The Effect of Birth Order on Educational Attainment among the Baby Boom Generation

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Abstract: We show that changes in birth order during the baby boom can explain a substantial share of the stagnation and recovery in educational attainment among cohorts born between 1946 and 1974. Combining birth order effects estimated using the Health and Retirement Survey and birth order data from Vital Statistics, we estimate that changes in birth order can explain more than 20 percent of the decline in white male college completion rates among the 1946–1960 cohorts, and more than one third of the rebound among the 1960–1974 cohorts. We also revisit the role of cohort size, finding smaller effects than previously reported.

Keywords: baby boom, birth order, educational attainment, cohort size

JEL codes: J13, I20, N32

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I. Introduction

High school and college completion rates abruptly stagnated among cohorts born in the late 1940s and early 1950s. This was an unexpected reversal of trends, as high school completion had doubled in the previous 30 years (Goldin and Katz, 2008) and college attainment had doubled in just 15 years (Acemoglu and Autor, 2012). But across cohorts born in the 1950s, high school completion rates did not change, and college completion rates fell. Educational attainment then resumed a more gradual rise among the 1960s birth cohorts.

These changes in educational attainment had important consequences for individuals (e.g., lower income) and for society as a whole (e.g., increased inequality and slower economic growth), and were surprising given that the decline began at a time when the college premium was increasing.[1] Previous research has attributed some of this reversal in trends to an abnormally high college completion rate for men born in the 1940s due to the Vietnam War (Angrist and Chen, 2011; Card and Lemieux, 2001). However, the estimated effects of the war are much smaller than the observed decline in men's college completion rates, and the abrupt halt in the growth of educational attainment remains largely unexplained (Goldin and Katz, 2008).

We add a new explanation for the changes in educational attainment among these cohorts. During the baby boom that followed World War II, the distribution of birth order changed dramatically, with first and second births becoming less common, and a far higher percentage of children being third-born or later. Because later-born children tend to have lower average educational attainment (Black et al., 2005; Kantarevic and Mechoulan, 2006), this change in birth order would tend to decrease high school and college completion rates.

Figure 1 illustrates our hypothesis.[2] The solid line shows college attainment among white men by birth cohort, which we measure using data from decennial censuses and the American Community Survey. The abrupt decline in college completion in this group begins with the late 1940s cohorts and does not reverse course

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[1] Autor, Goldin, and Katz (2020) estimate that the college premium increased by more than 50 percent between 1950 and 1970, declined slightly in the 1970s, then increased steadily after 1980. College completion peaked among the late 1940s birth cohorts, who would have turned 18 while the college premium was still increasing in the mid-1960s.

[2] Versions of Figure 1 for white women, nonwhite men, and nonwhite women are presented in Appendix Figures A1–A3.

until the late 1950s. The dashed line shows changes in birth order across the baby boom generation, using data from Vital Statistics. The share of births third-born or higher increased from just over 30 percent in the late 1940s to almost 50 percent around 1960.[3]

We use the Health and Retirement Survey (HRS) to estimate the effect of birth order on educational attainment for the baby boom generation. To our knowledge, the HRS is by far the largest available nationally representative survey in the U.S. that includes data on completed education for groups of siblings. We use this sibling data to estimate family fixed effects regressions of educational attainment on birth order indicators and cohort fixed effects. We find that birth order effects can be large. For example, among whites, third-born children are 8 percentage points less likely than first-borns to graduate from college. These results echo a large literature on the link between birth order and education (e.g., Black et al., 2005), but we believe we are the first to estimate birth order effects on college completion in the U.S.

We also use data from decennial censuses and the American Community Survey (ACS) to estimate the effect of cohort size on educational attainment. Previous research, including Card and Lemieux (2000) and Bound and Turner (2007), have found that larger cohorts are associated with lower educational attainment in regressions that include state and cohort fixed effects. But the composition of parents may change as fertility rates change — for example, we find that state-level changes in fertility are strongly negatively correlated with changes in parents' education — so we go beyond these previous papers and control for state-by-cohort measures of parents' characteristics, including race, education, age, and income. We find that the inclusion of these controls substantially reduces the estimated effect of cohort size on college completion for most groups, suggesting that some of the estimated cohort size effects in prior literature may be overstated.

Finally, we use our estimates of birth order and cohort size effects to study the effect of the baby boom on changes in educational attainment over time. We multiply our estimates by national changes in cohort size and the distribution of birth order, measured from Vital Statistics published tables and birth-level records, to construct counterfactual series of educational attainment holding birth order or cohort size constant. Our results are particularly effective in explaining the changes in college

[3] If we instead use the share of births fourth-born or higher, or fifth-born or higher, the figure looks very similar, and the inverse relationship between birth order and college completion is still clearly evident.

completion for white men: the baby boom explains more than one third of the 6.3 percentage point decrease in college completion across the 1946–1960 cohorts, and the end of the boom explains more than half of the 5.9 percentage point rebound in college completion across the 1960–1974 cohorts. Birth order is more important than cohort size in accounting for these changes. We also study the effects of birth order and cohort size on changes in high school attainment. In general, we find that birth order and cohort size effects are smaller for high school than for college completion. Consequently, the baby boom is not as important in explaining changes in high school completion as it is in explaining changes in college completion.

II. Background

a. The baby boom

The baby boom was a dramatic change in fertility patterns in the U.S. The fertility rate had been gradually declining for a century until about 1940 (Jones and Tertilt, 2006), when it abruptly increased for nearly twenty years. Figure 2 shows white and nonwhite fertility rates in the United States from 1909 to 2000. The fertility rate suddenly increased for both whites and nonwhites in the 1940s, and peaked around 1960. This substantial reversal of long-term fertility trends was concurrent with major social changes, including increasing educational and labor market opportunities for a generation of young veterans, changing urbanization patterns, and sharply altered labor market prospects for women (Klein, 2005). Women married earlier and at higher rates, had more children, and spaced children closer together (Bailey and Collins, 2011). Fertility rates increased during this period across income levels, racial and ethnic groups, and geographic regions, as well as in both urban and rural areas (Jones and Tertilt, 2006).

There is a rich literature exploring the causes of the baby boom. One early theory is Easterlin's (1976) "relative income" hypothesis, in which fertility is positively related to the gap between realized material resources and material aspirations that were formed during childhood. Easterlin postulated that those born just before and during the Great Depression formed low aspirations of material well-being, which where then greatly exceeded because of strong economic growth following World War II, leading to an increase in fertility.

Another strand of the literature focuses on the role of wealth and price effects in shaping the demand for children. Butz and Ward (1979) attribute the increase in fertility in the 1950s to rising income for men, and the baby bust of the 1960s to better labor market opportunities for women. Schaller (2016), studying a later time period, finds a similar pattern of sex-specific effects of labor demand on fertility. Jones and Schoonbroodt (2016) argue that the large negative income shock of the Great Depression caused a contemporaneous decline in fertility, followed a generation later by a baby boom.

Previous work has also explored a variety of other mechanisms. Greenwood et al. (2005) hypothesize that productivity growth in the household sector played a large role in the baby boom by lowering the cost of having children. However, Bailey and Collins (2011) use county-level data on electricity and appliance ownership to show that the baby boom did not occur earlier in places that got electricity and adopted household appliances more quickly. Doepke, Hazan, and Maoz (2015) focus on the role of the positive shocks to demand for female labor during World War II. The war effort created a persistent increase in female labor force participation, which they argue crowded out younger cohorts of women, who then tended to marry earlier and have more children. Zhao (2014) argues that increases in marginal tax rates after WWII decreased the cost of having children, particularly for wealthy households, contributing to the rise in fertility. Hill (2014) posits that the expansion of the housing supply after WWII lowered the cost of marriage and children, accounting for up to 10 percent of the baby boom.

In our main results, we treat the baby boom generation as beginning with the 1946 birth cohort and ending with the 1974 birth cohort. Some authors have treated the baby boom as starting as early as 1939 (e.g., Bailey and Collins, 2011), a local minimum in the fertility rate. However, our goal is to explain changes in educational attainment, and the reversal in longstanding trends in college completion only begins with the cohorts born after the war. In the appendix, we show that our estimated birth order estimates are not sensitive to defining the baby boom generation as the 1939–1974 cohorts instead of 1946–1974. We use the 1974 birth cohort as the end of the baby boom generation because, as Figure 2 shows, after the mid-1970s, fertility rates overall and among whites were very stable for a quarter century.

b. Birth order and educational attainment

Within families, later-born children typically have lower educational attainment than earlier-born children. Using data from Norway, Black et al. (2005) find substantial birth order effects: relative to the first-born child, second-, third-, and fourth-born children complete, respectively, 0.34, 0.52, and 0.61 fewer years of education on average. Subsequent work has found similar patterns in other countries, including Britain (Booth and Kee, 2009) and Denmark (Bagger et al., 2013).

In the U.S., Kantarevic and Mechoulan (2006) use the Panel Study of Income Dynamics (PSID) to show that first-born children have higher levels of educational attainment than later-born children, but their estimates for later-born children are imprecise due to the small sample size. De Haan (2010), using the Wisconsin Longitudinal Study, finds the effects of birth order to be approximately linear, such that second-born children complete approximately 0.3 fewer years of schooling than first-born children, and so on. Neither of these papers studies college completion as an outcome of interest, which is our focus in this paper, but our estimates of birth order effects on high school completion and years of schooling are similar to these previous papers. Compared to these earlier papers, the HRS data we use includes far more observations than Kantarevic and Mechoulan (2006) report in the PSID, and the HRS has the advantage of being nationally representative, unlike the state-specific survey used by De Haan (2010).

While we are narrowly interested in educational attainment in this paper, birth order has been shown to have important effects on a number of other outcomes as well, including cognitive test scores in childhood (Hotz and Patano, 2015; Lehmann et al., 2016), IQ (Black et al., 2011), health (Black et al., 2016), delinquency (Breining et al., 2020), and earnings (Kantarevic and Mechoulan, 2006).

There is a small literature on the sources of birth order effects, much of which uses U.S. data and focuses on the role of parental attitudes and time investments. Price (2008) finds that parents devote equal quality time to each child at any given point in time and that parents decrease total quality time as their children get older, so that earlier-born children accumulate more quality time. On the other hand, Monfardini and See (2016) find little evidence of differences across siblings in parent time investment. Lehmann et al. (2016) find that differences across siblings in home environment scores — based on factors like whether the mother reads to the child, parental attention, discipline patterns, and so on — can explain most of the effect of birth order on cognitive assessment scores in early childhood. Hotz and Pantano (2015) find that parents are stricter with earlier-born children, potentially leading to better outcomes.

In addition to differences in parent time investments, older siblings may get more financial investment. Conley and Glauber (2006) find that an increase in family size decreases the likelihood that second-born boys attend private school, but has no effect on first-born boys. Black et al. (2011), using Norwegian data, find limited evidence that selection plays a role; parents may stop childbearing after having a particularly "poor quality" child. Of course, children may also have an impact on their siblings. For example, older siblings could benefit from teaching younger siblings.

c. Cohort size and educational attainment

The size of one's birth cohort may affect educational attainment through a variety of mechanisms including larger class sizes in primary and secondary school, less than perfect elasticity of supply in higher education in the short run, and the anticipation of labor market effects such as crowding or thick market externalities. Stapleton and Young (1988) hypothesize the substitutability of workers across experience groups may decline with education, so that a large cohort of young workers reduces the wages of young college graduates more than young non-college graduates. In anticipation of this, members of larger cohorts may be less likely to complete a college degree.

Card and Lemieux (2000) use state-level variation in cohort size to estimate that a 10 percent increase in cohort size reduced college completion among the 1940–1964 birth cohorts by about half a percentage point. Bound and Turner (2007), using a similar empirical strategy, estimate a comparable effect. We find similar estimates in specifications that include state trends, but the effects decrease substantially when we add controls for parents' characteristics and the distribution of birth order, suggesting that previous estimates of cohort size effects are overstated.

III. Data

a. Census data on educational attainment

We use individual-level data for 24–65 year olds from the 1960–2000 censuses and the 2006–2017 ACS to estimate the high school and college completion rates for each birth cohort at the state-race-sex level. We use these estimates below to assess how much of the change in educational attainment across cohorts can be explained by changes in the distribution of birth order and by changes in cohort size.

Because of the age range we use, we observe a given birth cohort across multiple censuses, and reported educational attainment in a birth cohort can change over the life cycle. Following Card and Lemieux (2000), we adjust the observed educational attainment by regressing attainment at the state-cohort-census level on state-by-cohort fixed effects, a cubic in age (to account for changes in reported education over the life cycle), and an indicator for observations before 1990, when the census education classification was changed from years of schooling to degree attainment. For college attainment, for cohort c born in state s and observed in year t, this model is

$$\text{college}_{sct} = \gamma_{sc} + f(\text{age}_t) + \delta\, 1(t < 1990) + \varepsilon_{sct}\,. \tag{1}$$

Our estimate of college completion for cohort c born in state s is the predicted attainment at age 40 according to the new census education question. We estimate college completion separately for white men, white women, nonwhite men, and nonwhite women, and we repeat the process for high school completion for each of the four demographic groups. Throughout the paper, we use the terms "completion" and "attainment" interchangeably.

Appendix Figure A4 presents our estimates of the percentage of men and women in each birth cohort between 1930 and 1985 who graduated from college. For all four race-sex groups, college completion fell noticeably below trend beginning with the late 1940s birth cohorts. Among white men, college attainment peaked in 1948 at 34.7 percent before declining to 27.8 percent in 1960. The college completion rate did not return to its 1948 level until 1979. College attainment for white women experienced a shorter and smaller decline between 1951 and 1956, then increased much more quickly than for white men over the next two decades. For nonwhite men, the decline was smaller than for white men in percentage points, but similar as a percentage drop. College completion for nonwhite women never decreased, but it did stagnate at the same time that college completion for white women was declining.

b. Vital Statistics data on birth order

For information on birth order and cohort size, we use 1930–1980 natality data from Vital Statistics, measured at the state-race-year level. We collected data for birth cohorts 1930–1967 from published summary tables, and we tabulated data for birth cohorts 1968–1980 from the individual-level birth data. From these sources, we collect information on the total number of births (by mother's state of residence) and the percentage of total births that are first births, second births, and so on.[4]

Figure 3 presents changes in birth order during this period. The top graph plots the percentage of births that were first-borns, second-borns, etc. Here, one can see changes in fertility on the intensive and extensive margins. At the beginning of the boom in the 1940s, there was initially a spike in the percentage of first births as more women were beginning to have children. Shortly after, the percentage of first births declined and the percentage of higher-order births began to rise as these women had additional children. Around 1960, as the fertility rate started to decrease, the percentage of first births slowly started to increase and higher-order births started to decline. By the late 1960s, first and second births became more prevalent as the two-child household became more common.

The bottom graph shows a different view of the baby boom. Here, we plot the percentage of births that were third births or higher. This shows a decline in higher-order births in the 1930s, a quick spike around World War II, and then a longer, steady increase from the late 1940s until the early 1960s. The same pattern can be seen for whites and nonwhites, and the racial gap declines beginning in the 1960s.

c. Health and Retirement Survey data on siblings

Our estimates of the effect of birth order on educational attainment come from the Health and Retirement Survey (HRS). The HRS is a nationally representative longitudinal survey of individuals over age 50, and we believe it offers the largest available sample of groups of siblings with data on completed education. The survey began in 1992 with an initial cohort of respondents born 1931–41, and younger cohorts have been added every six years. The survey now includes more than 42,000 respondents. Respondents and their spouses are interviewed every two years, from the time of entry into the survey until death. The HRS collects data on a wide variety of topics, including respondents' health, cognition, family structure, income, assets, and

[4] The share of births with birth order not reported averages 2.6 percent, with a maximum of 5.7 percent in 1945. We drop these births from our sample when measuring the birth order distribution for each birth cohort. All results are robust to allocating birth order not reported cases among birth orders.

employment. The survey also asks respondents about their siblings, and we use data on respondents' and siblings' age, sex, and education to estimate the effects of birth order using a family fixed effects design.[5]

We report summary statistics in Table 1. We have data for 42,106 siblings (including respondents in this count) from 16,469 respondents who reported any siblings, and this makes up our sample for the birth order regressions. The range of birth cohorts is large, for a variety of reasons: respondents of course have older and younger siblings, successively later birth cohorts are added to the HRS over time, and HRS respondents may marry younger spouses and in some waves spouses were also asked about siblings. To facilitate a comparison between our sibling sample and census data, we also report summary statistics for the subset of siblings in our HRS data born during the baby boom generation, 1946–1974. For the census summary statistics, we use the 1946–1974 birth cohorts from the same data files described above in subsection (a).

We observe more women than men in the HRS (56 percent of HRS respondents are women), which is not surprising since women are more likely to survive to the age of eligibility for the HRS sample. Among the 1946–74 birth cohorts, our HRS sibling sample matches reported education in the census data remarkably well for whites. For nonwhites, high school completion and some college are somewhat higher in the census data, but college completion rates, our main outcome of interest, are very similar. Overall, we view this as a good match, particularly in light of the fact that the HRS sample is conditioned on respondents who reported any siblings. In unreported results, when we add HRS respondents who do not report siblings to the baby boom sample in Table 1, reported education matches the census estimates as well or better.

Comparing sibship size between the HRS and census is more difficult, because in the census, young children may have more siblings in the future, and older children may have siblings who have already left the household. However, through 1990 the census asked women how many children they ever had, and we use data for women 40–59 to measure average fertility by the mother's birth cohort, conditional on the woman having more than one child (recall that our HRS sample is limited to respondents who reported any siblings). In the census columns in Table 1, we report sibship sizes based on fertility among women born in 1930, because this is approximately when the mother of the typical person in our HRS baby boom sample would have been born. The estimated

[5] For more information about our construction of the sibling data, see the appendix.

sibship sizes are a little smaller in the census sample, although not greatly different. One possible explanation for this is that half-siblings with the same father are not being captured in the census data, but might be reported in the HRS.

IV. The effect of the baby boom on college completion

To estimate the effect of the baby boom on changes in college attainment, we first need to estimate the effects of birth order and cohort size on education. We estimate the effects of birth order using individual-level data from the Health and Retirement Survey, which allows us to observe the age and educational attainment of siblings. We then use Vital Statistics and census data to estimate the effect of cohort size on college completion rates. Finally, we take these estimates and apply them to national changes in birth order and cohort size for these cohorts. We explain these three estimation strategies and the results below.

a. The role of birth order

A major threat to the identification of birth order effects if using pooled data without family identifiers is that later born children are more likely to come from larger families, which differ in both observable and unobservable ways from smaller families. Following the existing literature on birth order, we address this by using our data on siblings to run regressions with family fixed effects. However, birth order effects may still be confounded by other factors. For example, as Figure 3 shows, any particular birth order is more common in some birth years than others, making it important to control for cohort effects. For person *i* in family *j* with birth order *b* (*b* = 1, 2, 3, 4, 5, 6 or above) and born in cohort *c*, we estimate the following family fixed effects model of college completion:

$$\text{college}_{ij} = \alpha + \sum_{b=2}^{6} \beta_b \, 1(\text{BO} = b) + \sum_{c} \gamma_c \, 1(\text{cohort} = c) + \mathbf{x}_{ij}'\delta + \lambda_j + \varepsilon_{ij} \, . \qquad (2)$$

In our preferred specification, birth orders 6 or higher are grouped into a single 6+ category. The vector of controls, $\mathbf{x}_{ij}$, includes a female indicator, an indicator for whether the individual is an HRS respondent, and the log cohort size, measured from Vital Statistics data, for the individual's year and census division of birth (we do not observe state of birth in the publicly available HRS data). We interact the birth order indicators with a non-baby boom indicator (individuals born before 1946 or after 1974) so that our sample includes all siblings, but the birth order coefficients we report apply specifically to the baby boom generation. In all birth order regressions, we cluster standard errors at the family level.

Our estimated birth order effects are presented in Table 2. In the first column, we estimate common effects for men and women. For whites, second-born children are about 5 percentage points less likely than first-borns to graduate from college, and the disadvantage relative to first-borns grows to 11 percentage points for children who are sixth-born or later. These are large effects, given that the college completion rate among whites in these birth cohorts hovers around 30 percent. We cannot directly compare these results to previous estimates in the birth order literature, because we are not aware of any other estimates of the effect of birth order on college completion in the U.S.[6] However, our results are roughly consistent with Booth and Kee (2009), who find that in Britain, university degree attainment is 14 percent for first-borns but less than 9 percent for middle children.

In the final two columns, we present separate estimates of birth order effects for men and women, estimated from a single regression in which we interact sex with the birth order indicators. Birth order generally matters more for white men than white women: compared to first-borns, third-born men are 9 percentage points less likely to graduate from college, whereas the corresponding difference for women is 6 percentage points. We can reject the null hypothesis that the birth order effects are the same for men and women at the 10 percent level ($p = 0.08$). The larger effects of birth order that we estimate for men are consistent with a broader literature finding that boys are more sensitive to disadvantage. Aucejo and James (2017), Autor et al. (2019), and Lundberg (2017) all find that boys' educational attainment is disproportionately affected by family background (e.g. family structure, resources, parental attention) compared to girls. Similarly, Chetty et al. (2016) finds that boys are more adversely affected by growing up in poor neighborhoods.

Results for nonwhites follow a similar pattern: later-born children tend to have progressively lower rates of college completion, and the effects are larger for men than for women. The estimated birth order effects are smaller for nonwhites than for whites.

[6] Kantarevic and Mechoulan (2006) study birth order effects in the U.S. on years of schooling and high school completion, but not college completion. We compare our results to this earlier paper in section V.

Part of this difference may be due to racial differences in educational attainment during this period. For the 1950 birth cohort, the college completion rate for white men and white women was 32.8 and 26.2 percent, respectively, compared to 14.8 and 14.5 percent for nonwhite men and women. The third-born coefficient for white men is consistent with a 29 percent decline in college completion from the average, while the coefficient for nonwhite men suggests a 34 percent decline.

Our birth order estimates come from our sibling dataset created from the HRS, and we made a variety of decisions in the data construction process that could have influenced our results. We test the sensitivity of our estimates to a number of alternative choices, such as limiting the sample to families with 10 or fewer children, limiting the sample to families with complete education and age data for all siblings, excluding individuals reported by siblings-in-law instead of siblings, and including reported siblings born more than 20 years before or after the respondent. Our estimates are robust to all of these choices. Our estimates are also robust to defining the beginning of the baby boom generation using the 1939 birth cohort instead of 1946. We describe these and other robustness checks in detail in the appendix, and our results are reported in Appendix Table A1.

b. The role of cohort size

In estimating the effect of cohort size on educational attainment, both Card and Lemieux (2000) and Bound and Turner (2007) regress college completion rates at the state-cohort level on log cohort size, state fixed effects, and cohort fixed effects. For state of birth s and cohort c, this yields the following model:

$$\text{college}_{sc} = \alpha + \beta \ln(\text{births}_{sc}) + \gamma_s + \delta_c + \varepsilon_{sc}\,. \quad (3)$$

Table 3 shows our estimates of the effect of cohort size on college attainment, estimated for the 1946–1974 birth cohorts. In all cohort size regressions, we cluster standard errors at the state level. In the most parsimonious specification in column 1, which includes state and cohort fixed effects, a 10 percent increase in cohort size is associated with a decrease in college completion of about 0.7 percentage points for whites and about 0.2 percentage points for nonwhites. These estimates increase slightly when we control for state-specific trends in column 2, as in Bound and Turner (2007). Our results in columns 1 and 2 are similar those reported by Card and Lemieux (2000) and Bound and Turner (2007), although an exact comparison is difficult because of slight changes in the range of birth cohorts used, and because neither of these previous papers estimates separate effects by race.

State and cohort effects, and even state trends, may be insufficient controls if the composition of parents changes within states over time. As an illustration of this threat, in census data we find that state-level changes in fertility are highly correlated with changes in parents' educational attainment. Figure 4 shows scatterplots of state-level changes in high school completion among white mothers and fathers between the 1946 and 1960 birth cohorts against state-level changes in log cohort size over the same period, with markers proportional to the state's 1946 cohort size. Here, we find a striking negative relationship between changes in cohort size and changes in parents' high school completion — the proportion of parents who completed high school is growing everywhere, but more slowly in states experiencing larger changes in cohort size.

Therefore, in column 3 of Table 3, we use our census data to add a number of controls measuring parents' characteristics at the state-cohort level. For both mothers and fathers, we include percent white, percent foreign-born, percent achieving each of three levels of education (9 years, high school, and college), and age at birth (percentiles 25, 50, and 75). We also include the percent of mothers who are married, mothers' age at marriage (percentiles 25, 50, and 75), the percent of fathers present in the household, and father's log earnings (percentiles 25, 50, and 75).[7] For all four race-sex groups, the point estimate of the cohort size effect is smaller once these controls are added. In column 4, we also include controls for the birth order distribution at the state-cohort level — the share first born, second born, etc. — measured from our Vital Stats data. When we do this, the estimated cohort size effect declines even further. With this full set of controls, we estimate that a 10 percent increase in cohort size reduces college completion by about 0.3 percentage points for white and nonwhite women, while for white and nonwhite men the effect is at most 0.1 percentage points and statistically insignificant.

c. Contribution of changes in birth order and cohort size to changes in college completion

We use the estimated birth order and cohort size coefficients above to assess the impact of the baby boom on college completion. To do this, we construct counterfactual series of college completion that hold birth order, cohort size, or both constant at their 1946 levels. For each year, we first compute the national change in birth order or cohort

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[7] For more details about our construction of these variables, see the appendix.

size since 1946, using our Vital Statistics data.[8] We then multiply these differences by our estimated birth order and cohort size coefficients, then subtract the result from the actual value for the year to obtain the counterfactual value. We believe these estimates isolate the separate effects of birth order and cohort size. Although data requirements force us to estimate the effects of birth order and cohort size from two different sources, our birth order regressions control for cohort size, and our cohort size regressions control for the distribution of birth order. Both estimates apply to the generation born 1946–1974: our cohort size regressions include only these birth cohorts in the regression sample, and our birth order regressions interact the birth order indicators with an indicator for being born before 1946 or after 1974, so that the results we use here apply specifically to the baby boom generation.

Figure 5 plots the actual and counterfactual college completion rates for all four race-sex groups. By construction, the counterfactual series coincide with observed college completion in 1946, our chosen base year. Choosing a different base year would shift the counterfactual series up or down, but would not affect the year-to-year changes in these series. For white men, college completion decreased more than six percentage points between the late 1940s and late 1950s birth cohorts, before rebounding during the 1960s. The counterfactual series in which birth order is held constant decreases noticeably less, indicating that birth order can explain an important share of the changes in college completion among this generation. We also plot a counterfactual series that holds constant both birth order and cohort size, and the difference between this series and the series that holds constant only birth order can be viewed as a measure of the explanatory power of changes in cohort size. For white men, cohort size contributes modestly to explaining the changes in college completion.

The other race-sex groups did not experience the large decline in college completion we see among white men, but for each of these other groups, college completion was well below trend for the 1950s and 1960s birth cohorts, stagnating or declining early in the baby boom generation before catching up later in the generation. In each case, the counterfactual series do not fall as far below trend as the actual series, indicating that birth order and cohort size explain some of the changes in college completion.

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[8] Birth order distributions and cohort sizes are reported by birth year in Appendix Table A2. For computing cohort size effects, we use the weighted average of state-specific values of log births.

In Appendix Table A3, we quantify the effects of birth order and cohort size by race-sex group. We split the baby boom generation into two periods: 1946–1960, during which fertility and average birth order were increasing (see Figures 2 and 3) and college completion was falling below trend, and 1960–1974, during which fertility and average birth order were decreasing and college completion was accelerating. For white men, college completion fell by 6.3 percentage points across the 1946–1960 cohorts. Comparing changes in the actual and counterfactual college completion rates for white men in 1946 and 1960, we find that changes in birth order can explain 1.4 percentage points of this decline, or about 22 percent. This effect of 1.4 percentage points can be seen on the plot in Figure 5: it is the difference between the 1946–1960 change in actual college completion and the change in in the counterfactual series that holds constant the distribution of birth order. For the 1960–1974 birth cohorts, college completion among white men rose 5.9 percentage points, and changes in birth order can explain 2.1 percentage points of this increase. Cohort size plays a smaller role, explaining only 0.3 percentage points of the change in both periods.

For race-sex groups other than white men, the interpretation of the effects in Appendix Table A3 must be more subtle. Although white women and nonwhite men experienced decreases in college completion across the 1950s birth cohorts, and college completion stagnated across these cohorts for nonwhite women, each group had higher college completion at the end of the baby boom generation than at the beginning. Other forces, such as increasing labor market opportunities for women, likely played a large role in the longer-term dynamics of educational attainment, so we prefer to think of changes in birth order and cohort size as providing a drag on or a boost to these longer-term trends. For example, across the 1946–1960 birth cohorts, college completion rose by 4.4 percentage points among women, but our estimates suggest that changes in birth order prevented this increase from being 0.9 percentage points larger.[9]

We see at least two potential concerns in applying our estimated birth order effects to national changes in the birth order distribution. First, the birth order effects are

[9] We have also explored ways to estimate the proportion of the "lost" college completion during the baby boom generation that can be accounted for by changes in birth order and cohort size. But this necessarily involves specifying a hypothetical trend in college completion against which the slowdown can be judged, which is not something we feel confident doing. As a very rough approximation, if we assume a linear trend for each group's college completion between the 1946 and 1974 birth cohorts, and then compute the average per-cohort shortfall in college completion relative to this trend, we estimate that changes in birth order can explain 20–30 percent of the lost college completion for white men and white women.

necessarily estimated from sibling data, and it is natural to wonder if the household-level effects of birth order might give a misleading impression of the effects of a national change in the distribution of birth order. For example, when birth order changes nationally, this may correspond to changes in cohort size or changes in birth spacing, both of which could separately affect children's outcomes through mechanisms such as class sizes or parents' time investments. However, we do not view these as major threats. We control for cohort size in our birth order regressions, but our results are almost identical without this control. Also, the HRS is a nationally representative survey, so any aggregate changes in features such as birth spacing should be reflected in our sample, and therefore in our estimated birth order effects.

A second concern, related to the first, is that birth order effects may change over time. In unreported results, we allow birth order effects to differ between children born in the first and second halves of the baby boom generation. For all four race-sex groups, we cannot reject the null hypothesis that the birth order coefficients are equal in the earlier and later halves of this generation.

d. The role of birth order and cohort size in college attendance

We also estimate the effects of birth order and cohort size on completing at least one year of college, and report the results in the appendix. For brevity, we refer to completing at least one year of college as "some college" in these tables and figures. In Appendix Table A4, we find that for whites, birth order generally has an even larger effect on attending at least one year of college than on college completion. For white men, the point estimates on our birth order indicators are typically one third larger in our some college regressions than in our college completion regressions, and for white women, the point estimates on third born and higher are about one quarter larger. The college and some college birth order results are more similar for nonwhites. Appendix Table A5 shows our estimated effects of cohort size. In our preferred specification with controls for parents' characteristics and the distribution of birth order, we find that a 10 percent increase in cohort size decreases some college by roughly 0.1–0.3 percentage points for each race-sex group, and none of these estimates are statistically distinguishable from zero.

In Appendix Figure A5, we plot actual and counterfactual trends in the completion of at least one year of college. As with college completion, most groups experienced at least some decline in completing at least one year of college, and the counterfactual trends indicate that changes in birth order and cohort size can help explain this phenomenon. Appendix Table A6 quantifies these counterfactual estimates, showing that changes in birth order across the 1946–1960 birth cohorts reduced completion of some college by 1–2 percentage points among whites and by about 0.5 percentage points among nonwhites, and boosted completion of some college by 1–3 percentage points across the 1960–1974 birth cohorts. Changes in cohort size generally play a much smaller role than changes in birth order.

V. The effect of the baby boom on high school completion

We also estimate the effect of the baby boom on high school completion for the baby boom generation. We first discuss our estimates of birth order effects and cohort size effects, then turn to the contribution of these two mechanisms to national changes in high school completion.

a. The role of birth order

Table 4 presents estimates of the effects of birth order on high school completion. The results indicate that earlier-born children are more likely to graduate from high school, but the magnitudes of the birth order effects are smaller for high school than for college. In the first column, we estimate common effects for men and women. The point estimates indicate that white second- and third-born children are about 2 percentage points less likely to complete high school than first-borns, but the deficit relative to first-borns stabilizes at 3–5 percentage points for children born fourth or later. In the final two columns, we present separate estimates of birth order effects for men and women, estimated from a single regression in which we interact sex with the birth order indicators. In contrast to the results for college completion, the point estimates indicate that birth order matters more for white women than for white men, but we cannot reject the null hypothesis that the effects are the same for men and women. For nonwhites, the estimated birth order effects are large for men (e.g., 9 p.p. for third-borns) but smaller and statistically insignificant for women.

These results are similar to the estimates in Kantarevic and Mechoulan (2006), which, to our knowledge, is the only other paper that estimates birth order effects on high school completion in the U.S. Kantarevic and Mechoulan (2006) use the PSID and report a difference in high school completion between first- and second-borns of about 3 percentage points in their family fixed effects regressions, although they do not estimate this effect by race and sex as we do. Like us, they find that the gap between first- and second-borns is larger than the gap between other pairs of adjacent birth orders. These authors do not report estimates of the effects of birth order on college attainment; to our knowledge, the estimates in the previous section are the first of their kind for U.S. data. Kantarevic and Mechoulan (2006) do estimate the effects of birth order on years of schooling. In results not reported, we find that the difference between first- and second-borns is about 0.3 years of education, slightly larger than the estimate of roughly 0.2 years in Kantarevic and Mechoulan (2006), but very similar to the estimates reported by De Haan (2010) from the Wisconsin Longitudinal Study. As with high school completion, both our estimates and those of Kantarevic and Mechoulan (2006) suggest that the effect of a marginal increment in birth order declines as birth order increases.

b. The role of cohort size

As with college completion, we estimate the effect of cohort size on high school completion by regressing state-by-cohort high school completion rates on log cohort size, state fixed effects, cohort fixed effects, and additional controls. The results are reported in Table 5. In column 1, we find that a 10 percent increase in cohort size decreases high school completion by about 0.5 percentage points for white and nonwhite men, with a smaller effect of around 0.1–0.3 percentage points among women. These estimates are somewhat smaller than those reported by Card and Lemieux (2000), and our estimates further decline as controls are added. Column 2 adds state trends, column 3 adds the same aggregate parents' characteristics we used in our analysis of college completion, and column 4 adds the state-by-cohort distribution of birth order. In each of these specifications, and for all four race-sex groups, we estimate that a 10 percent increase in cohort size decreases high school completion by no more than about 0.2 percentage points, and the estimates are almost all statistically insignificant.

c. Contribution of changes in birth order and cohort size to changes in high school completion

Figure 6 and Appendix Table A7 show the estimated contributions of birth order and cohort size to changes in high school completion, using the sex-specific birth order estimates from Table 4 and the cohort size estimates from column 4 of Table 5. High school completion slowed down for all four race-sex groups across the 1950s birth cohorts, but only white men experienced an actual decline in high school completion. The counterfactual series indicate that changes in birth order and cohort size do explain some of the slowdown in high school completion for all groups, but Appendix Table A7 shows that the effects are small. Changes in birth order boosted high school completion for nonwhite men by about 2 percentage points across the 1960–1974 birth cohorts, but in all other cases the effects of changes in birth order and cohort size are less than 1 percentage point.

VI. Discussion

We find that changes in the distribution of birth order caused by the baby boom is an important new explanation for the surprising decline in college completion experienced during that generation. Birth order does an especially good job explaining changes in college attainment for white men, accounting for more than 20 percent of the decline in college completion across the 1946–1960 cohorts, and more than one third of the increase in college completion across the 1960–1974 cohorts. Birth order effects are large for some other groups, as well. For example, changes in birth order can explain more than one third of the increase in college completion for nonwhite men across the 1960–1974 cohorts.

We also reexamine the role of cohort size in explaining trends in educational attainment. We find that controlling for aggregate measures of parents' characteristics can substantially reduce estimated effects of cohort size, so previous estimates may be overstated. But cohort size remains important in some cases, depressing college completion among women by about 1 percentage point across the 1946–1960 birth cohorts, and raising it by about 1 percentage points across the 1960–1974 birth cohorts.

The baby boom caused dramatic changes in birth order and cohort size, but a third way it might have affected educational attainment is through changes in family size. For example, additional children might spread parents' time or other investments more thinly across each child, lowering high school and college completion rates. Many studies use twins or the sex composition of the first two children as instruments for family size. Unfortunately, we lack the sample size in our HRS data to implement these IV strategies. But previous research generally finds small effects of family size on educational attainment (Black et al., 2005; Cáceres-Delpiano, 2006; Angrist et al., 2010; Tan, 2019).

Another contributing explanation that has been offered for the decline in college attainment among men is that college attainment for the late 1940s cohorts was unusually high due to the Vietnam War, either because of draft avoidance or the GI Bill. Card and Lemieux (2001) find that the impact of Vietnam draft avoidance behavior on male college completion for the 1947 birth cohort was 2.2 percentage points. Angrist and Chen (2011) argue that the link between increased educational attainment and the Vietnam War was due to the GI Bill, rather than draft avoidance. They find that serving in the military in the Vietnam War increased college completion among white male veterans born 1948–1952 by 5 percentage points. Multiplying this effect by the share of these cohorts that were veterans (about 30 percent) yields an estimate of the effect of wartime service on college completion of 1.5 percentage points. We find that the baby boom is just as important: changes in birth order can explain 1.4 percentage points of the decrease in college completion among white men across the 1946–1960 birth cohorts. Changes in birth order can also explain some of the slowdown in educational attainment among women during the baby boom generation, giving us an important new explanation for the evolution of human capital during this period of U.S. history.

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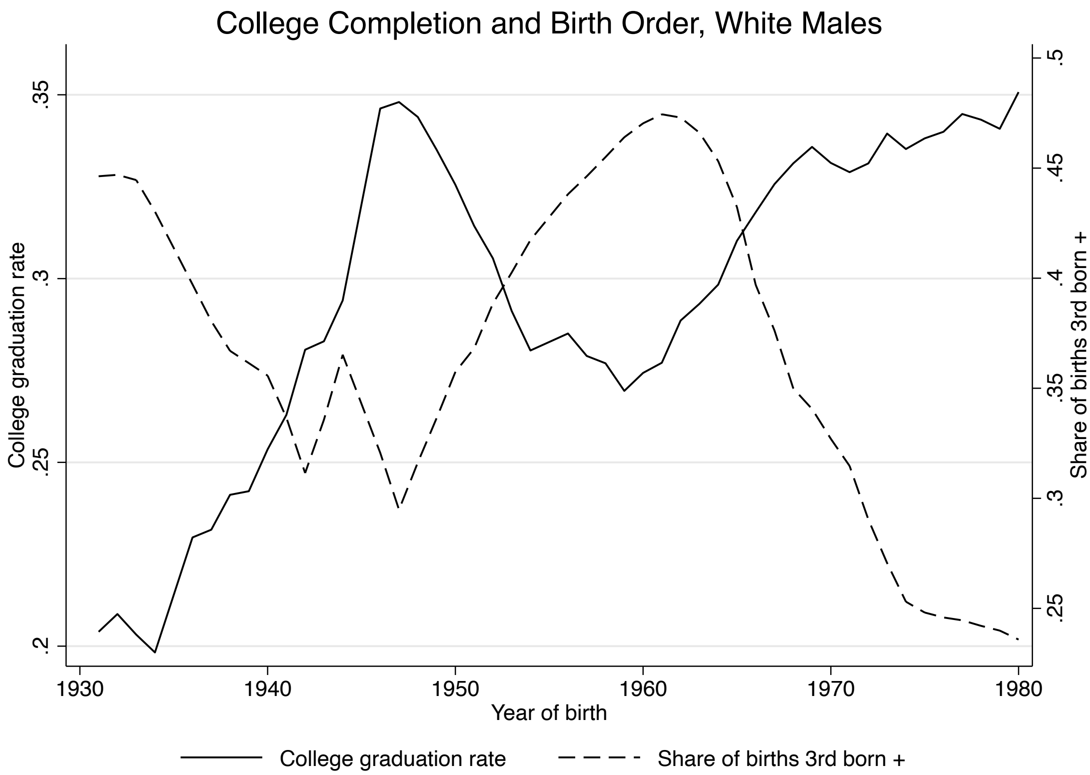
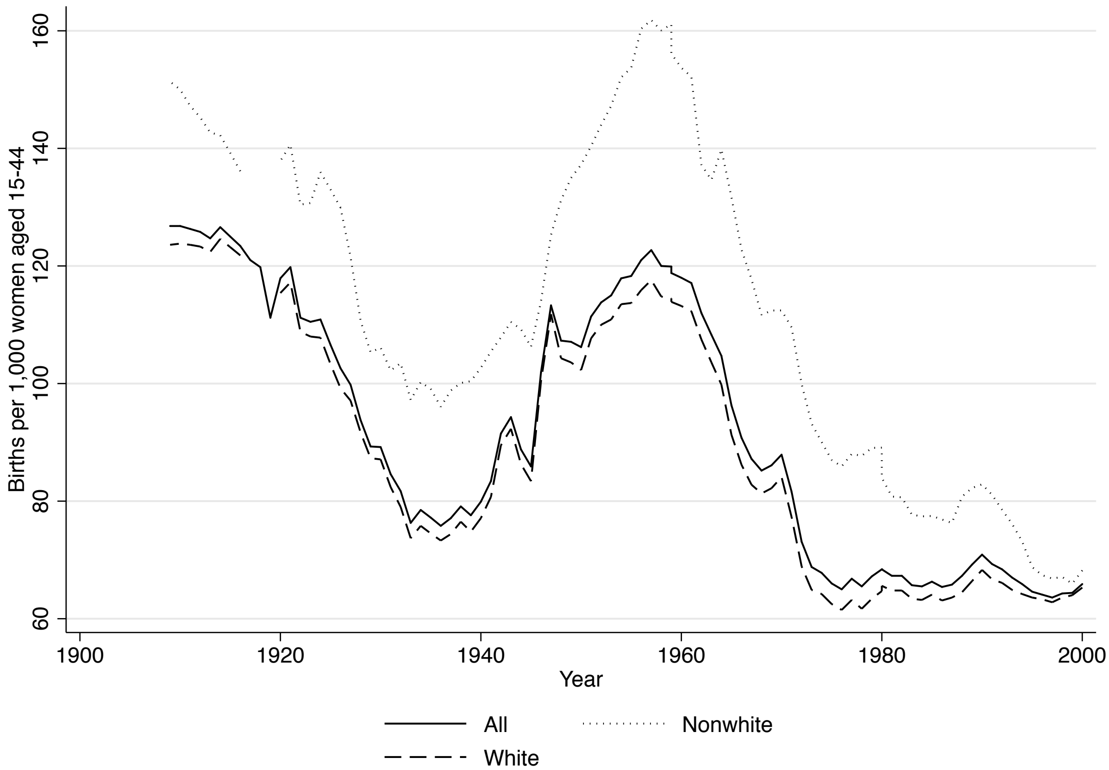
Figure 1: White male college completion and the share of individuals third-born or higher



Notes: Educational attainment for each cohort is measured using data for 36–45 year olds from the 1970–2000 censuses and the 2006–2017 ACS. Birth order data is from Vital Statistics and is aggregated across all states (including Washington, DC) with reported birth data except for Alaska and Hawaii. South Dakota is missing from the Vital Statistics data in 1930–1931 and Texas is omitted in 1930–1932.

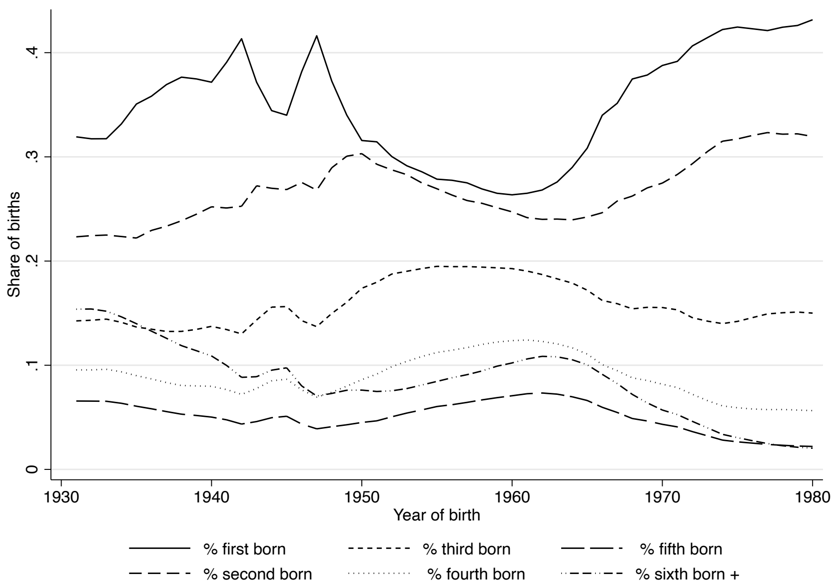
Figure 2: Fertility rates by year, 1909–2000



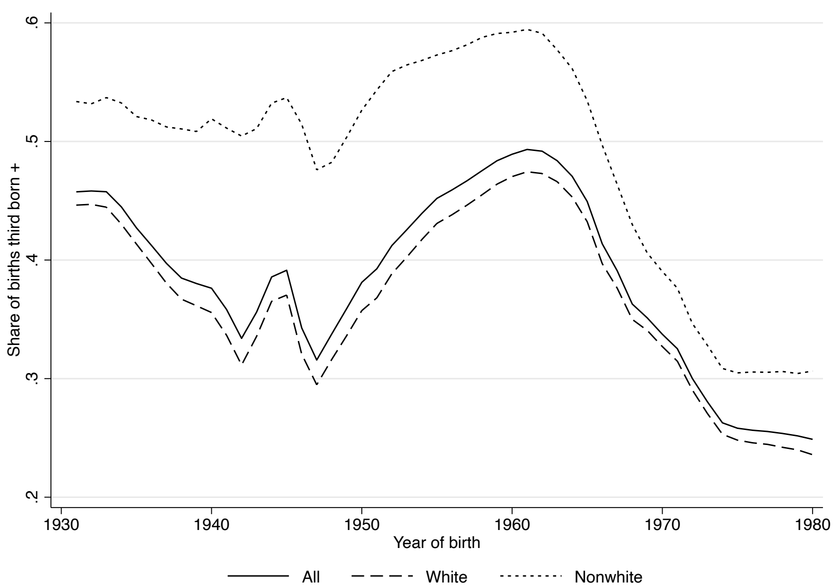
Notes: Data is from https://www.cdc.gov/nchs/data/statab/t001x01.pdf. Fertility rates are defined as births per 1,000 women aged 15–44. Births to nonresidents are excluded beginning in 1970. Race is defined by mother from 1980–2000 and by child before 1980. Birth counts before 1959 are adjusted for underregistration.

Figure 3: Distribution of birth order by birth year, 1930–1980

a. Share of births in each cohort with a given birth order

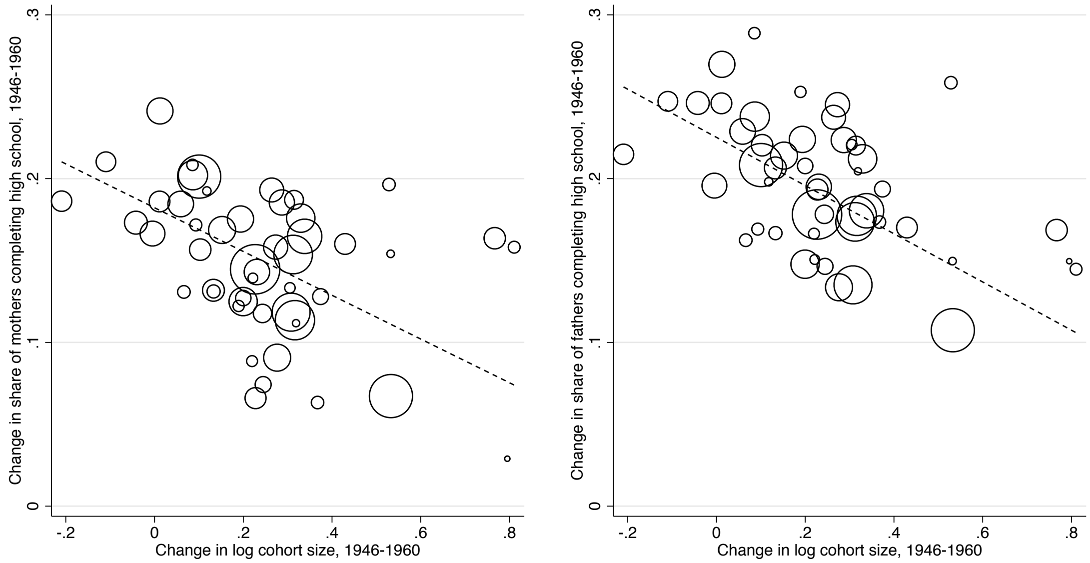


b. Share of births in each cohort that are third births or higher



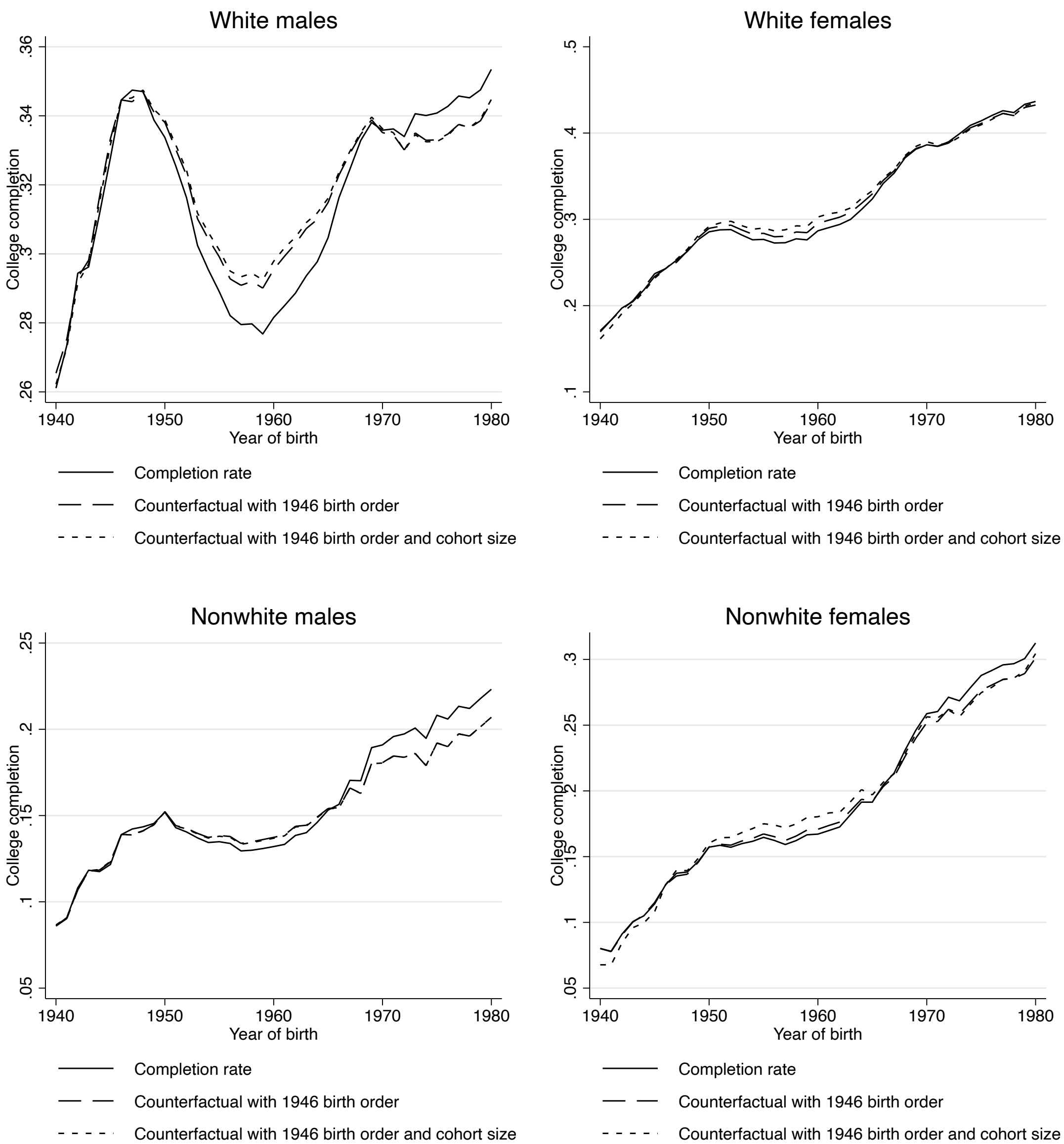
Notes: Data is from Vital Statistics and is aggregated across all states (including Washington, DC) with reported birth data except for Alaska and Hawaii. South Dakota is missing from the Vital Statistics data in 1930–1931 and Texas is omitted in 1930–1932.

Figure 4: State-level changes in log cohort size and high school completion among white parents, 1946–1960



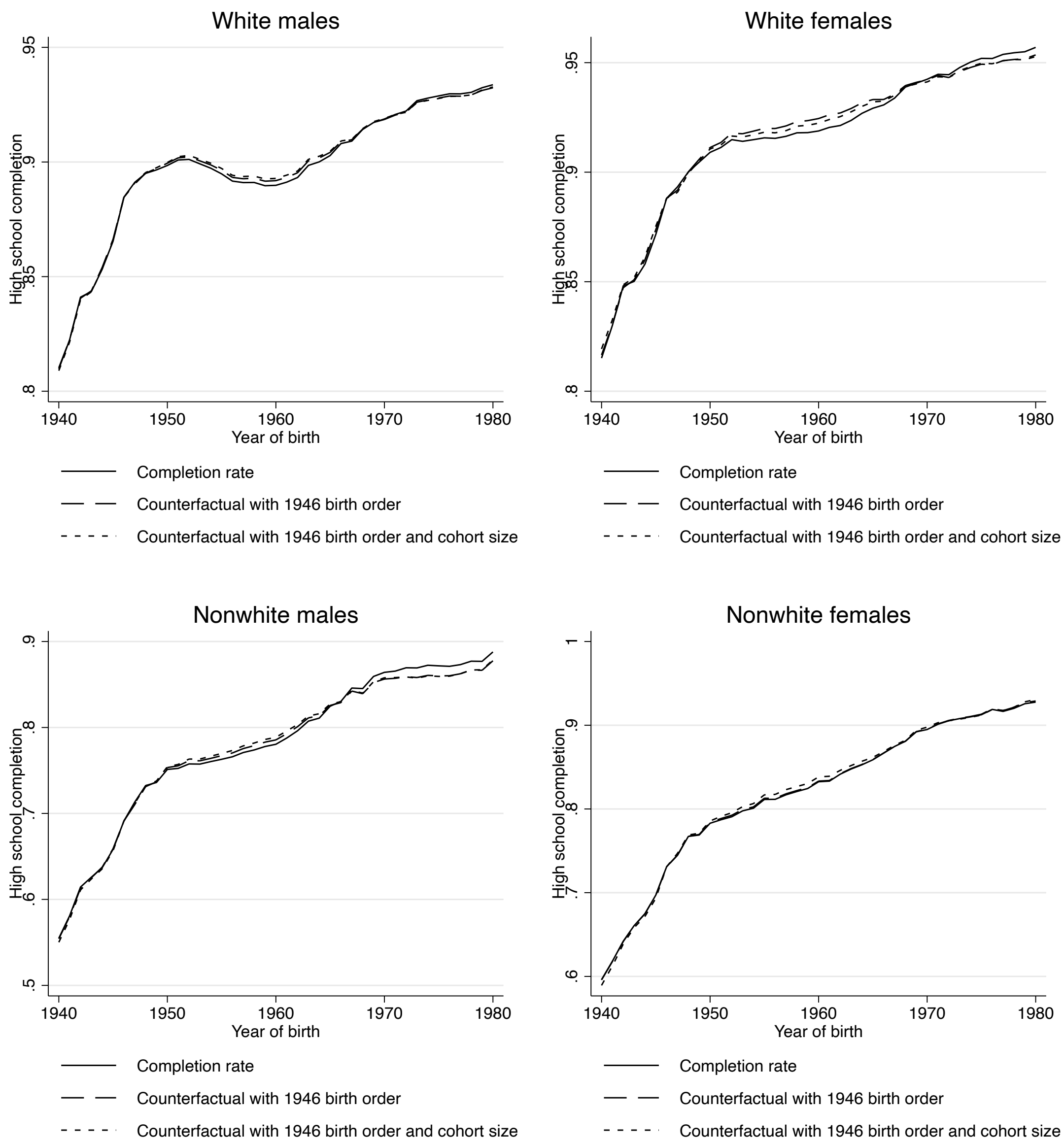
Notes: Cohort size data is from Vital Statistics and parents' education data is from the 1940–1990 censuses. Each circle represents a state and marker sizes are proportional to 1946 cohort size.

Figure 5: Actual and counterfactual trends in college completion



Notes: We plot estimated educational attainment at age 40 from the census and ACS for birth cohorts 1940–1980. We use the sex-specific birth order coefficients in Table 2 and the cohort size coefficients in Table 3, column 5 to to compute counterfactual educational attainment for each cohort using the 1946 birth order distribution and cohort size; see text for details about these counterfactual series.

Figure 6: Actual and counterfactual trends in high school completion



Notes: We plot estimated educational attainment at age 40 from the census and ACS for birth cohorts 1940–1980. We use the sex-specific birth order coefficients in Table 5 and the cohort size coefficients in Table 6, column 5 to to compute counterfactual educational attainment for each cohort using the 1946 birth order distribution and cohort size; see text for details about these counterfactual series.

Table 1: Summary statistics

	HRS Full regression sample 1912–91 birth cohorts		HRS Baby boom generation 1946–74 birth cohorts		Census Baby boom generation 1946–74 birth cohorts	
	White	Nonwhite	White	Nonwhite	White	Nonwhite
Birth year	1949.0	1954.2	1955.8	1958.0	1959.7	1961.0
Female	0.526	0.550	0.529	0.550	0.504	0.530
Sibship size	4.61	6.16	4.72	6.15	4.14	5.76
Men						
High school grad	0.880	0.701	0.910	0.733	0.902	0.806
Some college	0.486	0.336	0.530	0.358	0.566	0.417
College grad	0.298	0.169	0.325	0.181	0.312	0.160
Women						
High school grad	0.888	0.733	0.923	0.762	0.922	0.841
Some college	0.483	0.380	0.556	0.411	0.589	0.492
College grad	0.269	0.192	0.327	0.207	0.309	0.195
# siblings (*N*)	25,468	16,638	15,632	12,721		
# families	9,996	6,473	6,809	5,257		

Notes: For the HRS sample, we use sibling data for all HRS waves, 1992–2016. For more details about this sample selection process, see the appendix. Because we use family fixed effects in the birth order regressions, we keep only cases in which the HRS respondent has reported any siblings. For the census sample, we use the 1960–2000 censuses and 2006–2017 ACS. Educational attainment is adjusted for age, as described in section III(a). Sibship size in the census sample is reported for children of mothers who were born in 1930, approximately the typical mothers' birth cohort for individuals in our HRS sibling sample who were born 1946–1974. To match the HRS sample, we condition the census measure of sibship size on women with at least two children.

Table 2: The effect of birth order on college completion

White			
	All	Male	Female
birth order 2	–0.049***	–0.043***	–0.054***
	(0.014)	(0.022)	(0.020)
birth order 3	–0.078***	–0.094***	–0.064***
	(0.017)	(0.024)	(0.022)
birth order 4	–0.071***	–0.077***	–0.067**
	(0.021)	(0.027)	(0.026)
birth order 5	–0.096***	–0.127***	–0.068**
	(0.026)	(0.032)	(0.031)
birth order 6+	–0.115***	–0.157***	–0.079**
	(0.030)	(0.035)	(0.034)
Birth year fixed effects	x	x	
Family fixed effects	x	x	
# siblings (*N*)	25,468	25,468	
# families	9,996	9,996	

Nonwhite			
	All	Male	Female
birth order 2	–0.038**	–0.054**	–0.025
	(0.016)	(0.023)	(0.022)
birth order 3	–0.039**	–0.051**	–0.030
	(0.018)	(0.025)	(0.023)
birth order 4	–0.058***	–0.067**	–0.052**
	(0.021)	(0.028)	(0.026)
birth order 5	–0.070***	–0.086***	–0.059*
	(0.025)	(0.033)	(0.031)
birth order 6+	–0.081***	–0.103***	–0.064**
	(0.028)	(0.034)	(0.032)
Birth year fixed effects	x	x	
Family fixed effects	x	x	
# siblings (*N*)	16,638	16,638	
# families	6,473	6,473	

Notes: Data are from the HRS. All regressions include an indicator for sex and an indicator for whether the individual is an HRS respondent, as well as the log cohort size, measured from Vital Statistics data, for the individual's year and census division of birth. We interact each birth order indicator with an indicator for being born outside the baby boom generation (defined as the 1946–1974 birth cohorts), and report the coefficients for the baby boom generation. Male and female coefficients are estimated in a single regression in which sex is interacted with birth order. Standard errors are clustered at the family level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Log cohort size and college completion

	(1)	(2)	(3)	(4)
White				
Male	–0.063***	–0.066***	–0.037***	–0.010
	(0.011)	(0.015)	(0.012)	(0.011)
Female	–0.071***	–0.083***	–0.054***	–0.029**
	(0.012)	(0.016)	(0.013)	(0.012)
Nonwhite				
Male	–0.016**	–0.028	–0.006	0.002
	(0.008)	(0.025)	(0.019)	(0.019)
Female	–0.021	–0.045**	–0.041**	–0.037*
	(0.016)	(0.021)	(0.019)	(0.020)
State and cohort effects	x	x	x	x
State trends		x	x	x
Parent characteristics			x	x
Birth order distribution				x

Notes: Each coefficient is from a separate regression of college completion for a specific race-sex group on cohort size, measured as the log number of births in their birth state and birth year. We include the 1946–1974 birth cohorts in the regression. Educational attainment data are from the 1960–2000 censuses and 2006–2017 ACS and are adjusted for age, as described in section III(a). We use Vital Statistics data to measure cohort size and the percent of the cohort belonging to each birth order. Parent characteristics are measured from the 1940–1990 censuses and include percent white, percent foreign-born, percent achieving each of three levels of education (9 years, high school, and college), and age at birth (percentiles 25, 50, and 75), for mothers and fathers, as well as the percent of mothers who are married, mothers' age at marriage (percentiles 25, 50, and 75), the percent of fathers present in the household, and father's log earnings (percentiles 25, 50, and 75). Standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: The effect of birth order on high school completion

White	All	Male	Female
birth order 2	–0.026***	–0.020*	–0.031***
	(0.008)	(0.012)	(0.012)
birth order 3	–0.021*	–0.008	–0.032**
	(0.011)	(0.014)	(0.014)
birth order 4	–0.039***	–0.023	–0.053***
	(0.015)	(0.018)	(0.017)
birth order 5	–0.032*	–0.021	–0.040*
	(0.018)	(0.022)	(0.021)
birth order 6+	–0.045**	–0.027	–0.061**
	(0.022)	(0.027)	(0.026)
Birth year fixed effects	x	x	
Family fixed effects	x	x	
# Siblings (N)	25,468	25,468	
# Families	9,996	9,996	

Nonwhite	All	Male	Female
birth order 2	–0.035**	–0.068***	–0.008
	(0.014)	(0.022)	(0.019)
birth order 3	–0.047***	–0.085***	–0.016
	(0.017)	(0.023)	(0.021)
birth order 4	–0.058***	–0.091***	–0.033
	(0.021)	(0.027)	(0.025)
birth order 5	–0.052**	–0.075**	–0.037
	(0.024)	(0.031)	(0.029)
birth order 6+	–0.024	–0.077**	0.017
	(0.028)	(0.033)	(0.031)
Birth year fixed effects	x	x	
Family fixed effects	x	x	
# siblings (*N*)	16,638	16,638	
# families	6,473	6,473	

Notes: Data are from the HRS. All regressions include an indicator for sex and an indicator for whether the individual is an HRS respondent, as well as the log cohort size, measured from Vital Statistics data, for the individual's year and census division of birth. We interact each birth order indicator with an indicator for being born outside the baby boom generation (defined as the 1946–1974 birth cohorts), and report the coefficients for the baby boom generation. Male and female coefficients are estimated in a single regression in which sex is interacted with birth order. Standard errors are clustered at the family level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Log cohort size and high school completion

	(1)	(2)	(3)	(4)
White				
Male	–0.046***	–0.011	–0.007	–0.004
	(0.014)	(0.008)	(0.008)	(0.008)
Female	–0.028**	–0.015*	0.001	0.008
	(0.012)	(0.008)	(0.007)	(0.007)
Nonwhite				
Male	–0.045*	–0.023	–0.016	–0.012
	(0.027)	(0.022)	(0.021)	(0.021)
Female	–0.013	–0.020	–0.019	–0.020
	(0.019)	(0.015)	(0.017)	(0.018)
State and cohort effects	x	x	x	x
State trends		x	x	x
Parent characteristics			x	x
Birth order distribution				x

Notes: Each coefficient is from a separate regression of high school completion for a specific race-sex group on cohort size, measured as the log number of births in their birth state and birth year. We include the 1946–1974 birth cohorts in the regression. Educational attainment data are from the 1960–2000 censuses and 2006–2017 ACS and are adjusted for age, as described in section III(a). We use Vital Statistics data to measure cohort size and the percent of the cohort belonging to each birth order. Parent characteristics are measured from the 1940–1990 censuses and include percent white, percent foreign-born, percent achieving each of three levels of education (9 years, high school, and college), and age at birth (percentiles 25, 50, and 75), for mothers and fathers, as well as the percent of mothers who are married, mothers' age at marriage (percentiles 25, 50, and 75), the percent of fathers present in the household, and father's log earnings (percentiles 25, 50, and 75). Standard errors are clustered at the state level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Appendix

I. Details on data construction

a. Health and Retirement Survey (HRS)

We use data from the HRS on respondents and their siblings. Data on respondents for all waves, 1992–2016, is from the RAND HRS Longitudinal File, and data on siblings comes from the HRS files for each wave.

Sibling age was collected in the 1992 and 1994 HRS waves, but not in the 1993 and 1995 waves for the oldest cohort. Beginning in 1996, sibling age and educational attainment is collected in each biannual wave. In 1992–2000, the household's family respondent reports on both siblings and siblings-in-law. We use relationship codes to assign the sibling to the appropriate respondent. If the family respondent reports a sibling-in-law but is not married in that wave, we assign the sibling to the respondent's most recent spouse. If a sibling cannot be assigned in this way, they are dropped from the sample. Beginning with the 2002 wave, the family respondent reports only on his or her own siblings.

We set reported education to missing for any siblings younger than 25 when education is reported, which happens in a small number of cases in which an HRS respondent is married to someone younger who reports younger siblings. For some siblings, there is data on age and/or education from multiple waves. We infer the sibling's birth year from the reported age. If two siblings have the same birth year, we assume that the sibling reported first was born earlier. We then drop siblings with any inconsistent reports about sex, a range of reported birth years greater than 2, or a range of reported years of education greater than 1. Finally, we resolve the remaining small inconsistencies by taking the median of the sibling's reported birth years and completed years of education, rounded to the nearest integer.

b. Parent characteristics

We use data on parents' characteristics as controls in our cohort size regressions. We begin with the 1940–1990 decennial censuses and measure parents' characteristics for children age 17 and younger. We match parents to children using the IPUMS variables MOMLOC and POPLOC. We then measure a variety of parents' characteristics for each combination of child's state of birth, child's birth cohort, and child's race. For both mothers and fathers, we measure percent white, percent foreign-born, percent achieving each of three levels of education (9 years, high school, and college), and age at birth (percentiles 25, 50, and 75). We also measure the percent of mothers who are married, mothers' age at marriage (percentiles 25, 50, and 75), the percent of fathers present in the household, and father's log earnings (percentiles 25, 50, and 75).

c. Vital Statistics

Total births and birth order come from the Vital Statistics Natality files. Data for 1931–1967 come from summary tables in published pdfs. Data for 1968–2000 come from individual-level natality data from the NBER Vital Statistics page.

The share of births with birth order not reported averages 2.6 percent, with a maximum of 5.7 percent in 1945. We drop these births from our sample when measuring the birth order distribution for each birth cohort. If we instead allocate birth order not reported cases among birth order — by estimating separate regressions of the percentage of births of each birth order on the percentage of births without reported birth order, state fixed effects, and year fixed effects, and allocating birth order not reported cases across birth order bins based on these coefficients — our results are very similar.

Annual data on total births and the birth order distribution by race is reported in Table A1.

II. Sensitivity checks for estimates of birth order effects

We test the sensitivity of our reported birth order effects to changes in our definition of the baby boom generation, and to changes in our construction of the sibling sample in the HRS. We describe the sensitivity checks here, and the results are reported in Table A2. We report results of these checks only for white male college completion, because that is the key result in our paper, but results of the same sensitivity checks for other groups or other outcomes are available on request.

In Table A2, specification 1 is identical to the birth order effects reported in Table 2, and the effect of changes in birth order on college completion is identical to what is reported in Table 4.

In specification 2, we expand our definition of the baby boom generation to birth years 1939–1974, instead of 1946–1974 as in our base specification, because some authors (e.g., Bailey and Collins 2011) use 1939 as the beginning of the baby boom. This affects our birth order estimates because we interact our birth order indicators with a non-baby boom indicator, and report estimates specific to the baby boom generation. Here, our sample stays the same, but the reported birth order effects apply to the 1939–1974 cohorts.

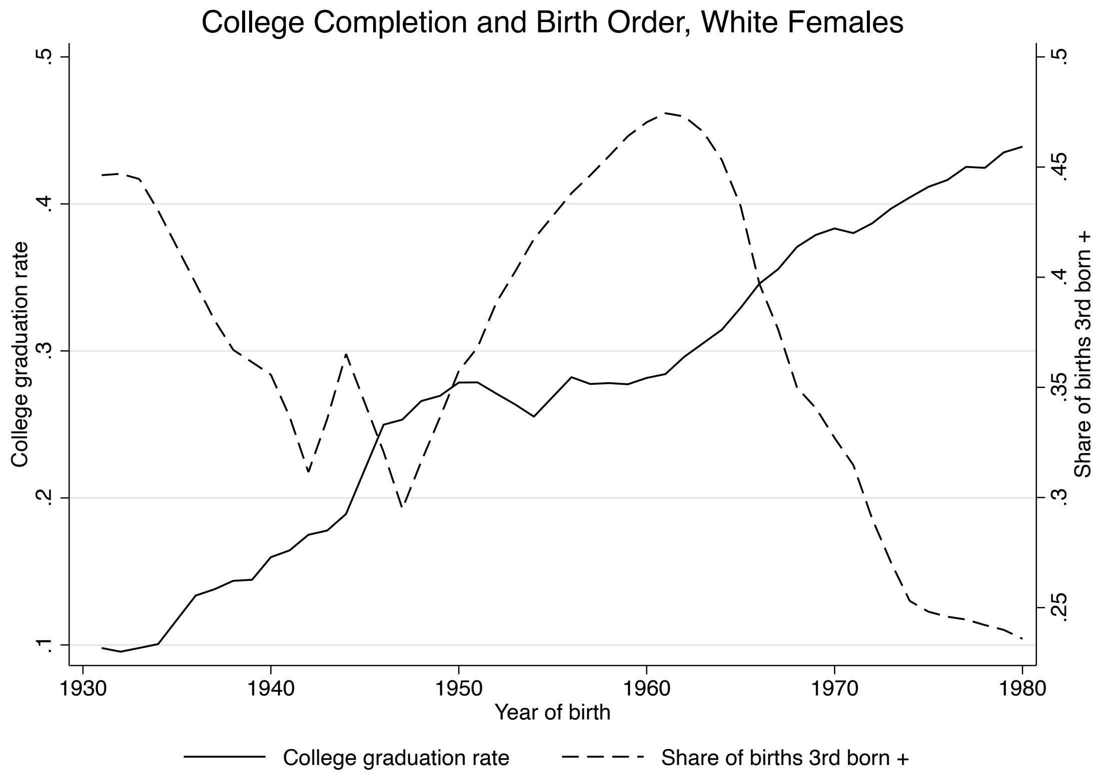
Specifications 3–8 limit or extend our sample. In our base sample, we include some HRS respondents who report many siblings, but some of these reports could be erroneous or duplicates. Specification 3 limits our sample to respondents and their siblings from families with 10 or fewer children (9 or fewer reported siblings). In our base sample, we drop reported siblings who are more than 20 years older or younger than the respondent. In specification 4, we include these possible siblings.

In our base sample, when siblings are missing age data, we assign birth order among the siblings with valid age data, implicitly assuming the siblings the valid data were born first. In specification 5, we instead assume the siblings with valid age data were born last. In specification 6, if any sibling is missing data on age or education, we drop the entire family from the sample.

In our base sample, we drop siblings with any inconsistent reports about sex, a range of reported birth years greater than 2, or a range of reported years of education greater than 1. In specification 7, we include these siblings in the sample. When we do so, we resolve inconsistent reports about sex by assuming the most commonly reported sex, and we resolve inconsistent reports about birth year and education in the same way as we do for our main sample, by taking the median of the sibling's reported birth years and completed years of education, rounded to the nearest integer.

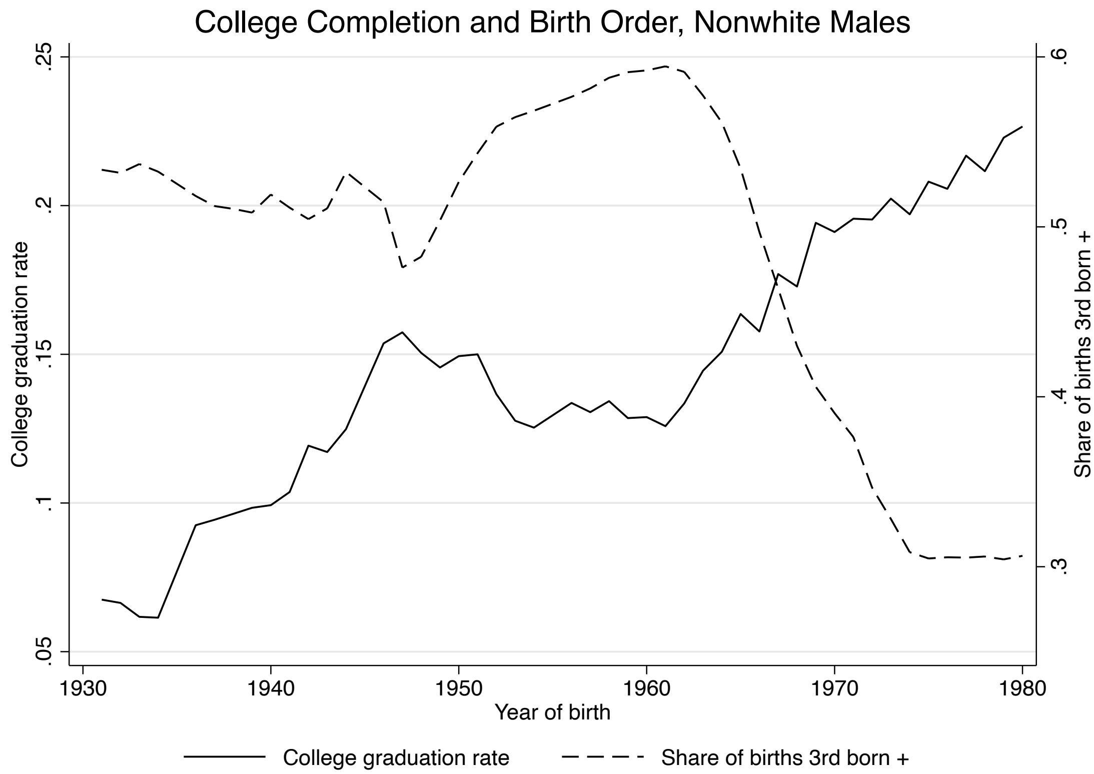
In earlier HRS waves, the family respondent reports both on his or her own siblings and his or her spouse's siblings, but reports about siblings-in-law might be of lower quality. In specification 8, we exclude individuals reported by siblings-in-law.

Figure A1: White female college completion and the percentage of individuals third-born or higher



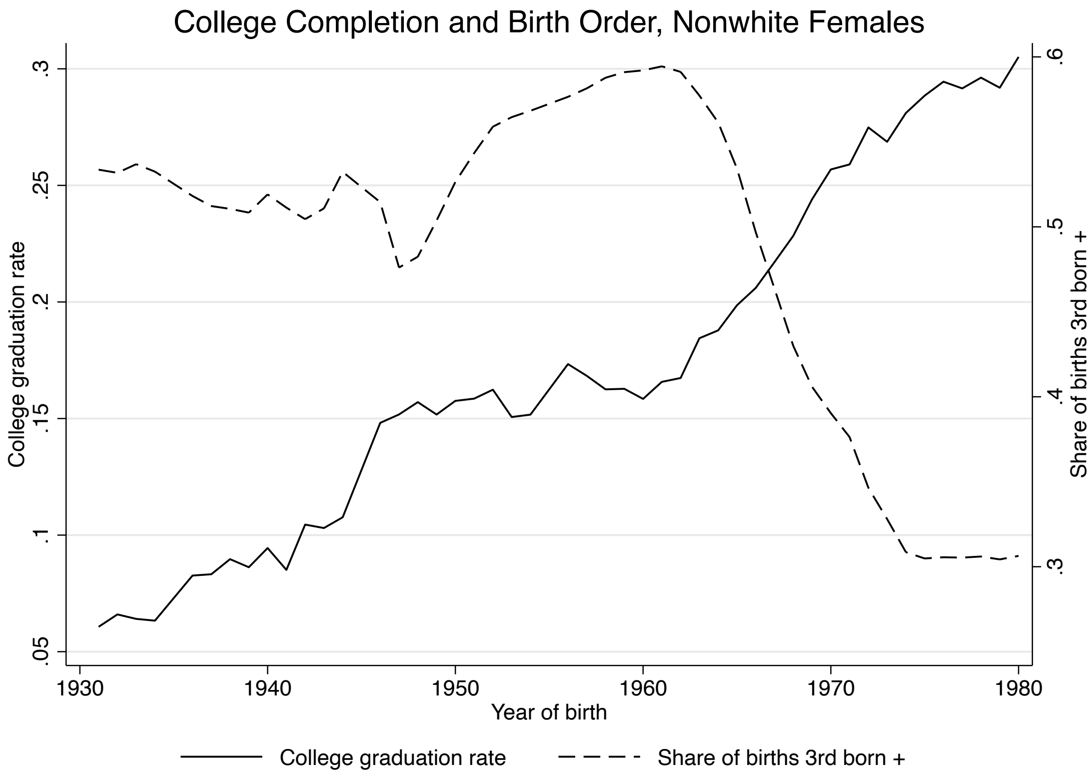
Notes: Educational attainment for each cohort is measured using data for 36–45 year olds from the 1970–2000 censuses and the 2006–2017 ACS. Birth order data is from Vital Statistics and is aggregated across all states (including Washington, DC) with reported birth data except for Alaska and Hawaii. South Dakota is missing from the Vital Statistics data in 1930–1931 and Texas is omitted in 1930–1932.

Figure A2: Nonwhite male college completion and the percentage of individuals third-born or higher



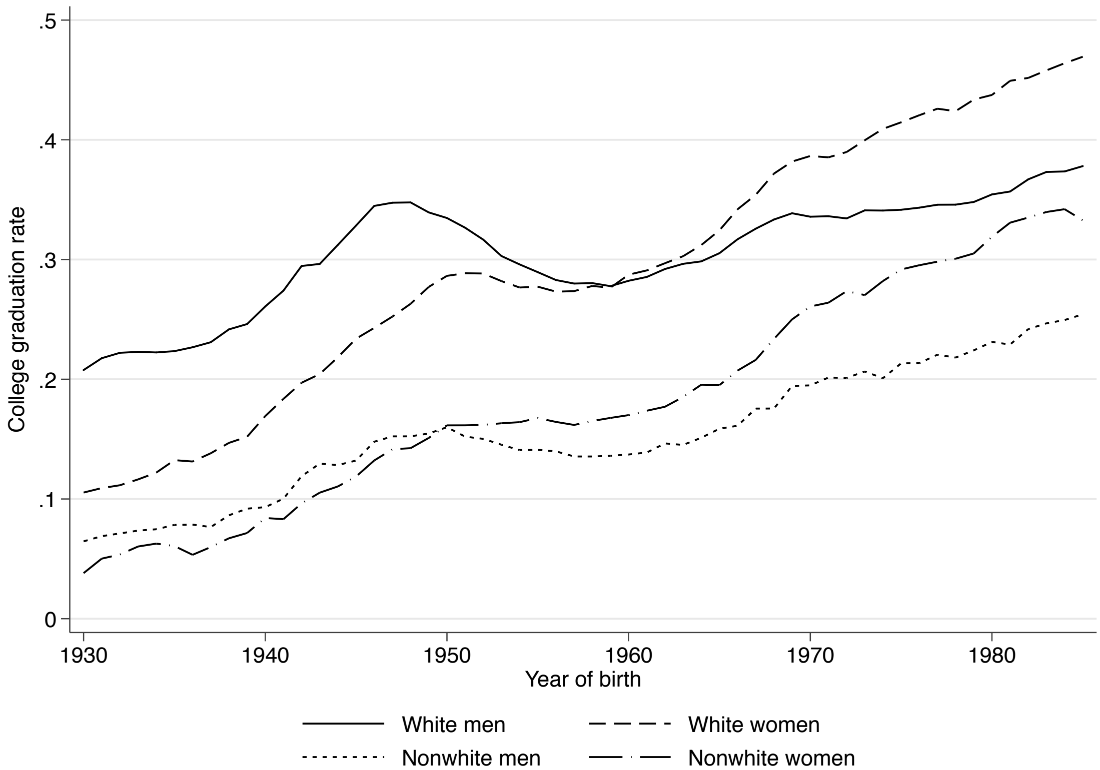
Notes: Educational attainment for each cohort is measured using data for 36–45 year olds from the 1970–2000 censuses and the 2006–2017 ACS. Birth order data is from Vital Statistics and is aggregated across all states (including Washington, DC) with reported birth data except for Alaska and Hawaii. South Dakota is missing from the Vital Statistics data in 1930–1931 and Texas is omitted in 1930–1932.

Figure A3: Nonwhite female college completion and the percentage of individuals third-born or higher



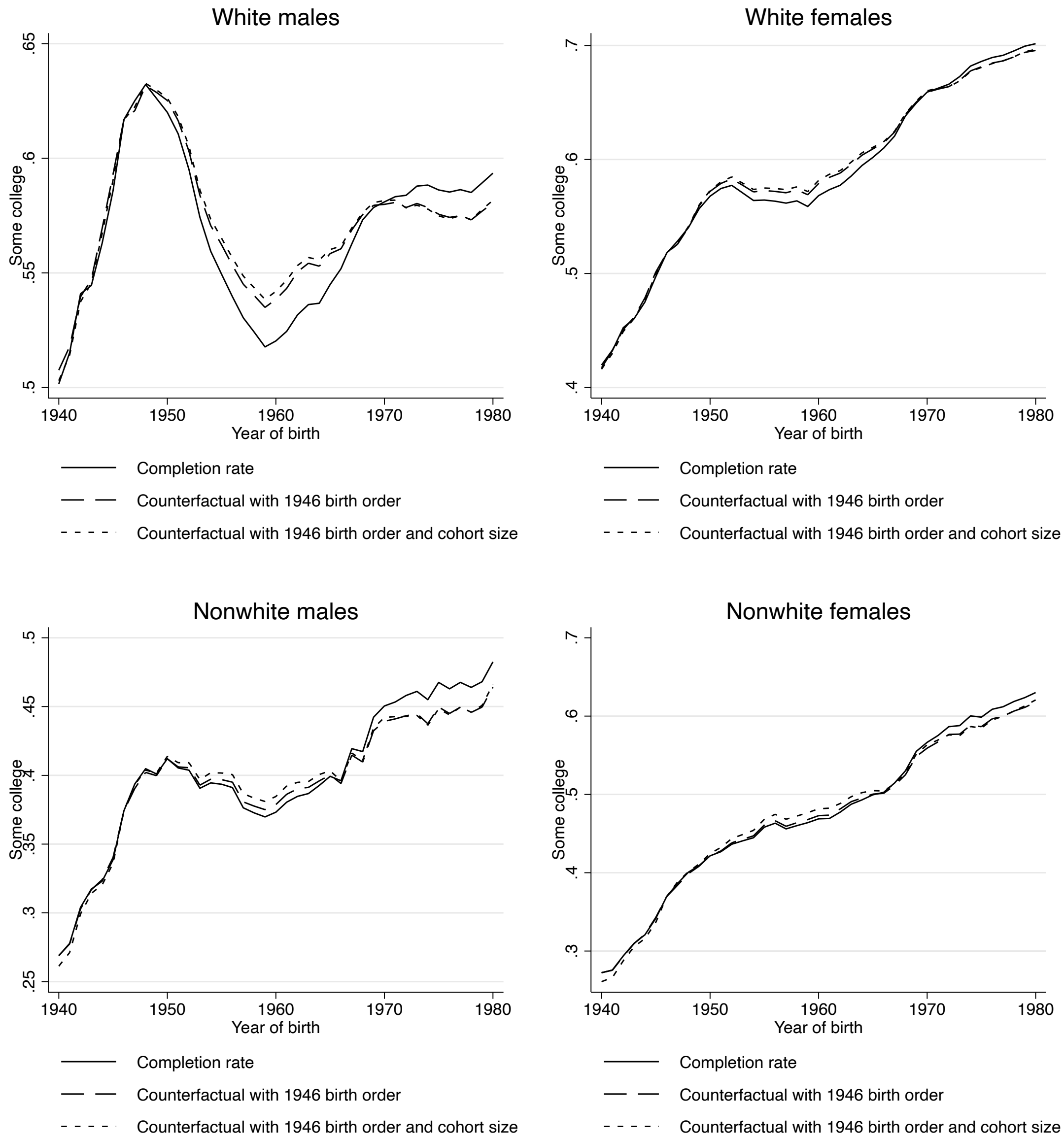
Notes: Educational attainment for each cohort is measured using data for 36–45 year olds from the 1970–2000 censuses and the 2006–2017 ACS. Birth order data is from Vital Statistics and is aggregated across all states (including Washington, DC) with reported birth data except for Alaska and Hawaii. South Dakota is missing from the Vital Statistics data in 1930–1931 and Texas is omitted in 1930–1932.

Figure A4: College graduation by birth year, 1930–1985



Notes: Educational attainment for each cohort is estimated using data for 25–64 year olds from the 1980–2000 censuses and the 2006–2017 ACS. We regress educational attainment on an age cubic, state-by-cohort fixed effects, and an indicator for whether the sample was before 1990, as the education question changed slightly at that time. The figure above plots predicted educational attainment for each birth cohort at age 40.

Figure A5: Actual and counterfactual trends in some college



Notes: We plot estimated educational attainment at age 40 from the census and ACS for birth cohorts 1940–1980. We use the sex-specific birth order coefficients in Table A3 and the cohort size coefficients in Table A4, column 5 to to compute counterfactual educational attainment for each cohort using the 1946 birth order distribution and cohort size; see text for details about these counterfactual series.

Table A1: Robustness of birth order effects: White male college completion

	Birth order estimates					*Δ in college explained by Δ in birth order*		# siblings
	2nd	3rd	4th	5th	6th +	*1946–60*	*1960–74*	# families
(1) Base specification, 1946–1974								
	–0.043**	–0.094***	–0.077***	–0.127***	–0.157***	*–0.014****	*0.021****	25,468
	(0.022)	(0.024)	(0.027)	(0.032)	(0.035)	*(0.003)*	*(0.005)*	9,996
(2) Extend baby boom back to 1939 (1939–1974)								
	–0.046**	–0.088***	–0.079***	–0.126***	–0.148***	*–0.013****	*0.020****	25,468
	(0.018)	(0.020)	(0.024)	(0.029)	(0.032)	*(0.003)*	*(0.004)*	9,996
(3) Limit sample to families with 10 or fewer children								
	–0.045**	–0.095***	–0.079***	–0.131***	–0.150***	*–0.014****	*0.021****	24,720
	(0.022)	(0.024)	(0.028)	(0.034)	(0.037)	*(0.003)*	*(0.005)*	9,780
(4) Include siblings more than 20 years older or younger than respondent								
	–0.041**	–0.091***	–0.070***	–0.128***	–0.151***	*–0.013****	*0.020****	25,624
	(0.022)	(0.024)	(0.027)	(0.032)	(0.034)	*(0.003)*	*(0.005)*	10,009
(5) Assume siblings with valid age data are youngest								
	–0.034	–0.085***	–0.063**	–0.104***	–0.131***	*–0.012****	*0.018****	26,259
	(0.022)	(0.023)	(0.026)	(0.031)	(0.031)	*(0.003)*	*(0.004)*	10,045
(6) Limit sample to families with education and age data for all siblings								
	–0.040*	–0.093***	–0.075***	–0.123***	–0.154***	*–0.014****	*0.021****	25,313
	(0.022)	(0.024)	(0.027)	(0.032)	(0.035)	*(0.003)*	*(0.005)*	9,913
(7) Include siblings with inconsistently reported sex, age, and/or education								
	–0.041*	–0.090***	–0.078***	–0.117***	–0.152***	*–0.013****	*0.020****	26,155
	(0.022)	(0.023)	(0.026)	(0.032)	(0.033)	*(0.003)*	*(0.005)*	10,043
(8) Limit sample to families in which siblings are self-reported (not by spouse)								
	–0.055*	–0.102***	–0.076*	–0.133***	–0.168***	*–0.014****	*0.022****	19,926
	(0.032)	(0.035)	(0.039)	(0.047)	(0.048)	*(0.005)*	*(0.007)*	9,978

Notes: Data are from the HRS. Each row reports results from a separate regression. Specification 1 is identical to the one reported in Table 2. All regressions include family fixed effects, birth year fixed effects, an indicator for sex and an indicator for whether the individual is an HRS respondent, as well as the log cohort size, measured from Vital Statistics data, for the individual's year and census division of birth. We interact each birth order indicator with an indicator for being born outside the baby boom generation (defined as the 1946–1974 birth cohorts), and report the coefficients for the baby boom generation. We estimate male and female coefficients in a single regression in which sex is interacted with birth order, and here report only the coefficients for men. Standard errors are clustered at the family level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: National births and birth order distribution by race

Year	Total births	White birth order distribution (%) 1	2	3	4	5	6+	Nonwhite birth order distribution (%) 1	2	3	4	5	6+
1946	3,288,672	39.3	28.3	14.4	7.4	4.0	6.5	28.2	21.1	14.3	10.2	7.4	18.8
1947	3,699,940	42.6	27.5	13.8	6.7	3.6	5.7	31.5	21.3	13.9	9.4	6.8	17.1
1948	3,535,068	38.4	29.7	15.1	7.2	3.8	5.9	29.3	22.9	14.8	9.7	6.8	16.6
1949	3,559,529	35.2	31.0	16.1	7.7	3.9	6.1	26.7	23.0	15.7	10.3	7.1	17.2
1950	3,553,688	32.7	31.5	17.5	8.2	4.0	6.0	25.2	22.3	16.7	11.0	7.5	17.3
1951	3,750,850	32.7	30.5	18.1	8.7	4.2	5.8	24.4	21.3	16.9	11.8	7.8	17.6
1952	3,846,986	31.2	30.0	19.0	9.4	4.5	5.9	23.6	20.6	16.7	12.5	8.5	18.0
1953	3,902,120	30.3	29.5	19.4	10.0	4.8	6.0	23.5	20.2	16.3	12.5	9.0	18.5
1954	4,017,362	29.6	28.8	19.8	10.5	5.1	6.2	23.6	19.7	15.9	12.4	9.2	19.2
1955	4,047,295	29.0	28.1	20.0	11.0	5.5	6.5	22.9	20.0	15.8	12.3	9.2	19.7
1956	4,163,090	28.9	27.5	20.0	11.2	5.7	6.7	22.7	19.8	15.7	12.2	9.2	20.5
1957	4,254,784	28.7	26.9	20.0	11.6	5.9	7.0	22.5	19.6	15.9	12.0	9.2	20.8
1958	4,203,812	28.1	26.6	20.0	11.9	6.2	7.3	21.9	19.5	15.9	12.2	9.2	21.3
1959	4,238,504	27.6	26.2	19.9	12.1	6.4	7.7	21.9	19.2	15.7	12.2	9.3	21.8
1960	4,233,082	27.5	25.8	19.9	12.3	6.6	8.0	21.9	19.0	15.6	12.1	9.2	22.2
1961	4,248,814	27.6	25.2	19.6	12.4	6.8	8.3	22.0	18.8	15.4	12.2	9.2	22.5
1962	4,012,710	28.0	25.0	19.3	12.2	6.9	8.6	22.3	18.8	15.3	11.9	9.2	22.5
1963	3,943,662	28.5	25.0	18.9	12.1	6.9	8.7	23.3	19.0	15.0	11.7	9.0	22.0
1964	4,002,864	29.8	24.9	18.5	11.7	6.7	8.4	24.6	19.3	14.8	11.3	8.6	21.5
1965	3,736,940	31.7	25.1	17.7	11.1	6.3	8.1	26.8	19.8	14.6	10.9	8.1	19.8
1966	3,584,722	34.9	25.4	16.6	10.0	5.7	7.4	29.7	20.6	14.3	10.2	7.4	17.8
1967	3,504,803	35.8	26.6	16.3	9.5	5.2	6.7	32.1	21.5	14.0	9.6	6.8	16.0
1968	3,480,602	37.9	27.1	15.7	8.7	4.6	5.9	35.1	21.9	13.7	9.0	6.1	14.1
1969	3,577,684	38.1	27.8	15.9	8.5	4.4	5.3	36.6	22.9	13.8	8.8	5.8	12.1
1970	3,707,422	39.0	28.2	15.8	8.1	4.1	4.7	37.2	23.7	14.1	8.7	5.5	10.8
1971	3,532,846	39.4	29.1	15.6	7.8	3.9	4.3	37.9	24.5	14.2	8.4	5.2	9.9
1972	3,236,071	40.6	30.2	14.8	7.1	3.4	3.9	40.2	25.0	13.9	7.9	4.7	8.3
1973	3,114,997	41.3	31.3	14.4	6.5	3.1	3.5	40.8	26.0	14.2	7.6	4.3	7.1
1974	3,137,402	42.2	32.3	14.0	5.9	2.7	2.9	41.6	27.4	14.1	7.1	3.8	6.0

Notes: Data are from Vital Statistics published tables and birth-level records.

Table A3: Changes in college completion explained by birth order and cohort size

	White		Nonwhite	
	Male	Female	Male	Female
1946–1960 Δ in % college	–0.063	0.044	–0.007	0.039
Effect of Δ in birth order	–0.014***	–0.009**	–0.005***	–0.004**
	(0.003)	(0.003)	(0.002)	(0.002)
Effect of Δ in cohort size	–0.003	–0.008**	0.001	–0.011*
	(0.003)	(0.003)	(0.005)	(0.006)
Total effect	–0.017***	–0.017***	–0.005	–0.014**
	(0.004)	(0.005)	(0.006)	(0.006)
1960–1974 Δ in % college	0.059	0.122	0.063	0.111
Effect of Δ in birth order	0.021***	0.011***	0.021***	0.015**
	(0.005)	(0.004)	(0.007)	(0.007)
Effect of Δ in cohort size	0.003	0.009**	–0.001	0.011*
	(0.003)	(0.004)	(0.006)	(0.006)
Total effect	0.024***	0.020***	0.021**	0.026***
	(0.006)	(0.006)	(0.009)	(0.009)

Notes: Birth order and cohort size data are from Vital Statistics. Educational attainment data are from the 1960–2000 censuses and 2006–2017 ACS. We multiply changes in birth order by sex-specific coefficients reported in Table 2, and we multiply changes in cohort size by the estimate reported in Table 3, column 4. These estimates correspond to changes in the appropriate counterfactual series plotted in Figure 5.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: The effect of birth order on some college

White			
	All	Male	Female
birth order 2	–0.050***	–0.049***	–0.052***
	(0.014)	(0.022)	(0.020)
birth order 3	–0.090***	–0.111***	–0.071***
	(0.018)	(0.025)	(0.024)
birth order 4	–0.095***	–0.110***	–0.083***
	(0.023)	(0.029)	(0.028)
birth order 5	–0.121***	–0.164***	–0.083**
	(0.028)	(0.035)	(0.033)
birth order 6+	–0.151***	–0.206***	–0.103***
	(0.033)	(0.038)	(0.038)
Birth year fixed effects	x	x	
Family fixed effects	x	x	
# siblings (*N*)	25,468	25,468	
# families	9,996	9,996	
Nonwhite			
	All	Male	Female
birth order 2	–0.027	–0.036	–0.018
	(0.018)	(0.027)	(0.025)
birth order 3	–0.042**	–0.049*	–0.036
	(0.020)	(0.028)	(0.027)
birth order 4	–0.061**	–0.068**	–0.057*
	(0.025)	(0.032)	(0.030)
birth order 5	–0.056*	–0.062	–0.053
	(0.029)	(0.037)	(0.035)
birth order 6+	–0.089***	–0.114***	–0.068*
	(0.034)	(0.039)	(0.038)
Birth year fixed effects	x	x	
Family fixed effects	x	x	
# siblings (*N*)	16,638	16,638	
# families	6,473	6,473	

Notes: Data are from the HRS. All regressions include an indicator for sex and an indicator for whether the individual is an HRS respondent, as well as the log cohort size, measured from Vital Statistics data, for the individual's year and census division of birth. We interact each birth order indicator with an indicator for being born outside the baby boom generation (defined as the 1946–1974 birth cohorts), and report the coefficients for the baby boom generation. Male and female coefficients are estimated in a single regression in which sex is interacted with birth order. Standard errors are clustered at the family level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Log cohort size and some college

	(1)	(2)	(3)	(4)
White				
Male	–0.106***	–0.059***	–0.038***	–0.014
	(0.025)	(0.013)	(0.013)	(0.011)
Female	–0.112***	–0.047***	–0.033***	–0.011
	(0.030)	(0.015)	(0.011)	(0.012)
Nonwhite				
Male	–0.053**	–0.025	–0.025	–0.023
	(0.022)	(0.028)	(0.024)	(0.024)
Female	–0.048**	–0.004	–0.027	–0.035
	(0.018)	(0.023)	(0.026)	(0.028)
State and cohort effects	x	x	x	x
State trends		x	x	x
Parent characteristics			x	x
Birth order distribution				x

Notes: Each coefficient is from a separate regression of completing some post-secondary schooling for a specific race-sex group on cohort size, measured as the log number of births in their birth state and birth year. We include the 1946–1974 birth cohorts in the regression. Educational attainment data are from the 1960–2000 censuses and 2006–2017 ACS and are adjusted for age, as described in section III(a). We use Vital Statistics data to measure cohort size and the percent of the cohort belonging to each birth order. Parent characteristics are measured from the 1940–1990 censuses and include percent white, percent foreign-born, percent achieving each of three levels of education (9 years, high school, and college), and age at birth (percentiles 25, 50, and 75), for mothers and fathers, as well as the percent of mothers who are married, mothers' age at marriage (percentiles 25, 50, and 75), the percent of fathers present in the household, and father's log earnings (percentiles 25, 50, and 75). Standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Changes in some college explained by birth order and cohort size

	White		Nonwhite	
	Male	Female	Male	Female
1946–1960 Δ in % some college	–0.097	0.050	–0.001	0.099
Effect of Δ in birth order	–0.018***	–0.011***	–0.006***	–0.004**
	(0.003)	(0.003)	(0.002)	(0.002)
Effect of Δ in cohort size	–0.004	–0.003	–0.007	–0.010
	(0.003)	(0.003)	(0.007)	(0.008)
Total effect	–0.022***	–0.014***	–0.012*	–0.014*
	(0.005)	(0.005)	(0.007)	(0.008)
1960–1974 Δ in % some college	0.068	0.114	0.082	0.131
Effect of Δ in birth order	0.028***	0.015***	0.023***	0.016**
	(0.005)	(0.005)	(0.008)	(0.008)
Effect of Δ in cohort size	0.004	0.003	0.007	0.010
	(0.003)	(0.003)	(0.007)	(0.008)
Total effect	0.032***	0.018***	0.030***	0.026**
	(0.006)	(0.006)	(0.011)	(0.012)

Notes: Birth order and cohort size data are from Vital Statistics. Educational attainment data are from the 1960–2000 censuses and 2006–2017 ACS. We multiply changes in birth order by sex-specific coefficients reported in Table A4, and we multiply changes in cohort size by the estimate reported in Table A5, column 4. These estimates correspond to changes in the appropriate counterfactual series plotted in Figure A5.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Changes in high school completion explained by birth order and cohort size

	White		Nonwhite	
	Male	Female	Male	Female
1946–1960 Δ in % high school	0.005	0.031	0.089	0.102
Effect of Δ in birth order	–0.002	–0.006***	–0.005***	–0.001
	(0.002)	(0.002)	(0.002)	(0.002)
Effect of Δ in cohort size	–0.001	0.002	–0.003	–0.006
	(0.002)	(0.002)	(0.006)	(0.005)
Total effect	–0.003	–0.003	–0.009	–0.006
	(0.003)	(0.003)	(0.006)	(0.005)
1960–1974 Δ in % high school	0.038	0.031	0.092	0.078
Effect of Δ in birth order	0.003	0.008***	0.017**	0.001
	(0.003)	(0.003)	(0.007)	(0.007)
Effect of Δ in cohort size	0.001	–0.003	0.004	0.006
	(0.002)	(0.002)	(0.006)	(0.006)
Total effect	0.004	0.006	0.021**	0.006
	(0.004)	(0.004)	(0.010)	(0.009)

Notes: Birth order and cohort size data are from Vital Statistics. Educational attainment data are from the 1960–2000 censuses and 2006–2017 ACS. We multiply changes in birth order by sex-specific coefficients reported in Table 4, and we multiply changes in cohort size by the estimate reported in Table 5, column 4. These estimates correspond to changes in the appropriate counterfactual series plotted in Figure 6.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$