect is no more than 0.5 percentage points. The effect is largest for the earliest cohort, born in 1940-49, because this cohort experienced higher age-specific mortality rates than others and has also advanced to older ages. However, even for this cohort, the effect never exceeds 1 percentage point. In comparison, we estimate the absolute composition effect of differential fertility on anti-gay sentiment at 3.0

percentage points, far in excess of all of these quantities.[†]

Although the simulated effects in Fig. S20 are useful for benchmarking how large mortality composition effects might be relative to fertility composition effects, they must be interpreted with caution. A major limitation is their treatment of children. Quantifying the composition effect requires knowledge of (future) anti-gay sentiment among children and its association with childhood mortality, neither of which is estimable with the GSS. For our benchmarking exercise, we have extrapolated mortality differentials from adulthood to childhood and assumed a starting point for prevalence at birth.

This point about children occasions a return to the discussion of Darwin in the main text. Differential mortality in old age—the main driver of the mortality composition effects in Fig. S20, whatever their size—is irrelevant for natural selection. Differential mortality only leads to natural selection if it occurs before the end of reproductive age, and we have limited data on that stage of the life course. However, because mortality rates are low for children, youth, and young adults (Figs. S17 and S19), we expect little natural selection due to differential mortality in the United States.

[†] Because Fig. S20 simulates mortality composition effects within cohorts, it may be preferable to compare it with the within-cohort fertility composition effect—2.5 percentage points, averaged across cohorts—in Fig. S11.

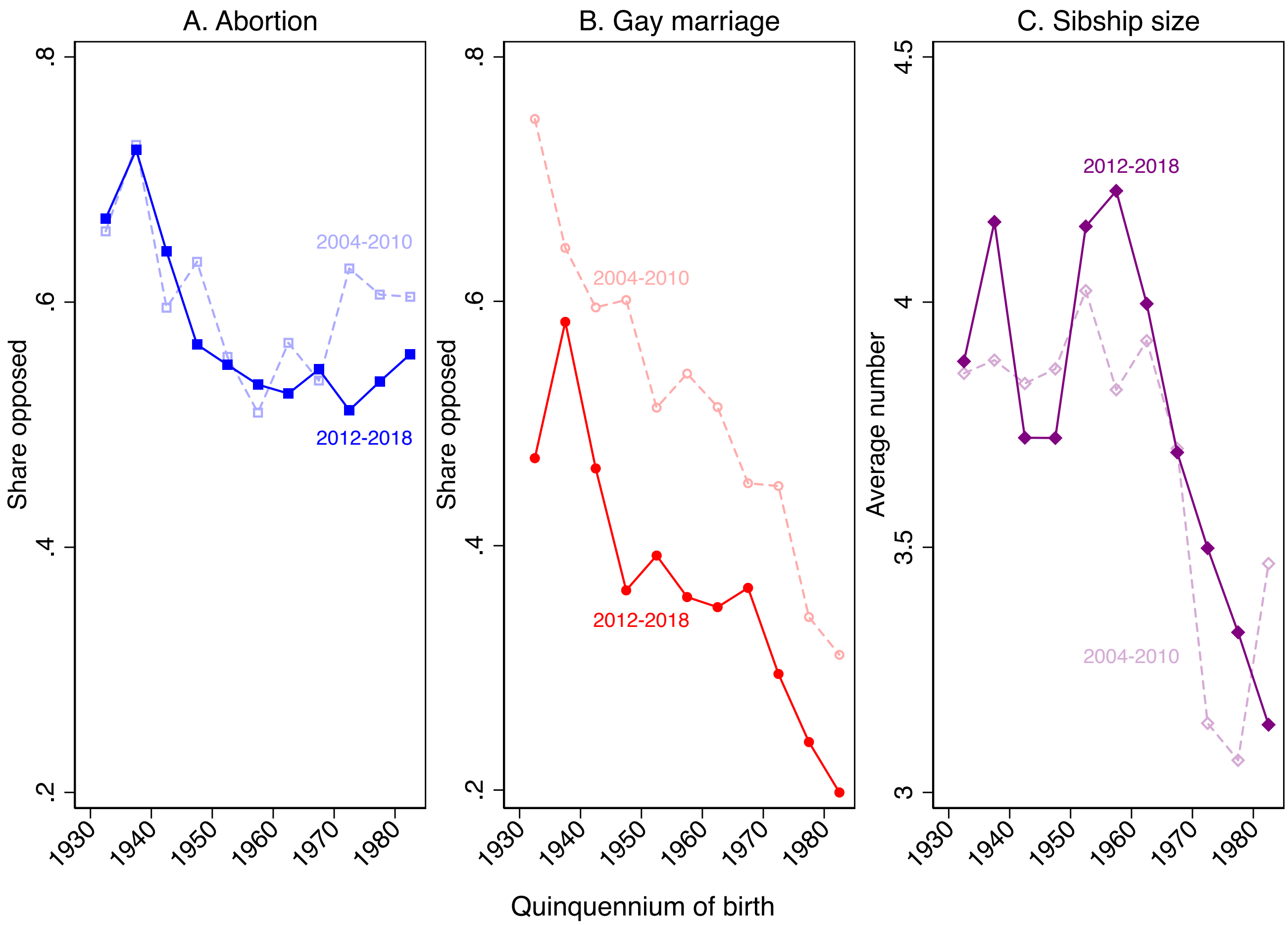

Fig. S1. Traditional-family conservatism and sibship size across cohorts and over time. Plots opposition shares and average sibship sizes across 5-year birth cohorts for the first (2004-2010) and second (2012-2018) halves of the sample period. Includes the central 90% of birth cohorts.

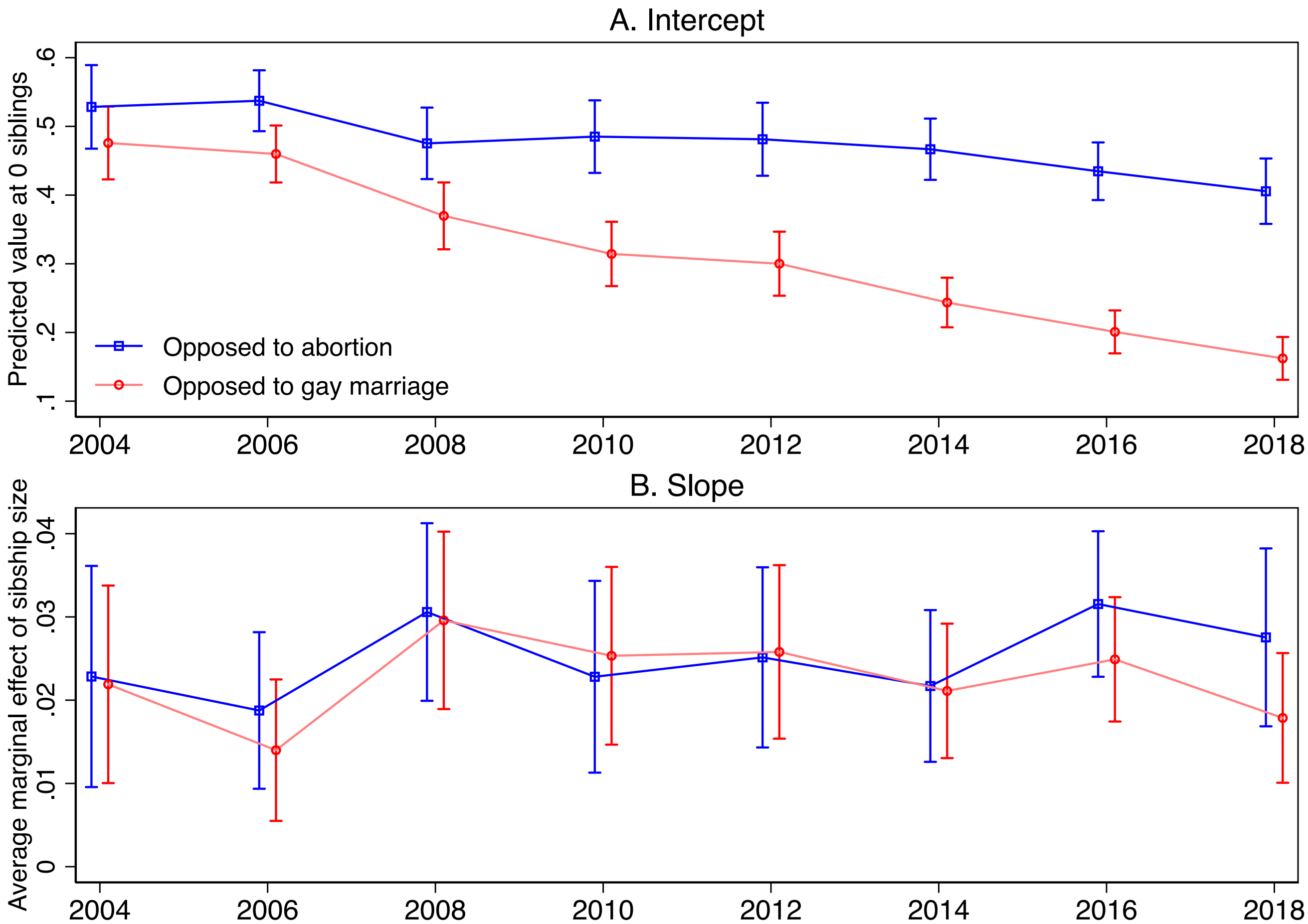

Fig. S2. Slopes and intercepts of sibsize-conservatism relationships by year. Plots opposition shares and average sibship sizes across 5-year birth cohorts for the first (2004-2010) and second (2012-2018) halves of the sample period. Includes the central 90% of birth cohorts.

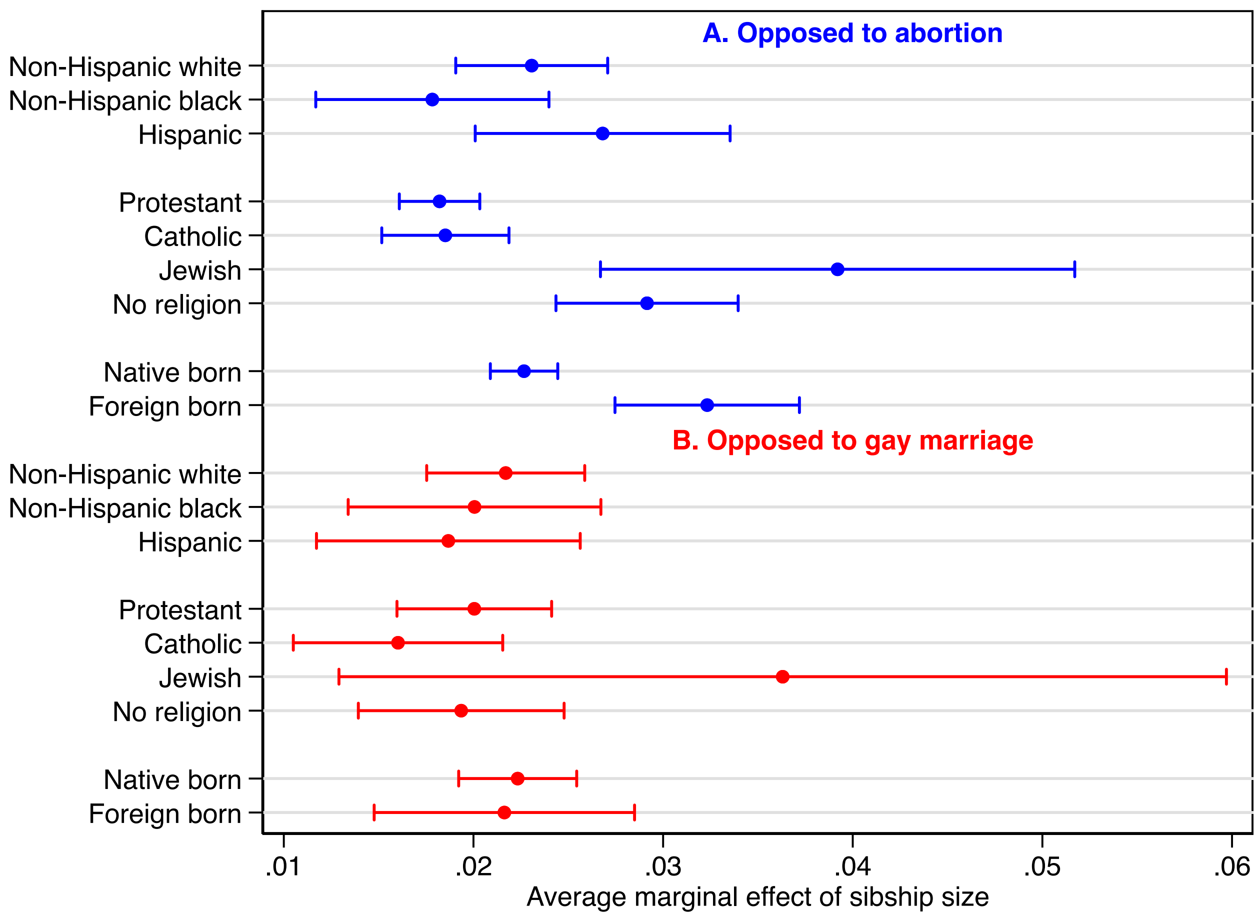

Fig. S3. Association of sibship size with traditional-family conservatism by demographic group. Points are marginal effects from probit models. Capped spikes are 95% confidence intervals based on heteroskedasticity-robust standard errors.

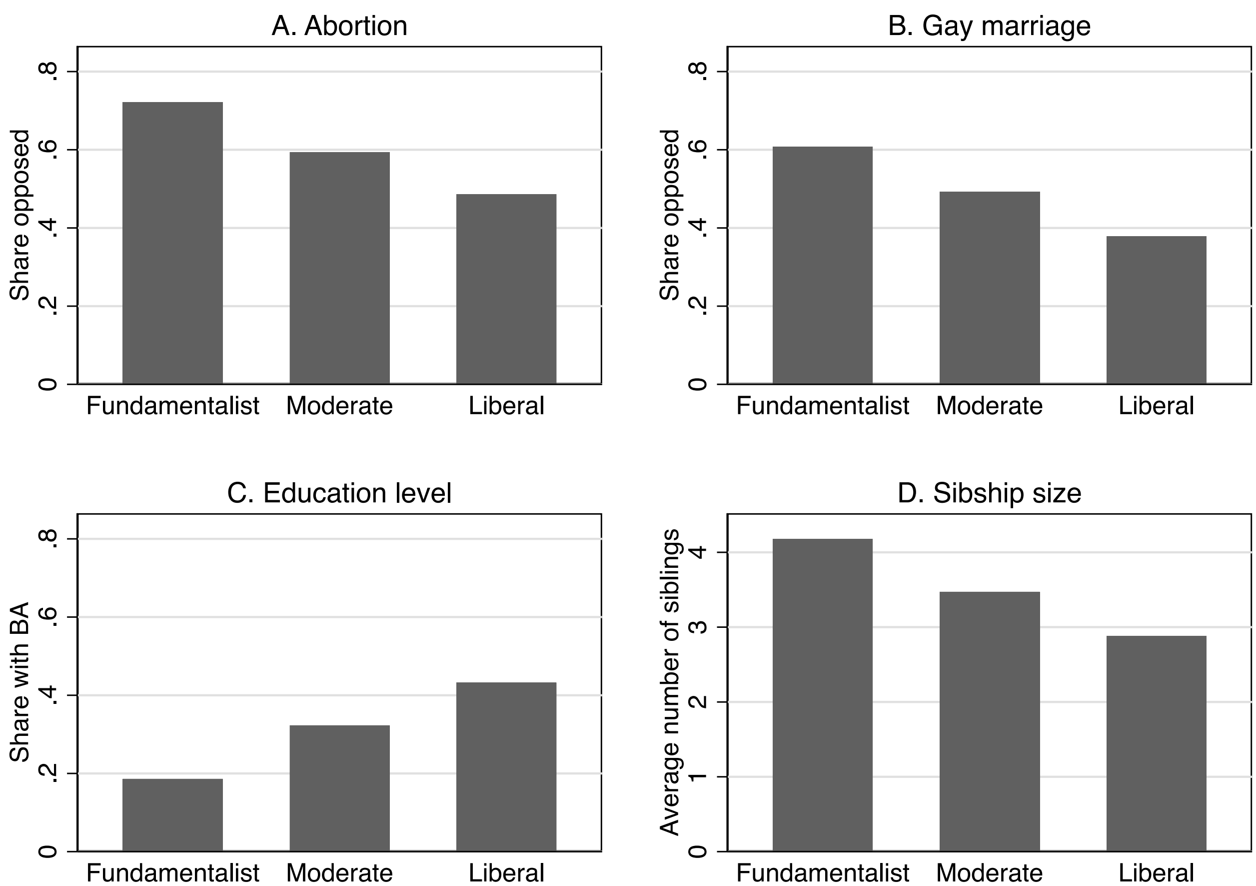

Fig. S4. Denominational differences among Protestants.

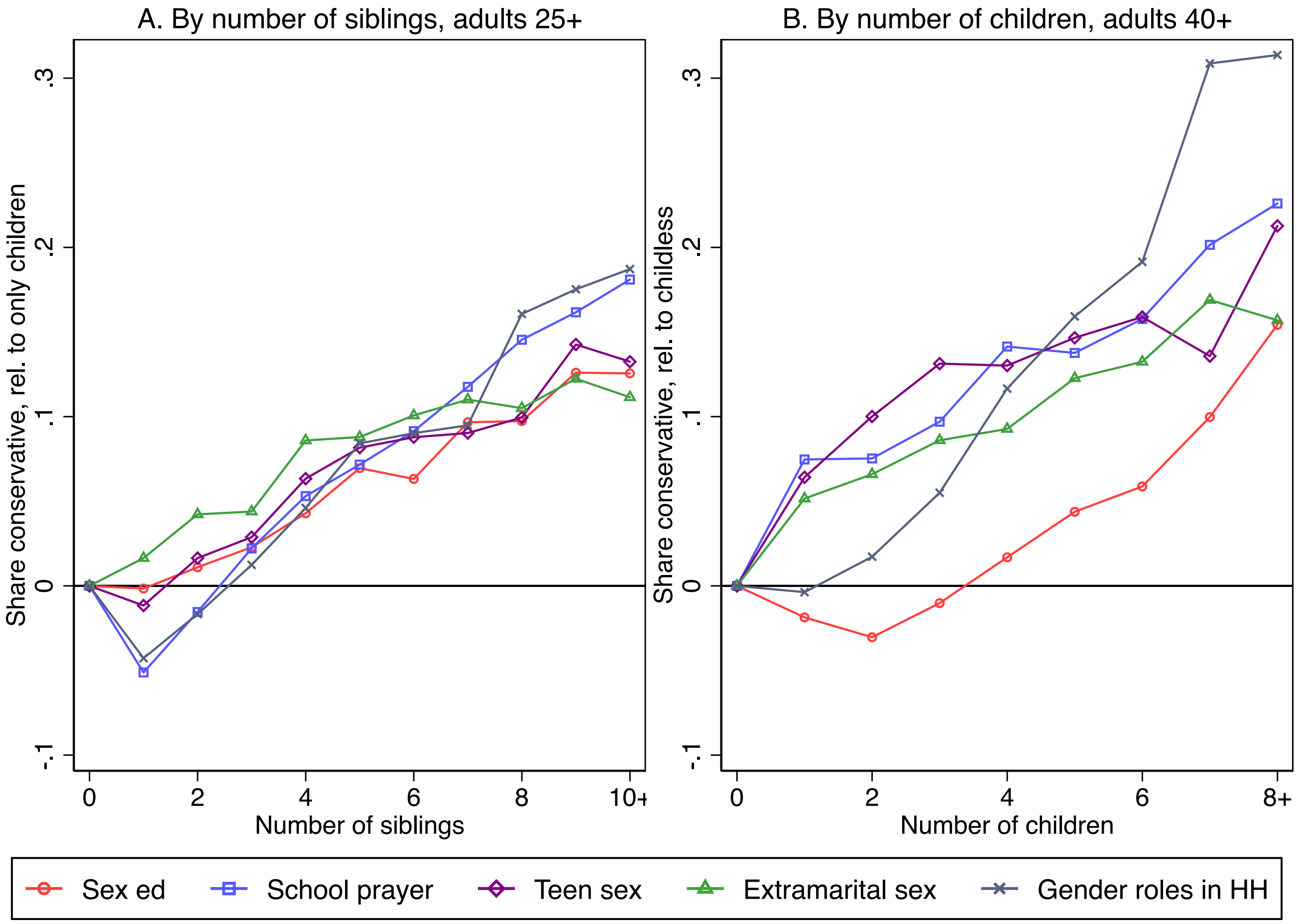

Fig. S5. Other dimensions of social conservatism. Because the overall prevalence of conservatism varies widely across issues, the figure compresses the y-axis by plotting the share reporting conservative values among individuals with x siblings/children, minus the share among individuals with 0 siblings/children. 'Sex ed' measures the share opposed to sex education in public schools, 'school prayer' measures the share approving of bible prayer in public schools, 'teen sex' measures the share opposed to sex before marriage among teens 14-16, 'extramarital sex' measures the share believing extramarital sex is wrong, and 'gender roles' measures the share agreeing that it is better for a man to work and for a woman to tend home.

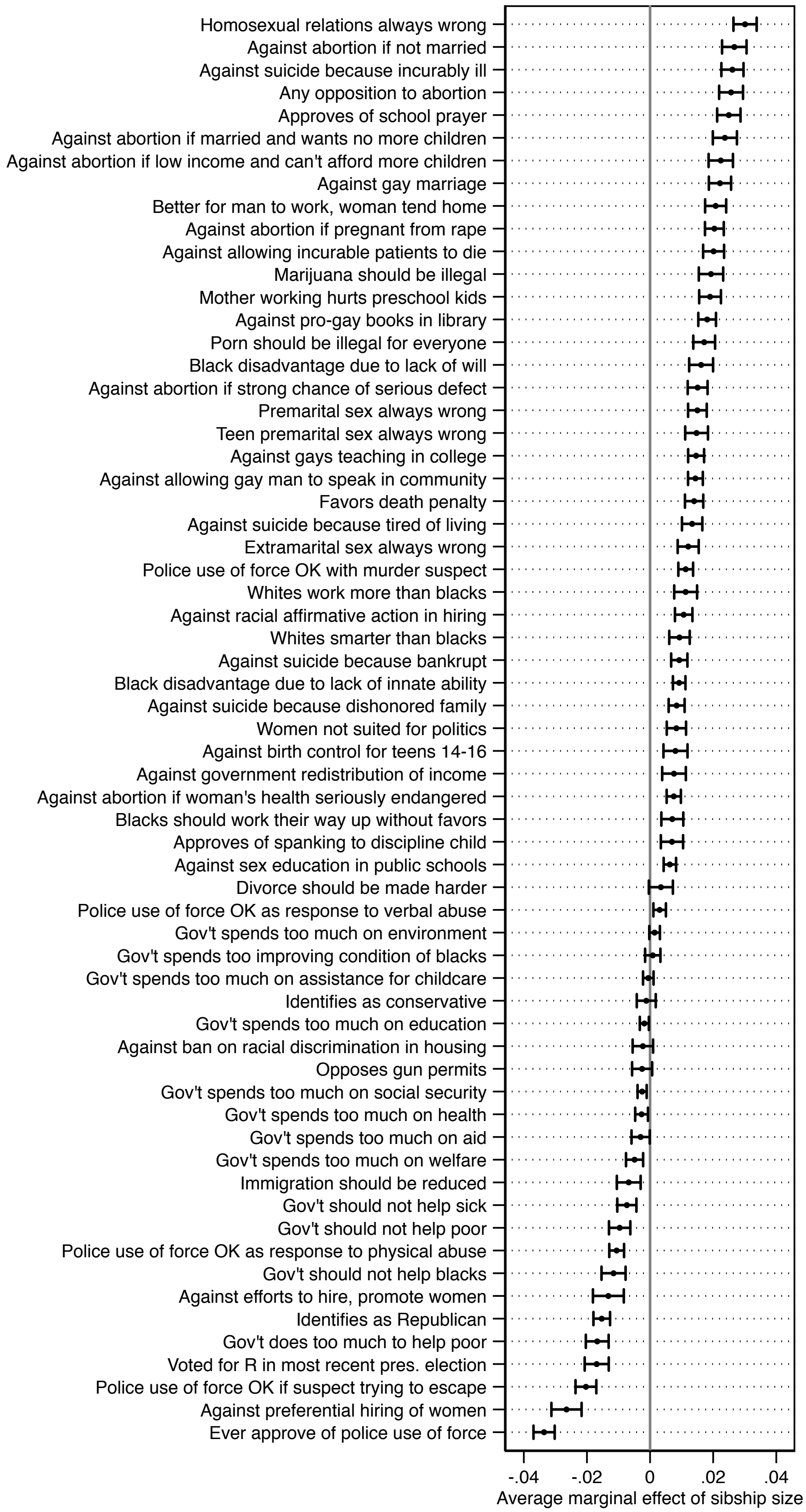

Fig. S6. Sibship size and conservative opinion on 63 issues. Points represent average marginal effects from probit models. Capped spiked represent 95% confidence intervals based on robust standard errors. Includes questions asked in every 2004-2018 survey for which conservatism could be coded.

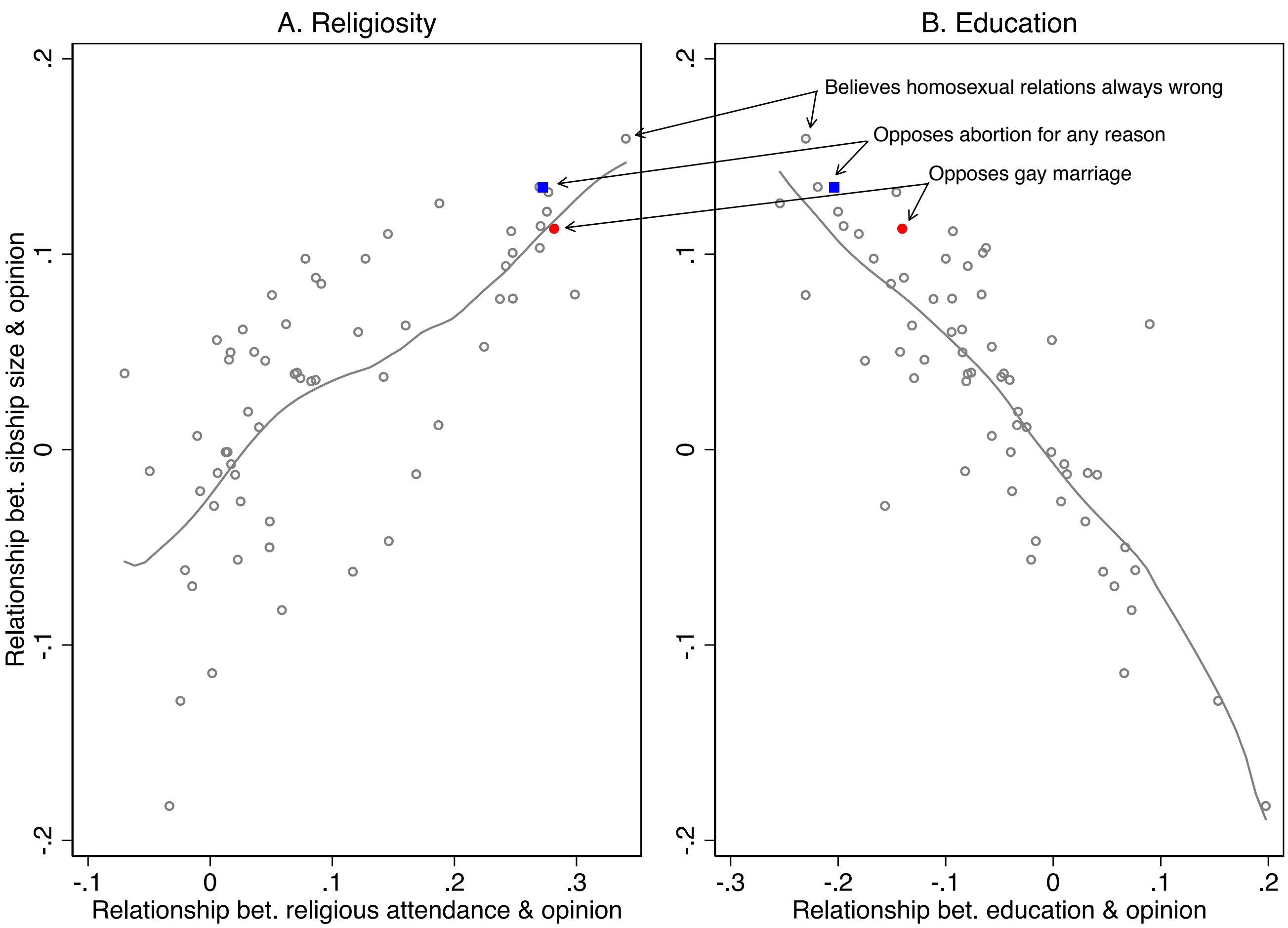

Fig. S7. Sibship size, religiosity, education, and conservative opinions on 63 issues. The relationship between a variable x and an opinion y is here measured as the average y among individuals with above-median values of x minus the average y among individuals with below-median values of x. Each marker represents a different opinion variable. Black curves are local linear regressions with bandwidths of 0.05. Includes questions asked in every 2004-2018 survey for which conservatism could be coded.

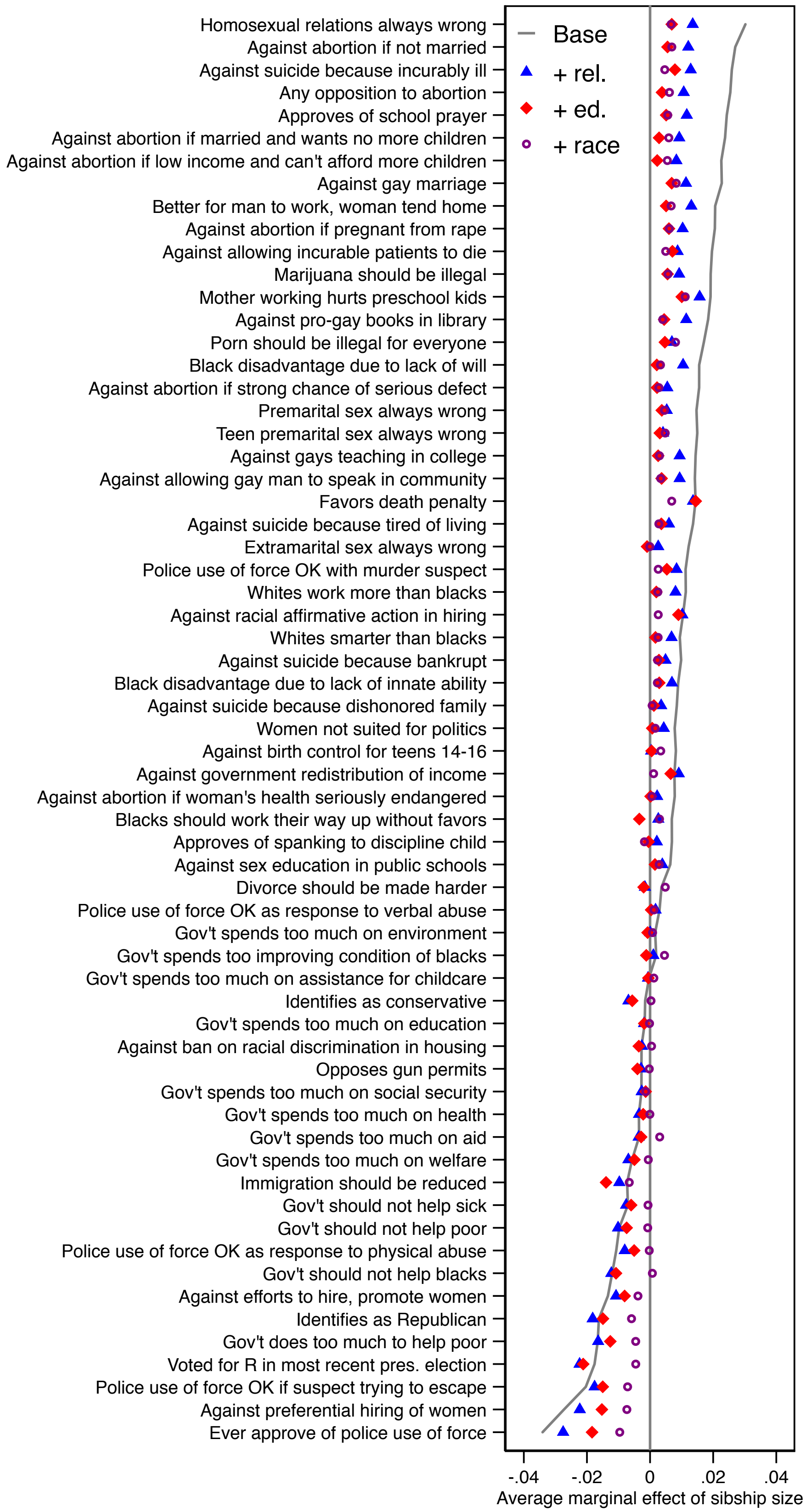

Fig. S8. Sibship size and conservative opinion on 63 issues, unadjusted vs. adjusted estimates. Average marginal effects from probit models. "Base" refers to unadjusted estimates. Scatter plots add successively more covariates. Covariates are described in *Materials and Methods*.

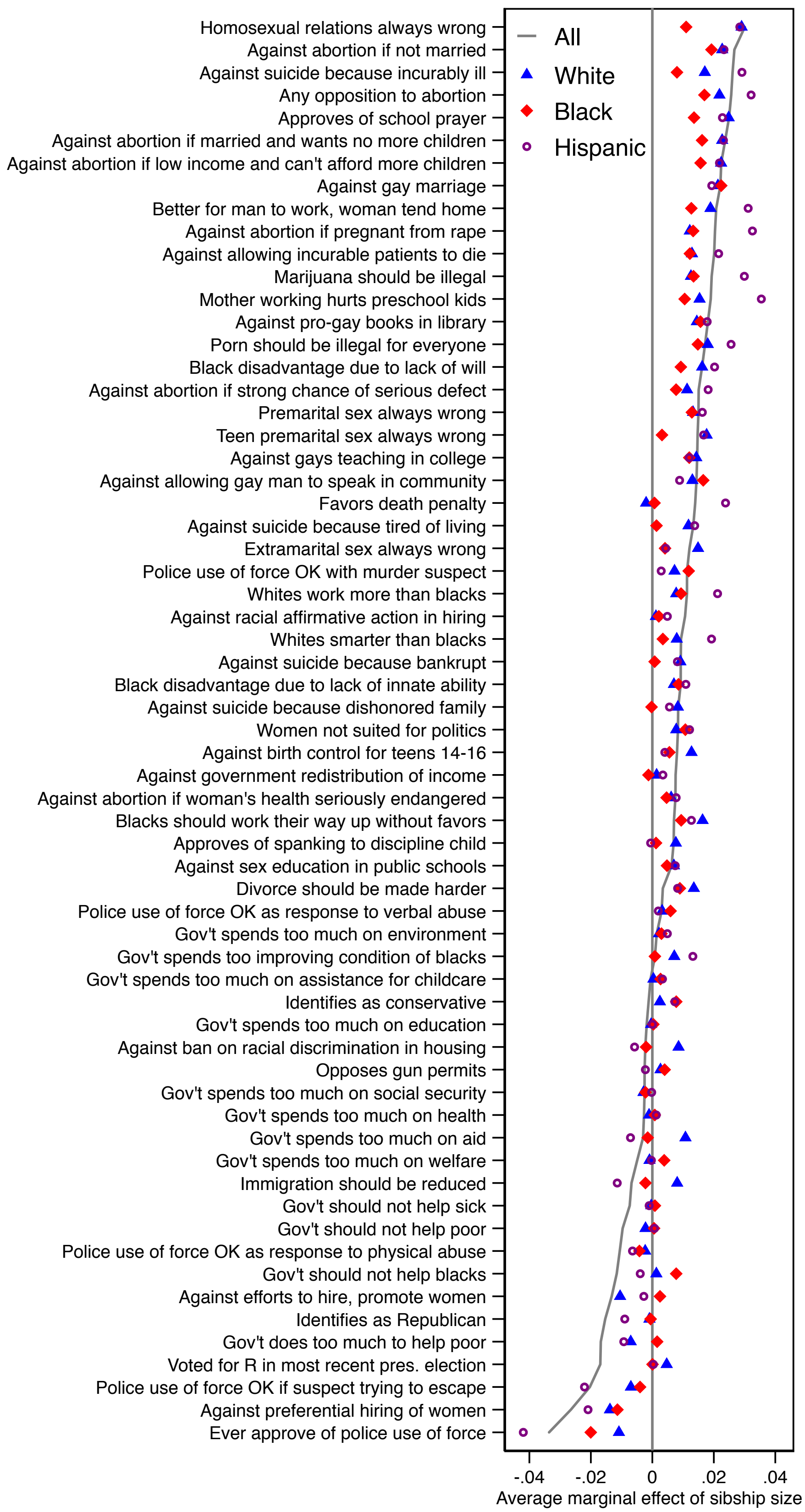

Fig. S9. Sibship size and conservative opinion on 63 issues, heterogeneity by race/ethnicity. Average marginal effects from probit models.

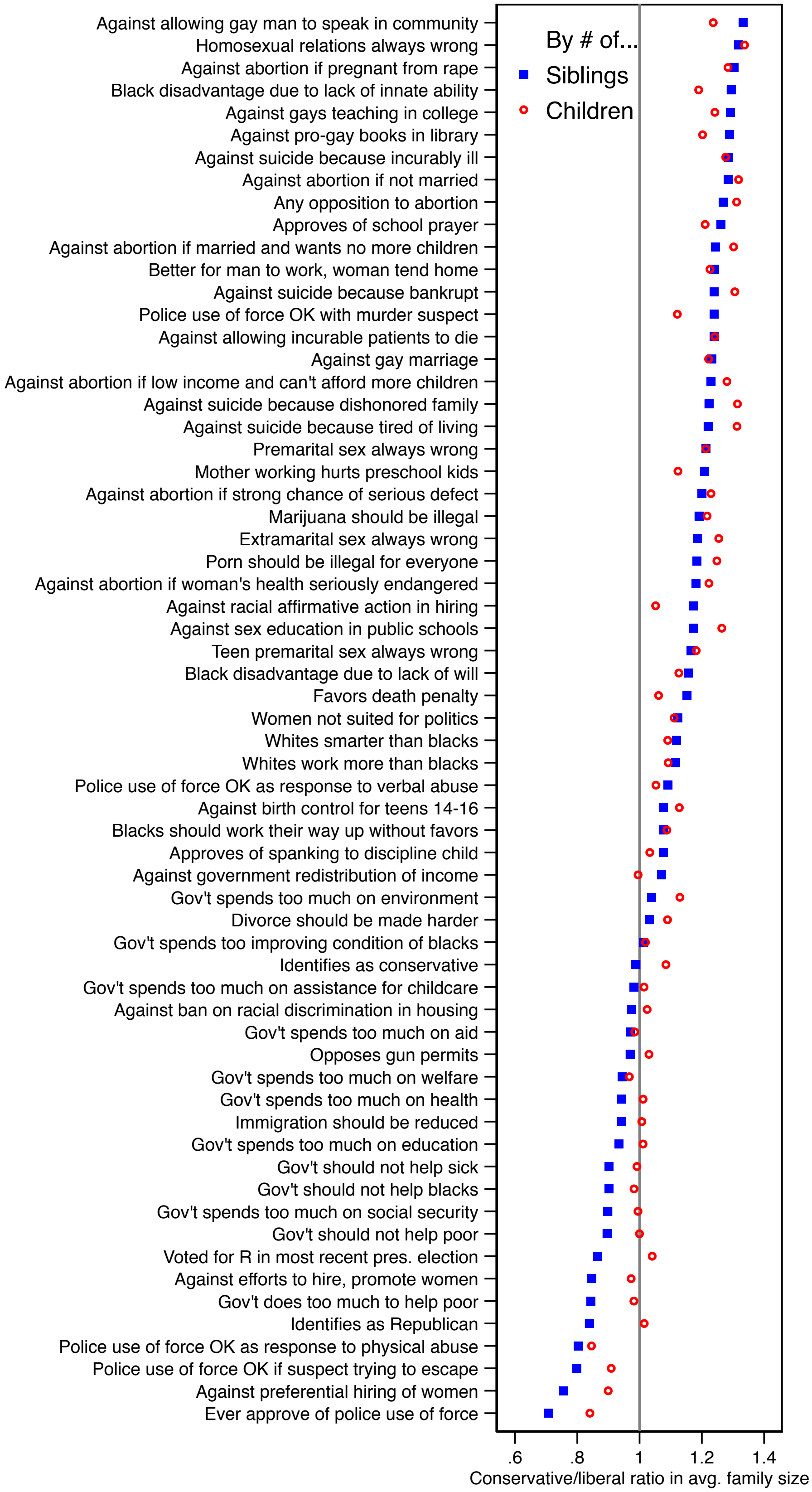

Fig. S10. Ratios of average family size among conservatives to average family size among liberals on 63 issues. For number of siblings, the sample includes adults ages 25+; for number of children, it includes adults ages 40+. We plot ratios to maintain a similar scaling with both family size measures.

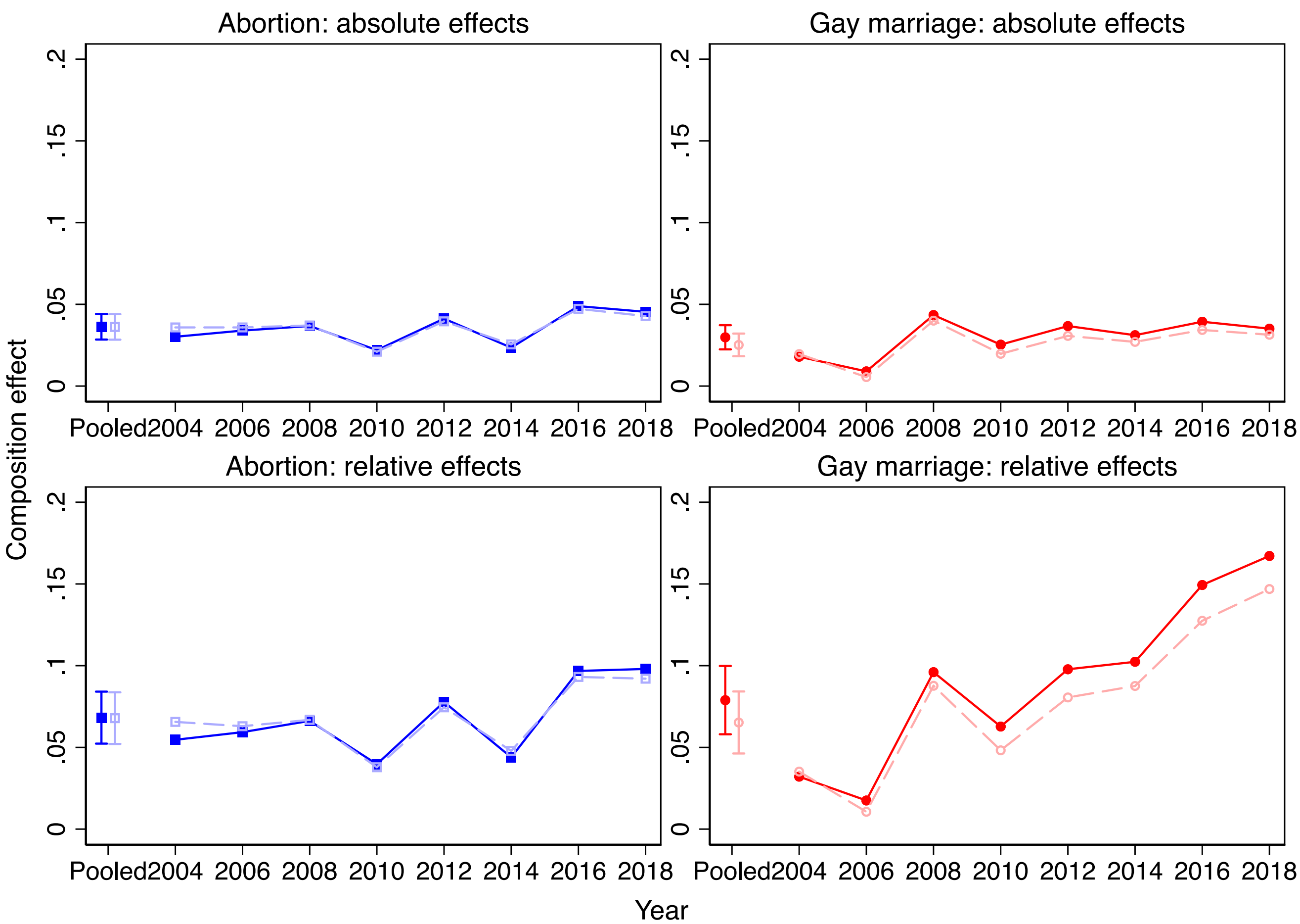

Fig. S11. Baseline composition effect (solid) versus within-cohort composition effect (dashed). The absolute composition effect is the actual prevalence of opposition minus deweighted prevalence; the relative composition effect is the absolute composition effect divided by deweighted prevalence. Capped spikes are 95% confidence intervals based on bootstrapped standard errors. The figure compares the baseline composition effect from Fig. 4 with a within-cohort version of the composition effect that first calculates a cohort-specific composition effect and then averages the cohort-specific effects, weighting by each cohort's share in the sample. Cohorts are defined by quinquennium of birth.

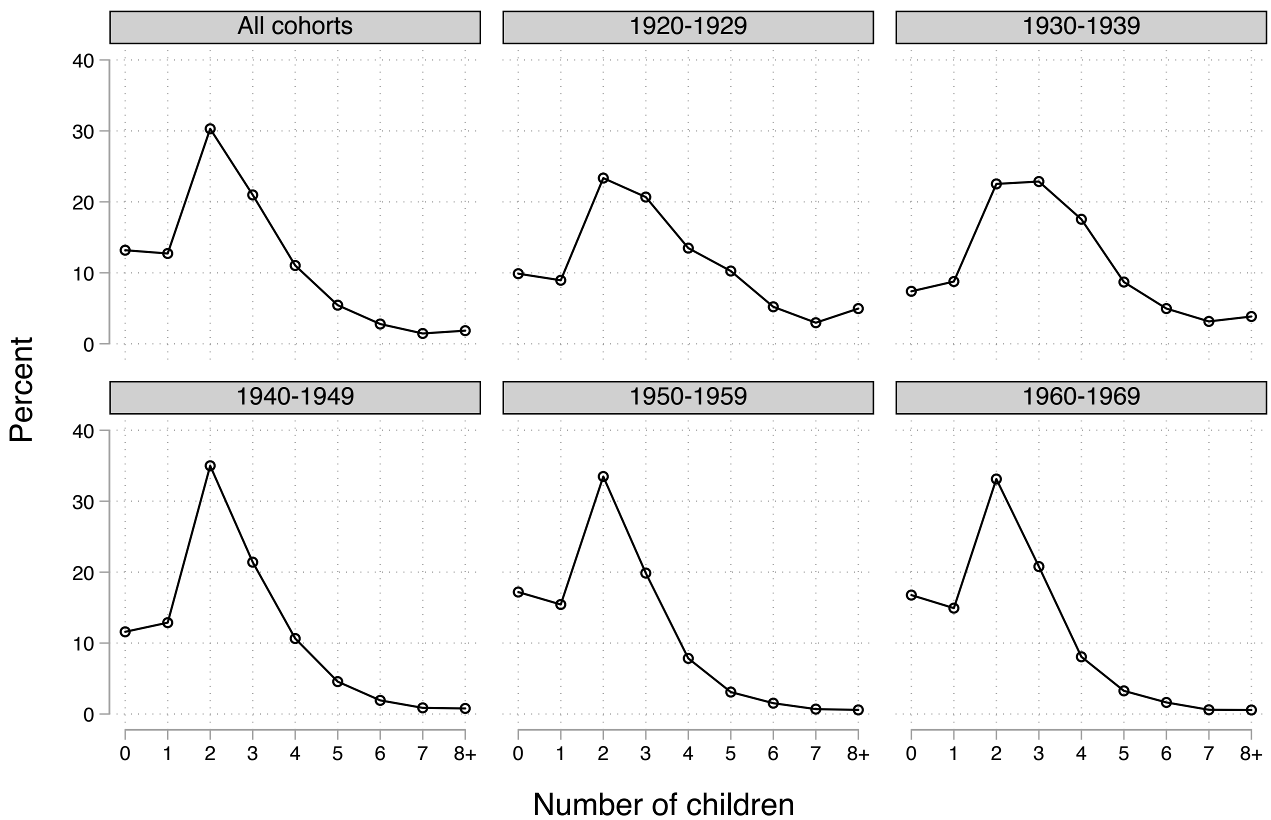

Fig. S12. Histograms for number of children, adults 40-60, overall and by decade of birth, GSS. Uses all GSS waves from 1972 to 2018. Full sample includes individuals born from 1912 to 1978; cohort-specific results are provided only for decades with full coverage.

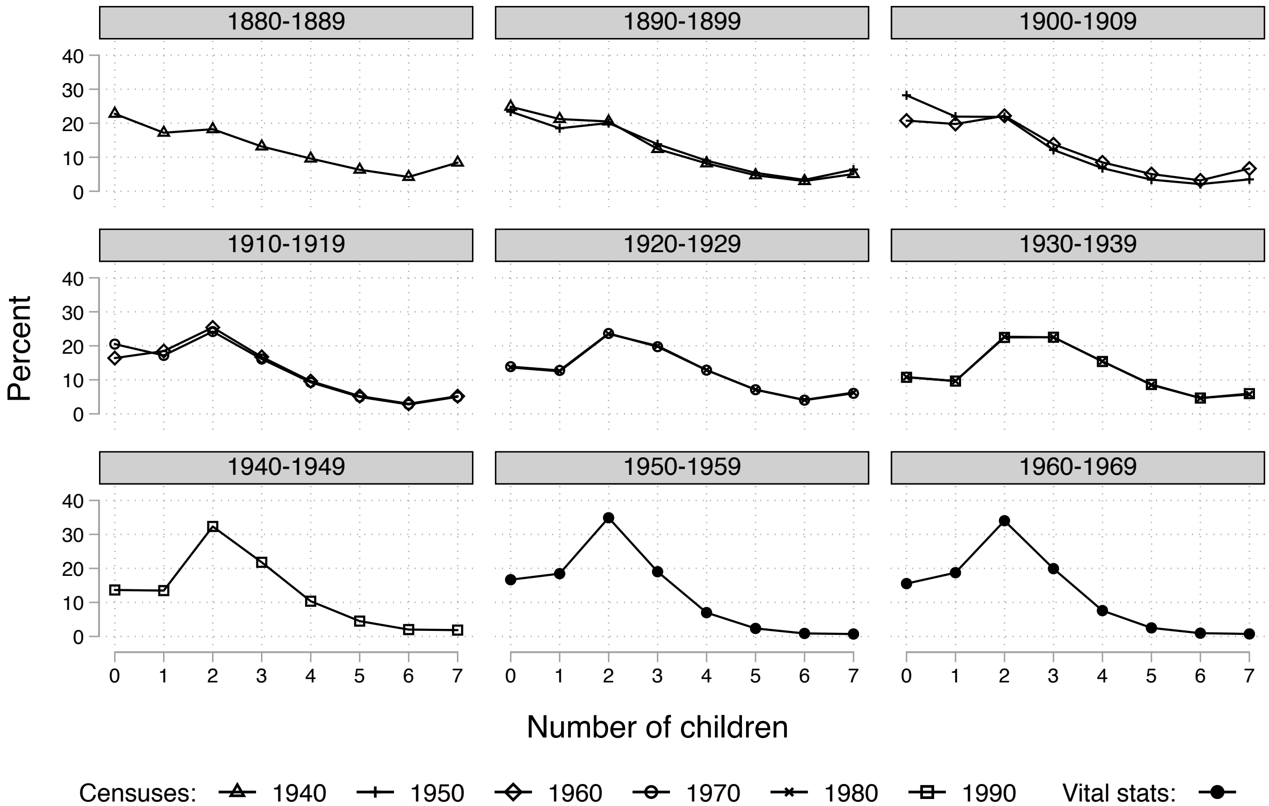

Fig. S13. Histograms for number of children by decade of birth, women only, census and vital statistics data. Uses data on children ever born among women ages 40-60 in IPUMS microdata samples from the 1940-1990 US Censuses, and cohort parity proportions at age 40 from the National Vital Statistics System. Later censuses did not collect data on children ever born, and parity proportions at age 40 are not available for earlier cohorts in the vital statistics system. Proportions are first calculated by the woman's year of birth and then averaged across years of birth within the decade of birth.

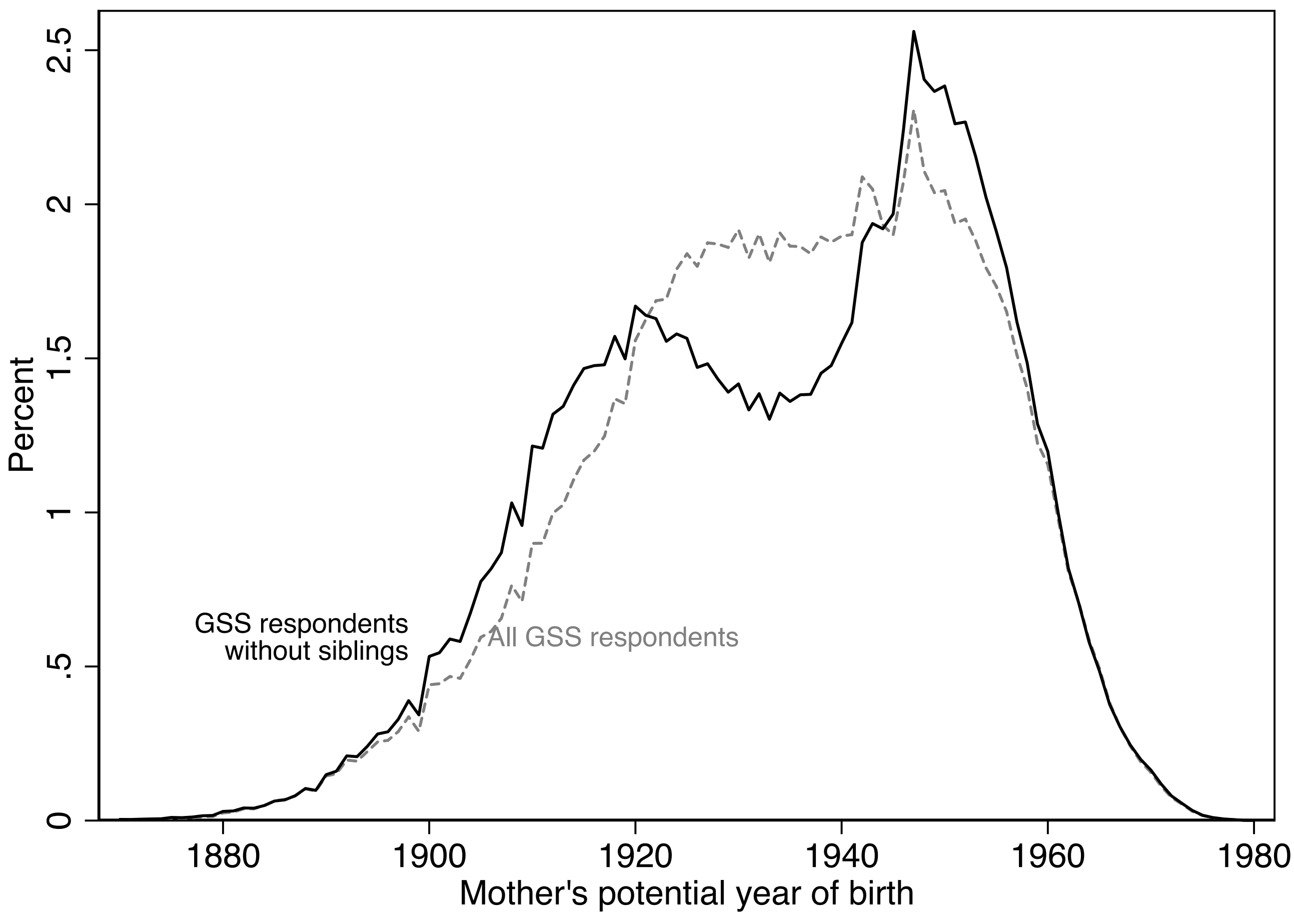

Fig. S14. Histogram of mother’s potential year of birth. Uses data on children 10 and under in every US Census from 1920 to 2000. For each birth year among children, we calculate the distribution of mothers’ birth years. We then aggregate these conditional distributions, weighting by the distribution of birth years of GSS respondents in the main sample. For respondents without siblings, the 5th percentile is 1901, the 95th percentile is 1960, and the interquartile range is 1918-1948. For all respondents, the 5th percentile 1903, the 95th percentile is 1960, and the interquartile range is 1922-1949.

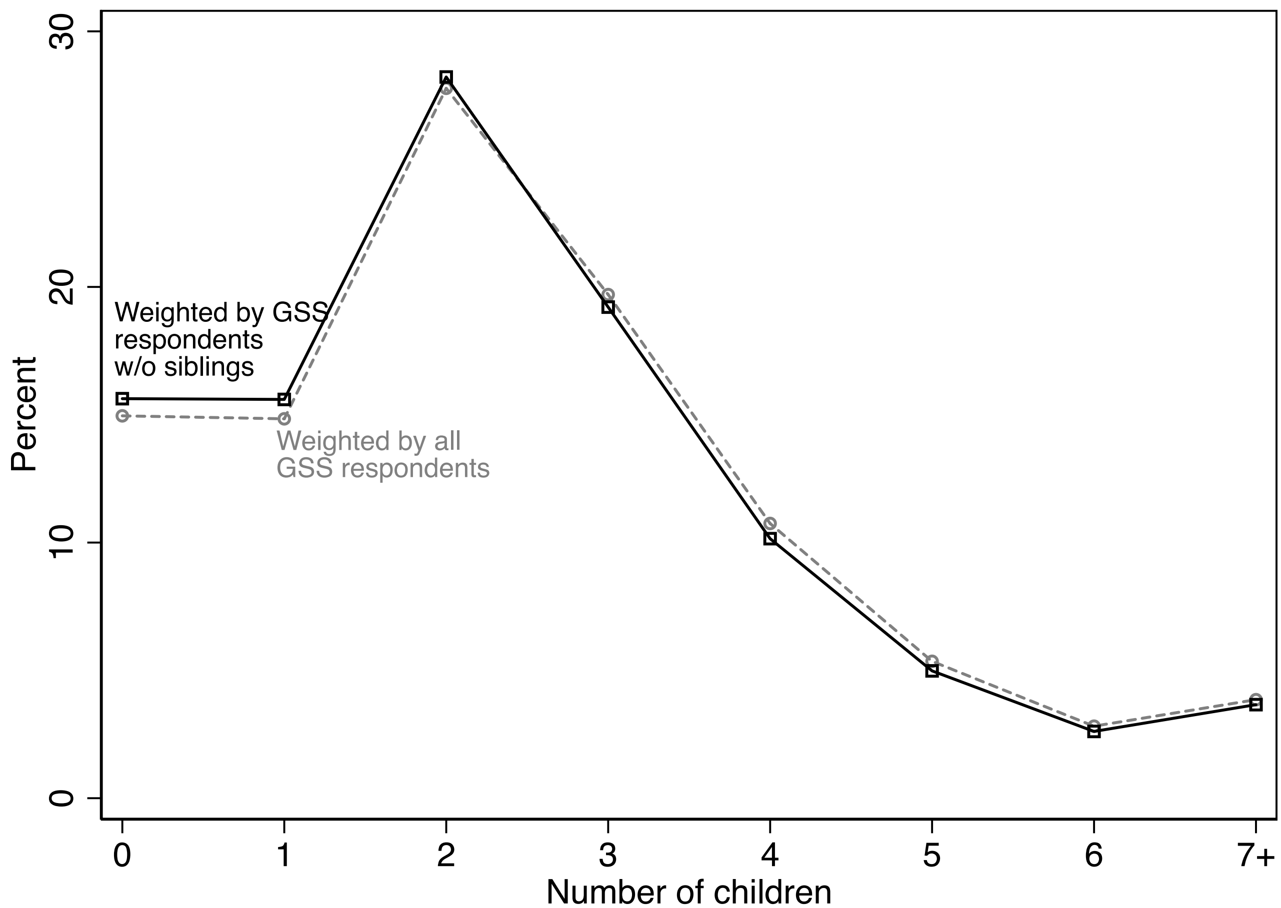

Fig. S15. Histogram for number of children, weighted by mother's potential year of birth. Average of birth year-specific histograms of children ever born from census and vital statistics, weighted by the distribution of mother's potential year of birth in the GSS. When histograms are available from multiple censuses, we take their average, weighting by the number of observations on which they are based. See Fig. S9 for more information on the children ever born histograms and SI Fig. S10 for more information on mother's potential year of birth.

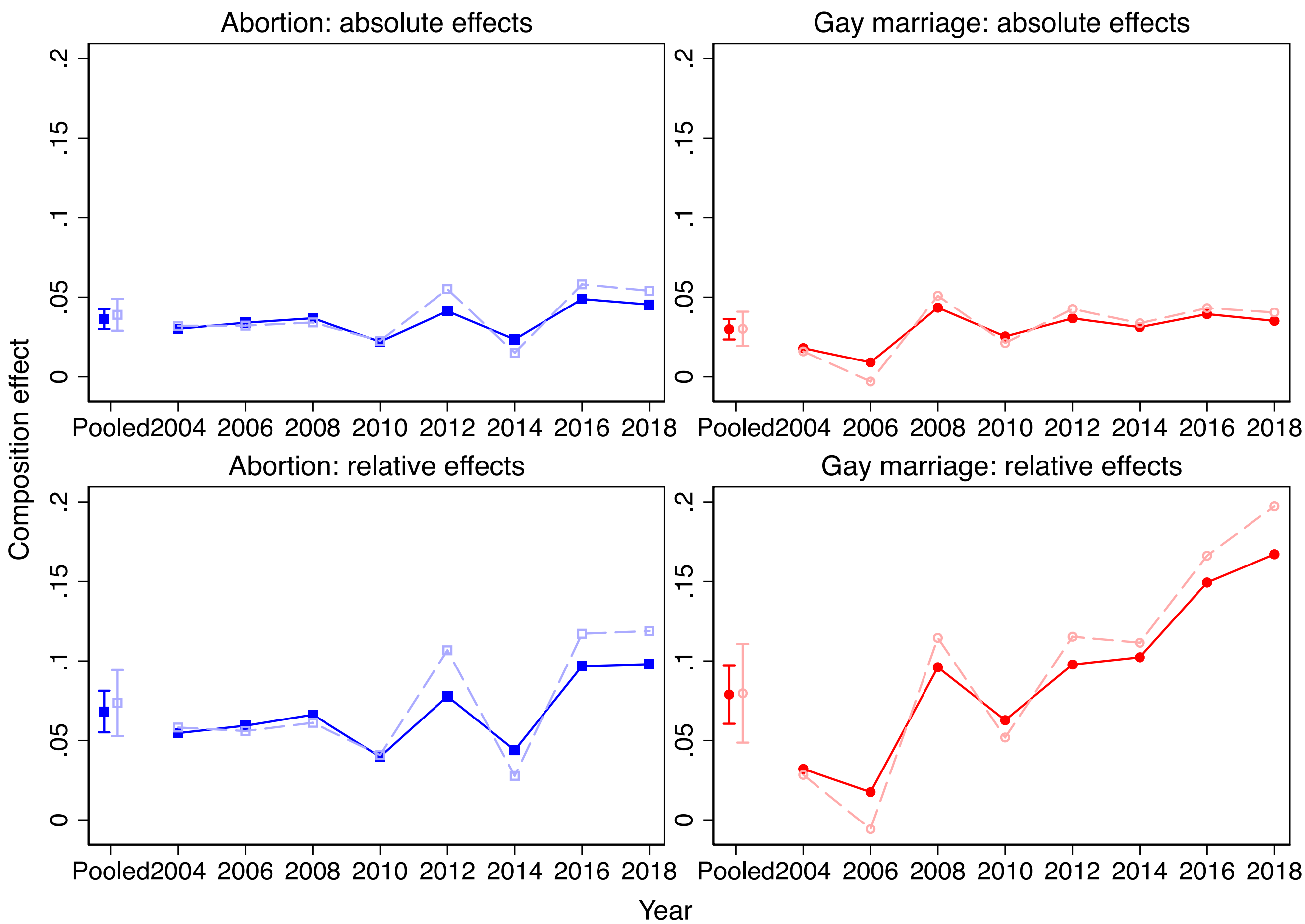

Fig. S16. Baseline composition effect (solid) versus lower bound on composition effect accounting for childlessness (dashed).The absolute composition effect is the actual prevalence of opposition minus deweighted prevalence; the relative composition effect is the absolute composition effect divided by deweighted prevalence. Capped spikes are 95% confidence intervals based on bootstrapped standard errors. The figure compares the baseline composition effect from Fig. 4, which omits the potential children of the childless, with a lower bound on the composition effect that includes the potential children of the childless. Based on Figs. S8, S9, and S11, we assume that these potential individuals have the same population share as only children. Based on Fig. 1, we assume that they would be no more opposed to abortion and same-sex marriage than are only children.

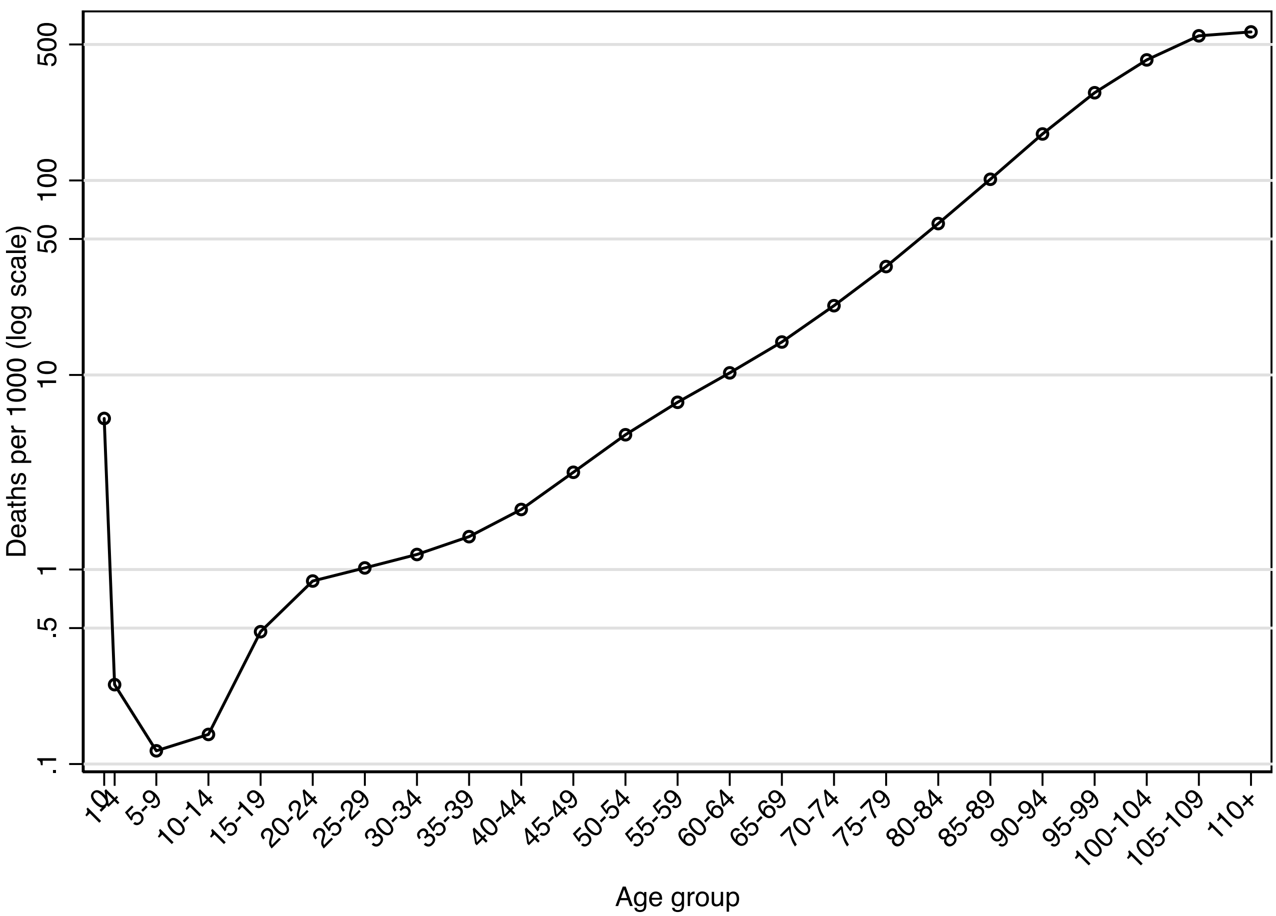

Fig. S17. Age-specific mortality rates for the United States, 2010-2016. Data from the Human Mortality Database.

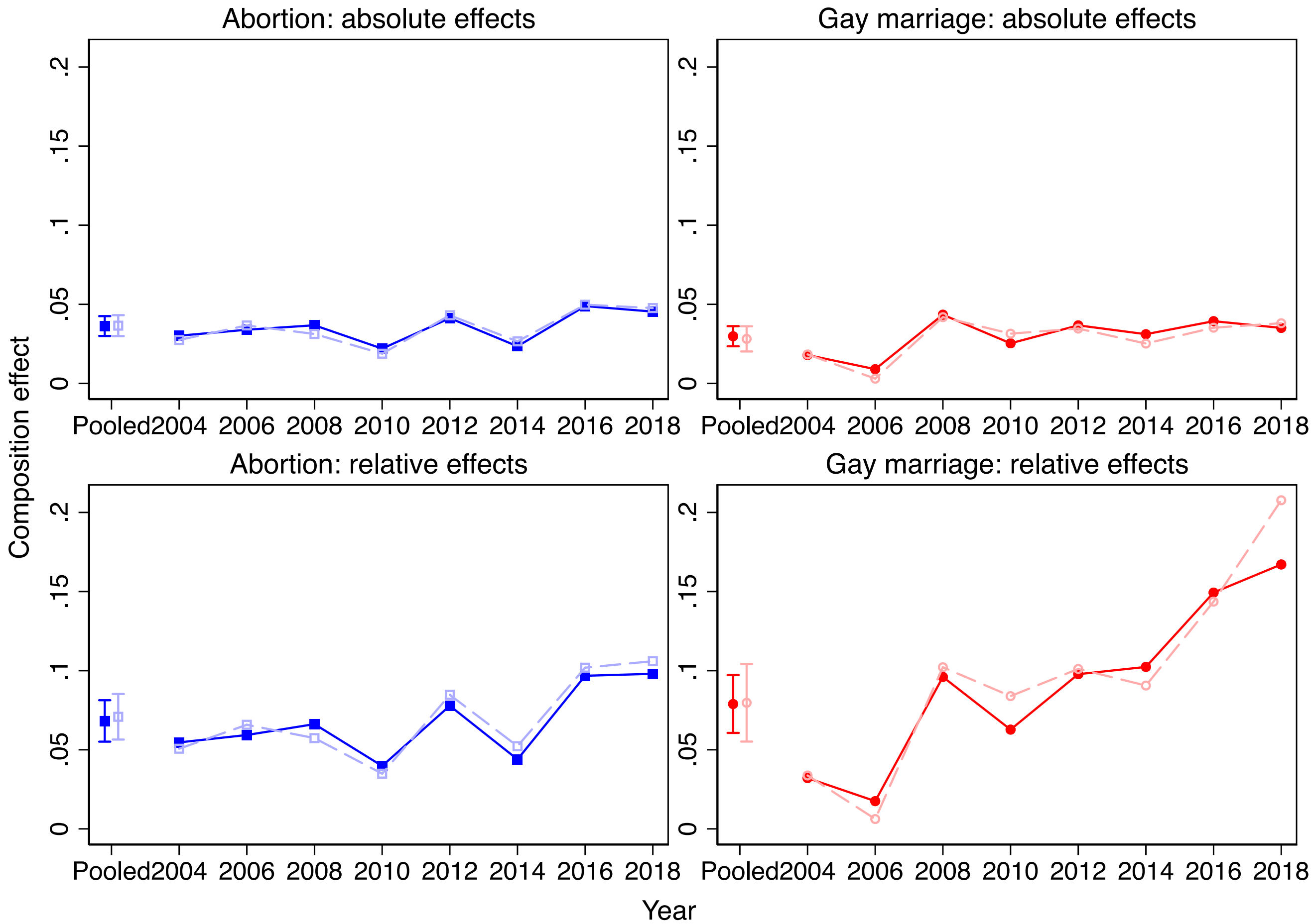

Fig. S18. Baseline composition effect (solid) versus under-65 composition effect (dashed). The absolute composition effect is the actual prevalence of opposition minus deweighted prevalence; the relative composition effect is the absolute composition effect divided by deweighted prevalence. Capped spikes are 95% confidence intervals based on bootstrapped standard errors. The figure compares the baseline composition effect from Fig. 4 with the composition effect for individuals under age 65.

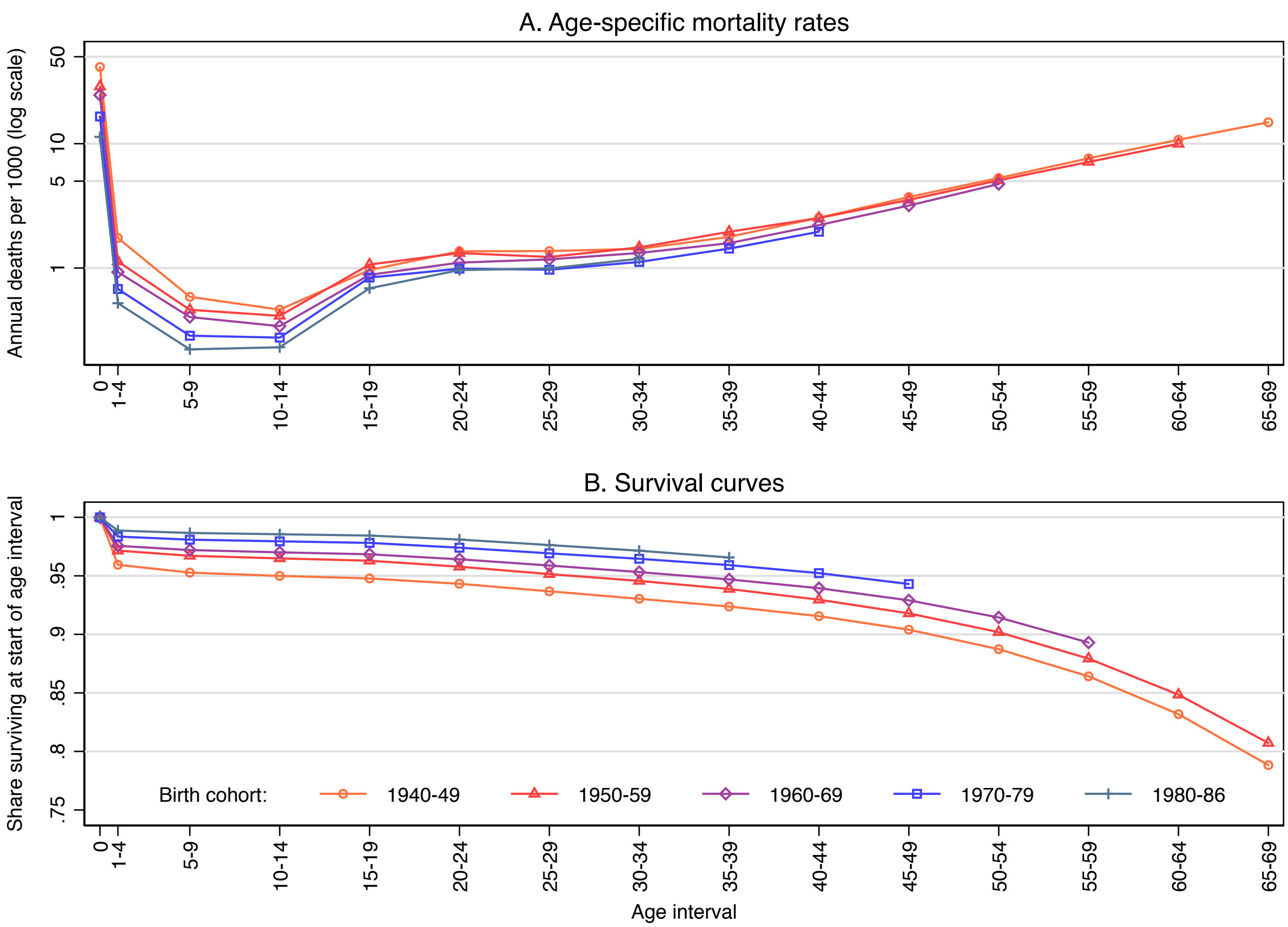

Fig. S19. Cohort age-specific mortality rates and survival curves for the United States. Data from the Human Mortality Database.

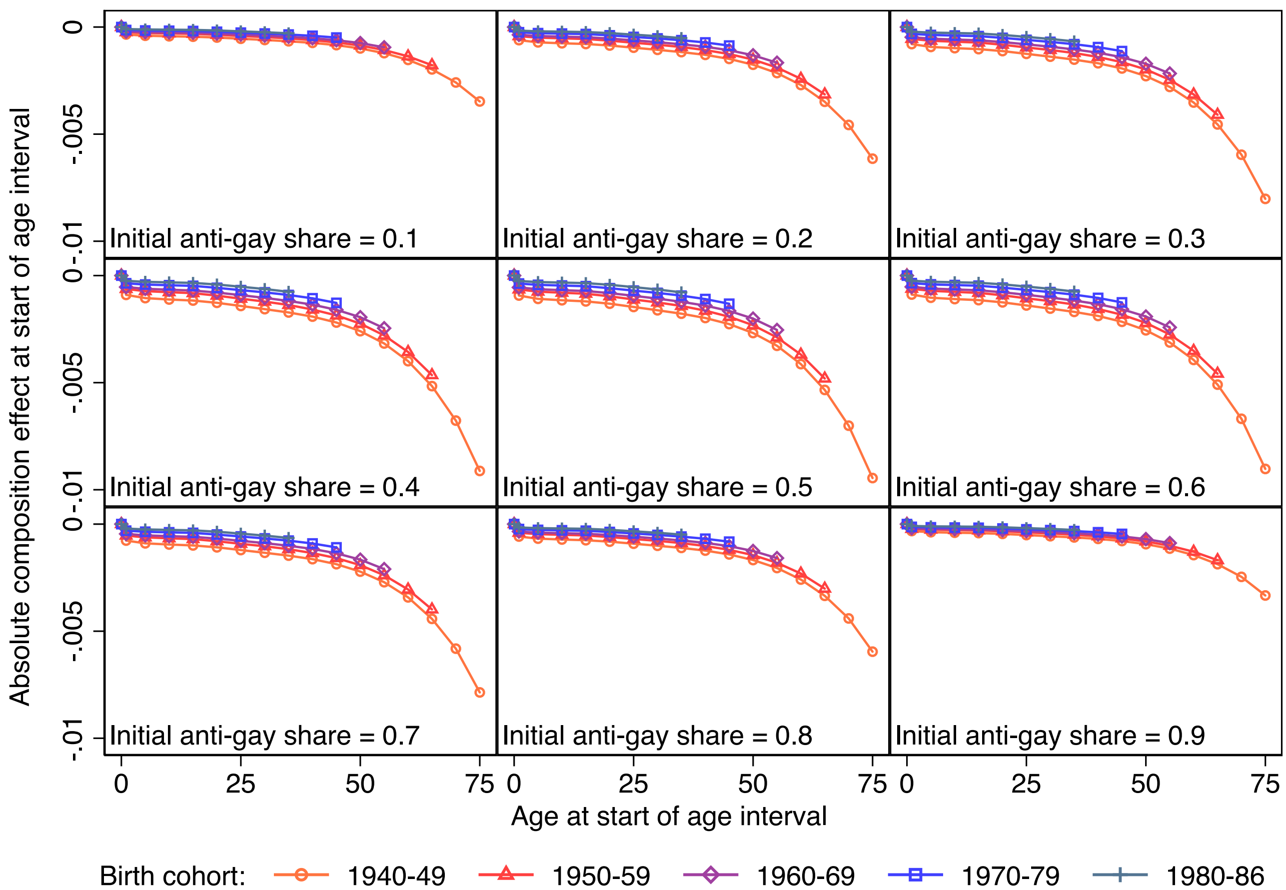

Fig. S20. Simulating the composition effect of differential mortality on the prevalence of anti-gay sentiment. Assumes a 9.4% higher mortality hazard for anti-gay individuals and an overall cohort survival curve as documented in Fig. S15. Detailed age intervals appear on the horizontal axis in Fig. S15.

	Abortion		Homosexuality		Marriage
	(1)	(2)	(3)	(4)	(5)
Opposed	1.035	0.930	1.094**	1.117	1.119
	[0.026]	[0.103]	[0.034]	[0.129]	[0.122]
Survey yrs.	1978-2010	2004-2010	1980-2010	2004-2010	2004-2010
Mortality follow-up yrs.	1979-2014	2005-2014	1981-2014	2005-2014	2005-2014
Observations	25,732	4,802	22,880	4,753	5,206

Table S1. Association between traditional-family conservatism and mortality. Annual hazard ratios from Cox proportional hazard models, with heteroskedasticity robust standard errors in brackets. All models include indicators for 5-year birth cohort and for 5-year age group at the time of the survey. Mortality data are from Muennig et al.'s (6) linkage of the 1978-2010 General Social Surveys with the National Death Index through 2014. Respondents who died in the survey year are dropped. Columns 2, 4, and 5 analyze survey waves included in the main analysis sample. Columns 1 and 3 use all available NDI-linked waves with questions about opposition to abortion or homosexuality. Only one pre-2004 survey wave included a same-sex marriage question, so we do not provide results from the extended sample for this attitude. * $p < 0.05$, ** $p < 0.01$.

References

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