Multigenerational Benefits of Women's Education*

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Abstract

Women's rise in college attendance – attaining and then surpassing parity with men – has been one of the largest shifts in post-secondary education in recent decades, yet little is known about how this has affected women's careers and families. This paper uses a regression discontinuity design based on individual birth dates and school entry laws to evaluate these impacts. We find that women born just before the school entry cutoff attend college at higher rates and experience significant career improvements: they work more, hold more selective jobs with better amenities, and earn more. Further, most of the additional jobs at the cutoff require *exactly* some college, suggesting a direct link between career improvements and increased post-secondary education. Career effects grow larger in mid-career and, when monetized, imply substantial returns to education for women. Finally, we find positive effects on children's health, but only once job amenities and earnings improve—consistent with a complementarity between better maternal careers and child investments. The results indicate that investments in women's college attendance may be among the most effective policies to improve maternal careers, with spillovers to the next generation that grow over time.

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Women's rise in college attendance – attaining and then surpassing parity with men – has been one of the largest shifts in the landscape of post-secondary education in recent decades. The growth in two- and four-year college attendance was accompanied by a relative rise in women's labor force participation, advancement into professional work, and career ambitions (Goldin, 2006; Goldin et al., 2006; Blau and Kahn, 2017; Leukhina and Smaldone, 2022). Even today, there is a strong association between women's education and the decision to work (Black et al., 2017). Nevertheless, it remains unclear whether these associations reflect the causal effect of *education* on women's careers, or other correlated decisions, such as differences in marriage and fertility. There is also limited evidence about spillovers of these shifts to the next generation, particularly during critical stages of early development.[1]

These questions are complicated by the fact that traditional measures of labor market success may not fully capture education's value for women's careers. Recent work suggests that women value many job amenities, such as social meaning, as highly as moderate wage increases (Maestas et al., 2023; Kesternich and De Schouwer, 2023). Education may enable women to access such amenities, while also helping buffer against costly productivity shocks like childbirth, a major contributor to the gender gap in earnings (Cortés and Pan, 2020). Yet, to date, there are no estimates of the return to education that incorporate these potential benefits.

This paper studies the impacts of greater schooling on women's work, job amenities, and children's well-being. Following Angrist and Krueger (1991) and refinements in McCrary and Royer (2011), we use a regression discontinuity design based on individual birth dates and school entry laws to evaluate the effects of completing more education. We draw on employment surveys, administrative earnings records, and confidential health records of mothers from California to measure intent-to-treat effects of being born before the entry cutoff on multigenerational outcomes. We also newly trace how these benefits evolve over the life cycle, from early-career (ages 16-24) through mid-career (ages 25-40).

We first establish that the entry cutoff affects completed education, without influencing fertility decisions. Women born just before versus just after the entry cutoff have, on average, roughly 0.10 more years of education, and are about 2.2 percentage points (p.p.) more likely to graduate from high school, 2.1 p.p. more likely to complete some college, and 0.5 p.p. more likely to have a college diploma. These education gains are persistent over the life-cycle, although by mid-career they are concentrated in higher rates of completion of some college. At the same time, women's pre-determined characteristics, propensity to have a child, and fertility timing are all well-balanced across career stages. This provides reassuring evidence that birth dates are not manipulated around the cutoff, validating our research design.

When we follow women over time, we find that higher education manifests in better outcomes

[1]For effects on children in early life, see Currie and Moretti (2003); McCrary and Royer (2011) and for effects in late-childhood and adulthood, see, e.g., Black et al. (2005); Oreopoulos et al. (2006); Björklund et al. (2006); Sacerdote (2007); Akresh et al. (2022).

later in life for themselves and for their children. First, women born just before versus just after the entry cutoff have substantially greater labor market engagement and higher earnings. On average, they are 0.6 p.p more likely to have worked in the last year and earn $703 more annually. These effects grow over women's careers: by mid-career, the employment gap increases to 0.9 p.p. and the earnings gap to $975. This growth appears to be partly driven by women working more after having children—a smaller child penalty.

Second, women born just before the cutoff are more likely to hold jobs that have greater social status and are more selective, particularly in mid-career. This includes increases in the shares of women holding professional jobs and jobs that require a college credential. College-educated positions can account for at least 61% of the additional jobs at the cutoff, suggesting that these education credentials increase access to desirable jobs. Impacts on job selectivity grow over a career, consistent with the increasing effects on earnings.

Third, job quality improves along other dimensions. In particular, women born just before the cutoff have jobs that are more prestigious, have higher-educated co-workers, and have greater social impact, among other better working conditions. This partly reflects more women becoming educators, jobs that feature many of these amenities. Like the effects on selective positions, these impacts on job amenities also grow in mid-career. Importantly, these work upgrades do not appear to come at the cost of greater effort—women don't work more hours per week or weeks per year, though their hours are more concentrated during typical work days (i.e., 8am to 5pm).

Fourth, we find that children's health at birth improves at the cutoff, but only once women advance into better jobs in mid-career. In mid-career, child birth weight increases by 0.4 percent (9 grams) and gestational age lengthens by 0.5 days. In contrast, there are no effects on children when women are in early career. Notably, since fertility timing is not affected by the cutoff, these effects cannot be explained by changes in *who* gives birth at different career stages.

Prenatal *inputs* also improve alongside careers, with mid-career reductions in smoking and less use of public insurance. We estimate that changes in maternal circumstances – such as earnings and smoking – can account for at least 75% of changes in infant weight. Thus, first-generation gains appear to meaningfully shape outcomes for the second generation.

An important question for interpreting these results is *why* women's careers improve around the entry cutoff – particularly at mid-career, where we find stronger effects. Prior work identifies two likely channels: (i) greater education, which may include improved labor market signals, credentials, or human capital (e.g Angrist and Krueger, 1991) and (ii) younger age-for-grade, which may include different peer interactions in the classroom or earlier work experience (e.g Fredriksson and Öckert, 2013; Cascio and Schanzenbach, 2016). To distinguish between these channels, we study how the cutoff affects jobs' education requirements, and how closely these align with the rise in some college. If greater education is a primary mechanism, women should be more likely to have jobs that require *exactly* some college – mirroring the education effects – and should not be more likely to have jobs

that require lower or higher levels of education. Instead, if younger age-for-grade is the main channel, the effects on jobs are should be more uniform across job skills.

In strong support of the education channel, we only find effects on jobs that require *exactly* some college. We find a precise zero effect on jobs with more or less demanding education requirements, including those that require a college degree or passing an exam. This suggests that greater post-secondary education in particular is likely to explain women's career benefits. Considering alternative mechanisms, we do not find evidence for changes in partner quality or partner presence. This may in part reflect the fact that women with some college tend to choose partners from the same pool as lower-educated women (Lundberg et al., 2016).

Assuming that education is the *only* mechanism at the cutoff, we find a substantive implied return to education investments for women's careers. Each additional year of education raises women's employment by 15-16 p.p., earnings by 77%, and earnings conditional on working by 57% in mid-career. These career returns are higher than the average gains from two-year colleges, but align with the impacts of the most effective credentials. They can also explain most, if not all, of the 45-point employment gap between high school dropouts and women with some college. Together, this suggests that women, and mothers in particular, achieve significant gains from attending college.

We also show that education-induced improvements in job amenities meaningfully increase worker welfare. Using existing willingness-to-pay estimates for working conditions (Maestas et al., 2023), we find that these improved amenities provide as much value as a 3% yearly pay increase per year of education. This implies that amenities add an extra 6% to the labor market benefits beyond the 57% direct wage gains, representing a small but important share of education's total value to workers. Thus, wage-based estimates understate education's full labor market value.

Finally, we use the long span of infant outcomes in the birth records to study how the intergenerational benefits of maternal education have changed over time. Using the same research design applied to a moving window of birth records, we find that the impact of the cutoff on child birth weight has increased substantially over the last three decades. This growth entirely reflects a stronger *transmission* of education benefits, since the direct effect on mother's completed education has remained stable over time. Since gains in birth weight are associated with greater career success in adulthood, these findings suggest that maternal education may have an increasingly larger role in children's economic mobility (Black et al., 2007; Royer, 2009; Bharadwaj et al., 2017).

These results rely on the identification assumption that birth dates around the entry cutoff are effectively random. The balance tests described earlier provide compelling evidence for this assumption. As additional support, we show that the discontinuities in outcomes are visibly apparent and occur precisely at the entry cutoff. This mitigates potential concerns about unobserved seasonal differences in birth composition, which would produce more gradual changes (Buckles and Hungerman, 2013). We also show that placebo entry cutoffs do not reproduce the main results, confirming that outcomes are typically similar within narrow windows of birth dates. Finally, outcomes for

mothers in our sample match those for women more broadly, supporting the external validity of our results.

This paper makes three primary contributions. First, we provide new evidence on how maternal post-secondary education affects both mothers and children over the life cycle. Earlier work studies related questions in isolation: the average impact of two-year credentials on women's earnings (e.g. Jacobson et al., 2005; Stevens et al., 2019; Grosz, 2020; Mountjoy, 2022; Minaya and Scott-Clayton, 2022); the average effect of maternal education on child health (Currie and Moretti, 2003; Lindeboom et al., 2009; McCrary and Royer, 2011); and the life-cycle effect of schooling for men (e.g., Bhuller et al., 2017; Nybom, 2017). However, to date, there is no unifying evidence on the life-cycle effects for mothers and their children together.

This gap leaves many questions unanswered about how education's returns in the labor market interact with motherhood and with child investments—whether these are complements or substitutes. We find that education has modest effects on both labor market and child outcomes early in mothers' careers, but these effects grow substantially and stabilize at higher levels by mid-career. Further, we show that this growth in women's careers is reflected both in their earnings and job amenities, and that these effects become slightly stronger after they become mothers. This indicates that early-career measures likely underestimate education's long-term benefits for both careers *and* the next generation. Moreover, the simultaneous timing of effects on women's professional success and child investments suggests that these are complements rather than substitutes. Thus, women's labor market gains may be an important mechanism through which education benefits children's long-run outcomes (e.g., Black et al., 2005).

Second, we show that post-secondary education improves job amenities, and that these upgrades contribute meaningfully to the return to education. This adds to a sparse literature on education's effects on job titles, which finds that obtaining a college diploma in France raises men's propensity to be in elite occupations (Maurin and McNally, 2008) and that admission to selective college majors increase employment in related industries (Bleemer and Mehta, 2022; Grosz, 2020). We extend this work in two ways. We examine effects on a broad set of job amenities, which allows us to capture how schooling affects job mobility and the overall quality of one's work environment. We also use novel estimates of workers' valuations of working conditions (Maestas et al., 2023) to monetize the effects of schooling on amenities for the first time. Our estimates imply that schooling-induced improvements in job amenities substantially increase worker well-being, separate from effects on earnings.

Third, we contribute to the work on the long-run effects of the school entry threshold on labor market outcomes (Dobkin and Ferreira, 2010; Black et al., 2011; Fredriksson and Öckert, 2013; Oosterbeek et al., 2021). Relative to the other evaluation of this policy in the U.S., which finds null effects on careers, we examine a more extensive set of labor market measures—including labor force participation, administrative earnings, occupations, and workplace amenities—which provides

a more complete assessment of policy impacts (Dobkin and Ferreira, 2010). This leads to a different conclusion: early school entry has substantial positive effects on women's professional development. We also develop a new approach to distinguish between the effects of younger age for grade and greater years of education, based on occupational skill requirements. This could be a useful diagnostic for future research on this policy to understand the relative importance of these mechanisms.

1 School Entry Policies and Conceptual Framework

To study the multigenerational effects of women's schooling, we rely on a sharp increase in completed education for individuals born just before versus just after the school entry cutoff date. This research design has been used to study related questions on the return to schooling, such as in Dobkin and Ferreira (2010) and McCrary and Royer (2011). In this section, we provide institutional background on the policies in California that generate this discontinuity, and provide an organizing framework that we use to consider the potential channels for downstream effects.

1.1 Background on School Entry and Leaving Laws

California has two age-based school policies that help explain the break in education at the entry cutoff. First, children can only legally enroll in kindergarten if they turn five by the prescribed cutoff date during the academic year. For the women in this study, the relevant cutoff date was December 1 or December 2 (Gregory, 1976, 1987; Dobkin and Ferreira, 2010).[2] This entry rule leads to a sharp change in a child's expected and actual school entry age based on their birth date. Two girls born on opposite sides of the cutoff are expected to begin school one year apart – at ages 4 years 9 months and 5 years 9 months. The actual entry age gap is 0.75 years, which indicates strong, but imperfect compliance with this policy (Elder and Lubotsky, 2009). Notably, girls comply with these policies more than boys, so the discontinuity for girls may be even larger (Schanzenbach and Larson, 2017).

Second, California students must stay in school until age 18 or high school graduation. Students who enter school earlier can therefore complete more grades before reaching the exit age. Under normal grade progression, early entrants will finish 12^{th} grade before turning 18, while late entrants will only finish 11^{th} grade by age 18. In line with this, prior work shows that early entrants are more likely to finish high school and attend college (McCrary and Royer, 2011; Dobkin and Ferreira, 2010).

[2]The cutoff date changed from December 1 to December 2 in 1987 (Gregory, 1987). We assume the new cutoff applied to children born in 1983 onward, who turned five in 1988.

1.2 Expected Effects of Early School Entry on Careers

While we aim to identify the effect of education, early school entry may affect later-life outcomes through other channels. To guide the interpretation of the results, we outline these potential mechanisms and develop empirical tests to help distinguish the effects of education from alternatives.

Greater years of schooling First, early entry may improve labor market outcomes by increasing years of education—especially through higher rates of high school graduation and college entry. For example, women who attend two-year colleges have been shown to earn substantially more (Jacobson et al., 2005; Stevens et al., 2019; Grosz, 2020; Mountjoy, 2022). This may reflect the productive skills that are taught in college courses (Arteaga, 2018) or better access to higher-paying jobs (Kleiner and Soltas, 2019). Greater schooling could also affect other dimensions of careers, such as working conditions or coworkers.

Younger age-for-grade Second, early entry makes children younger relative to their classroom peers. Unlike additional schooling, the effect of this channel on career outcomes is much less clear. On the one hand, younger children in class tend to perform worse on exams in early grades, and have higher rates of ADHD diagnoses (Bedard and Dhuey, 2006; Elder and Lubotsky, 2009). On the other hand, it is not clear that younger age-for-grade affects long-run outcomes, and any impacts (positive or negative) appear to be concentrated among men (Black et al., 2011; Bedard and Dhuey, 2006; Cascio and Schanzenbach, 2016). When younger grade-for-age does improve careers, these effects appear to be temporary, and stem from early work experience gained from finishing school sooner (Black et al., 2011; Fredriksson and Öckert, 2013).

Partner improvements Finally, the effects of greater schooling or younger age may be mediated through changes in one's choice of partner; such as through changes in assortative mating or exposure to older peers (Lefgren and McIntyre, 2006; McCrary and Royer, 2011; Geruso and Royer, 2018). However, the career implications of these changes are ambiguous—better partners may either amplify or detract from one's own career ambitions.

Empirical tests To distinguish whether greater education or other channels drive our results, we develop two empirical tests. First, to separate the effects of greater schooling and younger age-for-grade, we study the impacts of the cutoff on the required education credentials for women's jobs, and how closely these align with the effects on education. The logic behind this is straightforward: if education is the primary channel, these should align closely. For example, suppose that the cutoff only increases completion of some college—if schooling is a primary channel, women should be more likely to have jobs that require exactly some college, but not jobs that require a college diploma. In contrast, if younger age-for-grade is the main channel, job impacts should be spread more evenly

across skill levels. We test this by comparing effects on jobs whose education requirements are the *same* versus *differ* from the effects on completed education at the cutoff. We then use the ratio of these effects to measure the relative contribution of schooling versus age-for-grade.

Second, we study the role of partner selection by testing for changes in partner characteristics. These include a partner's age, which may be influenced by the composition of classroom peers; as well as a partner's completed education, employment, and occupation, as indications of changes in assortative mating. These detailed measures allow us to distinguish between different sources of changes in partner selection, as well as to consider the scope for potential impacts on careers.

2 Data

Our analysis draws on two main data sources: the microdata of birth certificates from California and administrative earnings records merged with Census surveys. Together, these data provide extensive information on women's education and careers over the life cycle, and their children's early life development. Below, we describe how we compile these outcomes across sources.

Vital Statistics birth records Our first main data are birth certificate records from California from 2007–2017.[3] These records include detailed information on parents *and* children, including mothers' education and profession, and children's health and prenatal investments, from forms filled out at the time of childbirth. This wide breadth of outcomes at a point in time allows us to examine potential pathways for the intergenerational transmission of schooling. Our core data begin in 2007 because this is when parental labor market outcomes appear; however, where relevant, we supplement these data with earlier records going back to 1960 (e.g., to calculate fertility rates).

For parents, we observe background characteristics, such as name, date of birth, state of birth, race, as well as completed education and recent work history. We determine a mother's age at first birth by using personal identifiers to construct her fertility history. Educational attainment is binned; we use these bins directly as outcomes and to predict completed years of education.[4]

Parents' professional details include their typical occupation and industry, and their last month worked. Relative to typical labor market surveys, parents' job titles in the birth records are unstructured, which allows for more granular descriptions; and can include roles outside of the labor force, such as homemaker or student. For tractability, we harmonize the raw occupations using the Census Bureau's 2010 classification system (see Appendix A.1). We measure a woman's labor market engagement based on her recent work history (and not being a student) and whether she

[3]The birth records are provided by the California Department of Public Health, with the approval from the Committee for the Protection Human Subjects.

[4]To predict years of education, we use earlier records from 1989 to 2005. We regress completed years of education on the four education categories, race, age, and maternal year of birth (linearly). We then use the estimated coefficients to generate predicted values for our sample.

has an occupation in the labor force.[5] We show that trends in these outcomes are highly consistent with national surveys and admin. records (see Sections 3 and 5.2).

For children, we observe precise markers of early life health and prenatal care. As indicators of well-being, we use a child's birth weight and gestational age, due to the strong correlations with later-life cognition and labor market outcomes (Black et al., 2007; Figlio et al., 2014). To capture maternal investments, we study three types of health inputs: smoking during or preceding pregnancy; prenatal doctor visits, and use of public health insurance (Medi-Cal) or nutritional assistance (WIC).

Census and IRS records Our second main data are tax records from the Internal Revenue Service (IRS) linked to population surveys from the Census Bureau. This provides us with a panel of individual and household earnings, which we use to trace income trajectories over a career.

We obtain data on annual earnings from information reported to the IRS from 1998-2021, which has been compiled in the Opportunity Databank (Chetty et al., 2018). We measure individual earnings using wages reported by employers on IRS W-2 forms. We measure household earnings using the total wage and salary income reported on Form 1040 tax filings, when available, or otherwise, the sum of W-2 earnings for oneself and one's spouse. We adjust all monetary values to 2016 dollars using the CPI (U.S. Bureau of Labor Statistics, 2018).

We add demographic characteristics to the earnings panel by drawing on a variety of Census administrative and survey resources. We use the "best race" variable from the Opportunity Databank, which is derived from self-reported race in the 2010 Decennial Census and 2005-2020 ACS surveys. We link mothers to their children using the Census Household Composition Key, for births after 1997 (Genadek et al., 2022), or the 2000 Census, for births before 1997. We then use the Numident file to attach the exact date and location of birth for each individual.

Job characteristics To characterize the status, selectivity and working conditions of parents' occupations, we assemble a wide range of average job characteristics for U.S. workers. See Appendix A.2 for details. Our measures of job status and selectivity include an indicator for being a professional (from Autor and Dorn, 2013); a percentile ranking of occupational prestige (from Hughes et al., 2022; Smith and Son, 2014); the minimum years of education required to obtain a license or credential; and the share of workers in an occupation who attended some college. Our measures of working conditions include the twelve job amenities evaluated in Maestas et al. (2023); the degree of scheduling inflexibility (from Cubas et al., 2019); the average hours worked per week; and the average hourly wage. When available, we use job characteristics specifically for California workers.

[5] We set the work indicator to zero for students because they typically work part-time. We treat it as missing for the 1% of individuals who report last working more than a year prior to childbirth, as many of these dates appear unrealistic.

Analysis samples We use two criteria to identify women who are likely to be affected by California's school entry policies. First, we focus on mothers who were born in California and had their first birth in California, who are likely to have been in the state during childhood. Second, we limit to mothers who have a first birth by age 31, who empirically are more likely to be compliers of entry cutoffs (see McCrary and Royer, 2011, and Section 6).[6,] Within this sample, we focus on women ages 16 to 40, based on data availability. We apply these criteria separately in the birth and earnings records, which allows us to avoid linking these sensitive data.

Finally, we use two other minor criteria to construct our final analysis samples from the birth and earnings records. For the birth records, we limit to each woman's first two births (the complete fertility history for 80% of mothers), so that analytical weights are roughly balanced across women. For the earnings records, we limit to the 1977–1987 cohorts, for whom we observe outcomes for all ages from 21–34. This ensures that we have a balanced panel when we estimate earnings effects over the life cycle. We recreate this sample in the birth records to analyze effects on fertility and education.[7] We show that our results are robust to using alternative sample criteria in Section 6.

Appendix Table A.7 compares our birth records sample (column 3) to the broader population of California mothers (columns 1-2). Relative to all mothers and California natives, our sample has similar rates of high school graduation (85%) and employment in the previous year (77%), but fewer women with a college education and a younger average age. Despite these differences, the sample captures 92% of all non-college-educated mothers in California. Sample characteristics remain similar when we limit to women born near the entry cutoff (column 4). Women in the earnings panel (column 5) have similar demographic characteristics but are observed at older ages since our main outcomes are only available for recent years.

Appendix Table A.8 compares the samples from the birth records and the earnings records. It shows that we are able to successfully recreate the birth records sample across datasets: the demographic characteristics and sample sizes are nearly the same (differing by less than 10%). Thus, our results across data sets can largely be interpreted as coming from the same sample.

3 Descriptive Evidence

As motivation for the analysis, Figure 1 presents descriptive patterns linking women's completed education, their careers, and their children's health. This allows us to evaluate the scope for early school entry to transform women's professions and, in turn, their children's well-being. We also validate these trends by matching the results from the birth records to those for a similar sample from the American Community Survey (ACS) (Ruggles et al., 2010).

[6]Higher compliance among young mothers may reflect their disadvantaged backgrounds, including having younger parents and coming from lower-educated counties (see Appendix Figure A.1).

[7]We use the full span of education data from the birth records (1989 onward) to analyze education effects for the earnings sample, which allows us to capture outcomes at younger ages.

The two top panels of Figure 1 show a steep education gradient in women's work (Panel A) and professional occupations (Panel B), traced across four levels of education: no high school diploma, high school diploma/GED, some college, and college or more. Rates of work rise substantially across these groups – from 37 percent to 72 percent, 81 percent, and 93 percent. Professional employment also increases with education: 16% of working women without any college hold professional jobs, compared to 31% with some college and 75% with a college degree. When including non-workers, these gaps widen further – professional jobs are four times as common among women with some college than high school dropouts (25% vs. 6%). Reassuringly, these trends are very similar across sources (although rates are slightly lower in the ACS).

The bottom two panels of Figure 1 show a similarly strong link between maternal education and infant health, which appears to be potentially moderated by women's careers. Panel C demonstrates that child health correlates positively with maternal education and employment. Birth weight differs by nearly 100 grams between least- and most-educated mothers. Further, for every level of education except college, children of working mothers have an 50-gram advantage over children of non-working mothers. This suggests that children of higher-educated mothers may in part benefit from having a working mother (e.g., due to having greater financial resources).

Panel D reveals that infant weight also correlates with maternal occupation. The markers shows the child outcomes for each maternal occupation, which are ordered by the profession's college attendance rates. The positive slope indicates that children of higher-educated mothers fare better (as in Panel C). However, the vertical spread of the markers suggests that job-specific factors may matter within education levels. For example, teachers have particularly good child outcomes (above the best-fit line), while nurses' children have outcomes below expectations (below the best-fit line). Thus, work *type* may mediate the relationship between maternal education and child health.

4 Estimation and Identification

To study the multigenerational impact of early school entry, we leverage the quasi-random variation in women's birth dates relative to the entry cutoff. Specifically, we estimate intent-to-treat effects of being born before the cutoff using a regression discontinuity (RD) design:

$$Y_{ia} = \alpha + \delta \text{BeforeCutoff}_i + \gamma \text{DOB}_i + \phi \text{BeforeCutoff}_i \times \text{DOB}_i + X_{ia}\beta + \rho_b + \varepsilon_{ia} \tag{1}$$

where Y_{ia} is an outcome for woman i (or her child) when she is age a; the running variable is DOB_i, which is the difference between i's birth date and the entry cutoff for her cohort; and BeforeCutoff_i is an indicator for $\text{DOB}_i < 0$. To increase precision, we include a fixed effect for birth year, ρ_b; and a vector of other individual covariates, X_{ia}, which includes fixed effects for a woman's age and race and, for child outcomes, for child parity and having multiple births. We recenter year of birth and age around the entry cutoff, so that these measures take on the same value for individuals born

nearby around the cutoff.[8]

The key coefficient of interest is δ, the impact of being born just before the cutoff. Because these effects may vary over the life cycle, we produce separate estimates for women in early career, ages 16 to 24, and in mid career, ages 25 to 40. Our baseline estimates use triangular kernel weights, a bandwidth of 70 days around the cutoff (based on the range of optimal bandwidths across outcomes using Calonico et al. (2014)) and cluster standard errors on the running variable. However, we show that the results are robust to alternative estimation schemes in Section 6.

4.1 Identification Assumption and Supporting Evidence

This design relies on the assumption that birth dates around the cutoff are effectively random, such that women on either side of the cutoff have the same potential outcomes. In our sample, this could potentially be violated in two ways. First, the types of women who are *born* on opposite sides of the cutoff may differ. This could occur if birth timing around the cutoff is systematically correlated with parent characteristics, such as from differences in seasonal jobs or health patterns (Buckles and Hungerman, 2013). Second, conditional on being born in California, the types of women who become mothers in California may differ around the cutoff. This could arise if early school entry affects either women's fertility or interstate mobility. However, prior research does not find evidence of these effects (McCrary and Royer, 2011).

We address these concerns using two complementary tests. First, we use binned scatter plots to show that our effects on outcomes occur precisely at the cutoff. This allows us to rule out any concerns related to seasonal variability in births, which would not create such sharp effects. To replicate the main variation in the analysis, we residualize outcomes of cohort fixed effects (but no other controls). This allows us to provide greater transparency into the underlying data; while avoiding confounding changes in birth timing with the effects of the cutoff.

Second, we test for balance in women's fertility and covariates around the cutoff. To measure effects on fertility *rates*, for each value of the running variable, we divide the total number of mothers by the total number of females who were born in California on that date. Because fertility responses may vary with maternal cohort or demographics, we estimate effects separately for the birth records and earnings samples, and by maternal race/ethnicity (Black et al., 2008).

Table 1 presents impacts on two dimensions of fertility: the likelihood of having a birth and maternal age. Across the two samples (shown in Panels A and B), we find no significant effect of the cutoff across a variety of fertility measures; including the likelihood of having any birth, a first birth, or second birth in the sample; and the average age at any birth, at first birth, or at second birth. We continue to find largely null effects when we stratify the sample on race (Appendix Tables

[8]This recentering avoids confounding treatment effects with differences in calendar years or ages around the cutoff. We also trim the ends of the data to ensure that age is balanced within recentered cohorts. For example, we trim the first six months of the birth records data (Jan.-June 2007) because we only observe birthdays for women to the right of the cutoff for that recentered year.

A.2 and A.3). Across 48 estimates, the only marginally significant change is in age at first birth for Black women in the birth records. Hence, fertility appears to be broadly unaffected by the cutoff.

Nevertheless, to be conservative and eliminate any role for selective fertility patterns, we exclude Black women from our baseline results. The remaining sample is balanced on fertility (Panels C and D of Table 2); the distribution of maternal ages (Appendix Figure A.2); and demographic characteristics (see Appendix Tables A.4 and A.5). We also visually confirm that the density of the running variable is continuous, which provides additional support that birth dates are not manipulated around the cutoff (see Appendix Figure A.3) (McCrary, 2008). Appendix Table A.8 shows summary statistics for this sample.

5 Results

5.1 Effects on Completed Education

Figure 2 shows a binned scatterplot of years of education for women born within 100 days of the cutoff. For transparency into the effects across samples, we plot separate markers for the birth records sample (in dark gray) and earnings sample (in light gray). Panels A–C show the averages for all women, early-career women (ages 16-24) and mid-career women (ages 25-40). We report corresponding regression estimates in Appendix Tables A.6 and A.9.

Across the three panels, women born before the cutoff consistently have higher completed education. In early career, this advantage ranges from 0.15–0.16 years, and in mid career it spans from 0.05–0.06 years. This narrow range of estimates demonstrates the substantial overlap across samples. Further, the increase in schooling occurs precisely at the cutoff, consistent with school entry policies. In contrast, alternative explanations, like differences in family background, would produce more gradual shifts rather than a sharp jump.

Figure 3 unpacks the increase in completed schooling, and shows that these effects stem from greater persistence in secondary and post-secondary education. In early career, this includes increases in high school graduation, some college, and college completion. However, by mid-career, only the effects on some college persist; remaining consistently large and statistically significant across samples. The improvements in college attendance vary from 1.2–1.5 p.p., a 2 percent increase over the average. These effects also occur exactly at the cutoff (Appendix Figure A.4).

For analyzing long-run outcomes, it is useful to distinguish whether this increase in education represents more total schooling or simply earlier completion of the same schooling. We study school attendance rates to test these alternatives: if early entrants are simply finishing *sooner*, they should have lower attendance rates; while *greater* schooling would imply similar rates of attendance. In fact, we do not find any mid-career impacts on school attendance (Appendix Table A.6).[9] This

[9]We only show these effects for the birth records sample because school attendance is only available after 2007, when most of the earnings sample has reached mid-career.

suggests that our impacts on education are likely permanent.

5.2 Effects on Careers

To provide a comprehensive accounting of impacts on women's careers, we draw on a wide range of measures of labor market success; including standard measures of compensation, as well as more nuanced metrics of job quality. First, we use our earnings panel to estimate the life-cycle effects of the cutoff on women's earnings and employment. Second, we use surveys of new mothers to study the evolution of impacts on professions and job amenities over a career.

Earnings and employment To begin our investigation of labor market outcomes, Figure 4 presents career-averages of annual earnings (Panel A) and employment (Panel B) around the cutoff. We find that both measures increase sharply for women born before the cutoff, consistent with women's educational gains. Column 1 of Table 2 shows that these earnings improvements amount to an additional $556 per year, a 3.8 percent increase over the baseline average (Column 1 of Table 2). Over the 25-year observation window, this translates to $13,900 in additional lifetime earnings per woman. Employment rates also rise by 0.5 p.p. annually, which represents a 1% increase relative to the baseline (Column 2 of Table 2).

Next, we examine *when* these career effects emerge over the life cycle. One possibility is that early school entry may be most influential early in women's careers, by helping them to develop a strong career path when they are younger. Conversely, these benefits may compound over time, becoming more pronounced as women transition into more stable, long-term professional roles— in line with the evidence of increasing returns to education over the life cycle (Lagakos et al., 2018, e.g.). To distinguish between these hypotheses, we estimate the impact of the cutoff separately for each age in the panel (ages 22 to 35), tracking how treatment effects evolve across women's early-to-mid career years.

Figure 5 presents age-specific impacts on earnings (Panel A) and employment (Panel B), along with 95% confidence intervals. We find gains in both earnings and employment at every age that increase steadily over women's careers. The rise in treatment effects is steepest at younger ages, before age 30, after which the impacts grow more slowly or stabilize.

We summarize these patterns in the last four columns of Table 2. Women born before the cutoff earn $177 (6.6%) more in early career; and by mid-career, their earnings advantage grows to $758 (3.6%). They are also 0.7 p.p. more likely to be working in mid-career, compared to smaller and less-precisely-estimated impacts in early career. Greater employment only partly explains higher earnings, however; earnings conditional on working also increase in early and mid-career (Appendix Table A.10).

Finally, Panel C of Figure 5 examines whether higher earnings translate into greater *household* income. This may not be the case, if for example, there are offsetting reductions in spousal labor

supply. However, the figure shows that women born before the cutoff have higher household income at every age, and that these impacts grow over time. In addition, the size of the effects suggest that family resources increase one-for-one with with women's greater earnings (see Appendix Table A.10).

Child penalty Given that motherhood represents a critical career transition, we examine whether these labor market effects change with its onset. Ex ante, the expected direction of this potential interaction is not clear. Motherhood could either *reduce* earnings effects if early entry increases maternal investments (i.e., larger child penalty), or *increase* earnings impacts if early entry strengthens labor force attachment or improves access to child care (i.e., smaller child penalty). We test for these competing possibilities by comparing the size of our effects before and after childbirth.

Figure 6 shows the RD estimates for earnings and employment in the three years leading up to and four years following the birth of a first child. We estimate the impacts in each year separately, and use a balanced panel of women's outcomes around childbirth (which drops a small number of women with births close to the start of our data). We find small, stable increases in earnings before childbirth, consistent with the effects for young women in Figure 5. In the year after childbirth, the impacts increase from \$200 to \$470, and the effects remain modestly elevated for the next three years. This rise in the RD estimates indicates that women born before the cutoff have a smaller child penalty, which contributes in part to their income gains.

To provide context for these effects, Appendix Figure A.5 shows the baseline child penalty for women born after the cutoff, along with the RD-implied penalty for women born after the cutoff. At baseline, women's earnings are \$5,872 lower in the year after a first childbirth ($t = 1$). Our estimates suggest that women born before the cutoff experience a slightly smaller earnings drop—about 2% less severe ($\frac{400-270}{5872} = 0.022$).

Job profiles and amenities Shifting focus to women's professions, Figure 7 studies impacts on employment in the past year (shown in Panel A) and identifying as a worker (shown in Panel B).[10] This analysis serves dual purposes. First, we can validate the outcomes in the birth records, by taking advantage of the overlap in the working outcome with the administrative records. Second, we can capture potential changes in career identity, which may underlie changes in labor force attachment.

The two panels of Figure 7 show a remarkably consistent pattern of effects: the likelihood of working last year and the share with a career identity increases by 1.1 to 1.2 p.p. for women born before relative to after the cutoff (Table 3), with similar effects for early- and mid-career women. Compared to the effects on employment for the panel sample (above), these effects are slightly larger; suggesting that career impacts may generally increase for younger cohorts. Importantly, we

[10] These measures are related, but contain independent signals about work: 40% of non-working women still identify as a worker, and 20% of working women identify as stay-at-home mothers.

replicate these patterns in the tax records (see Appendix Table A.11), which validates that these outcomes are accurately reported in the birth records.

Next, we examine effects on women's career status and preferred work. As one proxy for job selectivity, we analyze an indicator for whether a job requires post-secondary schooling; since this is the highest-skill job attainable by marginal students at mid-career. As a proxy for preferred work, we focus on the social impact of jobs, a highly valued job amenity for women (Kesternich and De Schouwer, 2023; Burbano et al., 2020). Further, we identify women who are pre-school educators as being in a "high social impact" profession because it has the greatest impact among jobs that do not require a degree.[11,12] To avoid conditioning on having a job, we focus first on a set of unconditional job outcomes, and then expand to broader job conditions among workers.

As a first measure of having a more selective job, Panel A of Figure 8 investigates effects on the propensity to hold a professional job. In contrast to the effects on working, the cutoff has no significant effect on being a professional in early career. However, in line with the increasing effects on earnings, we find that the effects on being a professional manifest more strongly in mid-career. Women born just before the cutoff are 1.3 p.p. more likely to hold jobs in these top positions in mid career, relative to a baseline rate of 35.7 percent (see Column 1 of Table 4). This effect size implies that the increase in professional jobs can account for the entirety of the overall rise in work.

Column 2 of Table 4 shows effects on our second measure of job selectivity: having a job that requires post-secondary education. As with professional jobs, these effects grow substantially over a career. While women born just before the cutoff are 0.4 p.p. more likely to have a job that requires some college in early-career, this effect doubles to 0.8 p.p. in mid-career. This suggests that greater access to licensed work accounts for 61 percent of the rise in professional careers, since all of the jobs requiring a post-secondary credential fall into this category.

Similarly, women are more likely to be in a high social impact profession, with impacts that grow over a career. The point estimates range from 0.2 p.p., when we use the most narrow definition of preschool teachers, to 0.8 p.p., when we use a broader definition (Columns 3–4 of Table 4). The more conservative estimates suggest that high impact jobs make up at least one-sixth of the additional jobs at the cutoff. Hence, many marginal students appear to prefer careers in education.

When we shift to consider how the cutoff affects the careers of *workers*, we continue to find improvements in job status—particularly in mid-career. Panel B of Figure 8 and Column 5 of Table 4 show that occupational prestige of women born just before the cutoff increases by around 0.27 points, or roughly 1 percent. This reflects a shift into jobs in the top half of the prestige distribution, and out of less-prestigious work (Column 6). Colleagues in one's profession are also more likely have attended college (Column 7). Together, this suggests that workers born before

[11] In the American Working Conditions Survey, over 90% of pre-school teachers believe they have a positive impact most or all of the time, versus 26% of licensed practical nurses and 81% of registered nurses (Maestas et al., 2017).

[12] California preschool teachers must have a child development permit, which requires college-level course work. See `https://www.ctc.ca.gov/credentials/leaflets/child-development-permits-(cl-797)`.

the cutoff are held in greater esteem by the public, perhaps in part because of having better peers at work. These effects can not be explained by changes in selection into work (i.e., more capable women joining the workforce), as we show in Section 6.

These increases in job status also come with changes in working conditions. Appendix Figure A.6 presents mid-career effects on twelve other job amenities that workers value (Maestas et al., 2023). We find that the jobs of women born just before the cutoff have greater social impact (in line with the educator effects above); more team-based activities; and fewer self-driven tasks. In terms of work effort, we do not find any changes in weekly hours of work or weeks of work; however, scheduling flexibility declines slightly at mid-career (see Appendix Table A.12).

Overall, these results provide compelling evidence that improvements in women's careers extend beyond greater earnings. However, it remains unclear how much better amenities raise the *value* of career gains at the cutoff. We address this question in Section 7.2.

5.3 Effects on Infant Health and Prenatal Investments

Turning to the outcomes of the second generation, Figure 9 presents a binned scatter of the log birth weight of children (ordered by their mother's birth date relative to the cutoff). To parallel our analysis of labor market outcomes, Panel A presents effects for mothers in early-career, while Panel B presents effects for mothers in mid-career.

The two panels of the figure show increasing effects on child health over time, which mirror the gains in mothers' careers. In early career, the children of women born before the cutoff have no change in birth weight, consistent with the small effects on earnings and jobs for mothers at this stage. However, in mid-career, these children begin to exhibit clear improvements in weight, in line with the strengthening effects on labor market outcomes. The point estimates in Table 5 align with these patterns: children to the left of the cutoff weigh 0.4 percent more, or are 9 grams heavier at birth in mid-career (columns 1–2). In contrast, we find small imprecise null effects in early career.

Moreover, we find that improvements in child weight occur across the entire distribution, unlike previous research that focuses on education's role in preventing rare, adverse outcomes (Currie and Moretti, 2003; McCrary and Royer, 2011). In particular, while Appendix Figure A.7 shows that children are less likely to have very low birth weights (1,500–2,500 grams), this seems to explain a small share of the total weight gain. Instead, most of the weight increase appears to come from children reaching or slightly exceeding median child weight (3,000–3,500 grams). This aligns with recent evidence that interventions to improve mothers' prenatal care or physical environment are associated with broad health gains (Miller et al., 2022; Chyn and Shenhav, forthcoming).

Accompanying the rise in child weight, we also find suggestive improvements in gestational age. Column 3 of Table 5 shows that children to the left of the cutoff in mid-career are born 0.45 days later ($p < 0.10$), which amounts to a 0.1 percent increase in gestation. This suggests that lengthening gestation may contribute to gains in infant size. As with birth weight, these effects do

not manifest in early career.

To explore potential mechanisms for the effects on child health, we examine impacts on maternal inputs in the remaining columns of Table 5. The most striking result is that women born before the cutoff are 0.5 p.p. less likely to smoke (column 4)—a 12.5 percent reduction relative to the average. This could reflect changes in women's careers, such as working in education, having better peers, or earning higher income, or other channels that could influence risky behavior, such as improved access to prenatal care.

To test for changes in medical access, we examine effects on the reported number of prenatal visits (column 5). We do not find any change in this measure of the *quantity* of care. However, there may be other changes in medical care that are not captured by this outcome, such as improvements in provider quality or in the provider-patient relationship.

We also find that women born before the cutoff are less likely to participate in means-tested public programs. This includes a reduction in use of public health insurance (Medi-Cal) and nutritional assistance (WIC), each of which decline by around 1 p.p. If women are able to afford equivalent or better alternatives to these resources, this could suggest an improvement in health care or nutritional intake. More generally, the fact that changes in inputs accrue across multiple domains – maternal health capital, financial resources, and insurance – suggests that there is a substantial shift in the quality of the prenatal environment.

Importantly, these changes in prenatal investments can explain at least 54% of our effects on child health, based on existing estimates of their efficacy. For example, greater earned income in the year before birth alone can explain 4.0 grams (45%) of the increase in birth weight, based on the $628 increase in earnings (Appendix Table A.11) and a 6.4 gram effect per $1,000 of income (Hoynes et al., 2015). Reductions in smoking can explain another 0.9 grams (9.8%) of the improvement in health, based on the effect of smoking on birth weight (15 grams per cigarette per day) and the average number of cigarettes per day among smokers in the quarter prior to pregnancy (12) (Permutt and Hebel, 1989).

These estimates could also understate the true impact of these inputs, if earnings increases *prior* to pregnancy also influence child outcomes. This could occur if women use these additional resources to invest in complements to prenatal health. For example, greater earnings may allow women to move to safer housing, reduce their exposure to hazardous materials, or spend less time participating in risky behaviors.

6 Robustness

We now examine the robustness of the results to alternative estimation approaches and samples. The exercises focus on mid-career effects, where our patterns are strongest.

First, we test the sensitivity of the results across estimation windows and inference procedures. Our estimates remain stable when we use the mean-squared-error optimal bandwidth for each

outcome, which balances the bias-variance trade-off for the RD estimator (Calonico et al., 2014), and when we test a range of other bandwidths (Appendix Table A.13 and Appendix Figure A.8). Our conclusions are also unchanged when we implement alternative inference methods. This includes using Eicker-Huber-White robust standard errors to address possible under-coverage of clustering (Kolesár and Rothe, 2018), bootstrapped standard errors for the two-step estimation of years of education, and bias-corrected estimates and confidence intervals to correct for potentially "large" bandwidth selection (Calonico et al., 2014) (Appendix Table A.13).

Second, we relax our sample selection criteria to include a broader sample of women. Including Black women slightly reduces the "first stage" on education, but effects on other outcomes are qualitatively similar (see Appendix Table A.13). When we vary the cutoff for age at first birth, our findings are unchanged when we include older first-time mothers. However, we find larger effects when we focus on younger first-time mothers, consistent with these women being more marginal to school policies (Appendix Figure A.9). We also find similar effects when we change the age range for mid-career to be more or less inclusive (Appendix Figure A.10).

Third, we test whether the results could have arisen by chance using placebo cutoffs. We choose cutoffs within an eight-week window around the true cutoff, to capture possible seasonality around this period, and spaced apart by two-weeks. Across 48 placebo estimates, only two are statistically significant, and both go in the opposite direction of the main estimates (Appendix Figure A.11). The remaining estimates are typically small, inconsistently-signed, and uncorrelated across outcomes. This makes it highly unlikely that our systematic improvements in outcomes reflect statistical noise.

Fourth, we use Lee (2009) bounds to assess the role of selection into work for impacts on job amenities. Appendix Table A.14 shows that this sample trimming exercise generates tight bounds around our main estimates. Thus, improvements in workers' amenities are unlikely to be explained by changes in the composition of workers.

Finally, we address whether the different treatment effects across early- and mid-career women in the birth records could reflect distinct *populations* of women rather than true changes over a career. In particular, the varying treatment effects could reflect distinct LATE's across different types of women. As one piece of evidence against this explanation, we find the same pattern of increasing of career effects in the earnings panel, where the composition is fixed across career stages.

To test this directly, we rerun the analysis using a balanced panel of women where we observe each woman exactly twice.[13] If the larger treatment effects at mid-career are driven by selection, this balanced sample should lead to constant impacts across different ages (births) of the same mother. Contrary to this, Appendix Table A.15 shows that treatment effects increase when the *same* mother is older. This suggests that age effects, not selection, explain the growth in impacts across career stages.

[13]This sample includes mothers with first and second births between 2007-2017 (for work outcomes) or between 1989-2017 (for infant outcomes), spaced at least two years apart to create a gap in outcome timing.

7 Implications and Extensions

Having shown that the entry cutoff meaningfully affects outcomes across generations, we now return to examine the role of education in generating these changes. We then use this result to contextualize our findings in two ways. The first extension calculates the implied career return to education and compares this across studies. The second extension examines how the transmission of education to children has evolved across cohorts.

7.1 Mechanisms for Labor Market Gains

As discussed in Section 1.2, early school entry could affect careers through two main mechanisms: completing *more* years of education and finishing schooling *sooner*. To distinguish between these channels, we implement a new test that examines whether the job types affected by the cutoff align with our effects on post-secondary education. Specifically, we compare mid-career effects on two types of jobs: those that require exactly some college (matching our increases in education) versus those that require a college degree (implying effects of earlier schooling). Education-driven effects should appear only in the first category.

The results strongly support greater education as a primary mechanism. While we find that jobs requiring some college can account for at least 61% of new jobs at the cutoff (Section 5), we find no effect on jobs that require a college degree (Appendix Table A.16). We also find no effect on teaching positions within college jobs—indicating our increase in preschool teachers does not simply reflect greater interest in education. This credential-specific pattern is difficult to reconcile with women being younger relative to their peers, which would be expected to produce more widespread effects.[14]

The effects of education do not appear to work through changes in partner selection. Appendix Table A.17 shows that women born before the cutoff are no more likely to have a partner nor to have a successful partner (measured by completed education, employment, or professional status). This is consistent with evidence that women with some college tend to choose partners from the same pool as lower-educated women (Lundberg et al., 2016). The only difference is in partner age, which reflects matching within school cohorts. However, this alone cannot explain our main results.

7.2 Implied Labor Market Return to Education

If we assume that education is the *only* source of improvements at the cutoff, we can rescale our results to compare to education effects in other settings. To do this, we calculate the implied return to a year of schooling in the standard way—by dividing the cutoff's impact on career outcomes

[14] We also do not find an effect on less-selective positions that require only an exam. This supports our finding that career improvements come from access to more selective jobs.

by its impact on years of education.[15] For job amenities, we then convert this return to a wage-equivalent value, by multiplying by women's willingness-to-pay for these benefits (Maestas et al., 2023). This allows us to provide the first direct comparison of education gains from earnings versus other workplace benefits. Throughout, we focus on mid-career effects to capture longer-term benefits of schooling.

Employment and earnings This rescaling yields a steep labor market return to education. For each year of schooling, women's likelihood of working increases by 12 p.p. ($\frac{0.7 \text{ p.p.}}{0.06 \text{ years}}$), their total earnings rise by 61% ($\frac{3.6\%}{0.06 \text{ years}}$), and their earnings conditional on working grow by 44%, based on the estimates from the earnings panel.

These career returns are generally 10-15% higher than previous estimates from similar interventions, suggesting that mothers gain more from college than other groups. For context, the average return to a year of community college is around 10% (Jacobson et al., 2005; Mountjoy, 2022), and completing a one- or two-year credential yields a 5–45% increase in earnings (Stevens et al., 2019; Grosz, 2020; Minaya and Scott-Clayton, 2022). Our employment effects are comparable to the upper range of existing estimates. Prior studies find employment increases of 2 to 15 p.p. with a vocational degree (Stevens et al., 2019) and of 7.5 to 17 p.p. for women with short certificates or associate's degrees (Minaya and Scott-Clayton, 2022), and 11 p.p. for winners of a nursing program lottery (Grosz, 2020).

These estimates also suggest that increasing education is a highly effective policy to improve mothers' careers relative to other policy interventions (Olivetti and Petrongolo, 2017). For example, our employment return is 2–3 times larger than the well-known effect of expanding the Earned Income Tax Credit in the mid-1990's for young, single mothers (Michelmore and Pilkauskas, 2021; Kuka and Shenhav, 2024). Thus, education investments appear to offer particularly strong returns for supporting mothers' long-term economic outcomes.

Returning to Figure 1, our estimates also suggest that the causal effect of education can explain most, if not all, of the observed employment gaps across education groups. The estimated 12 p.p. increase per year of education can explain over 80% of the 44-point gap in work between women with some college and dropouts, assuming a three-year education difference. However, since our employment effects are imprecisely estimated, education may explain a smaller share – such as around 40% – of the gaps.

To explain the increase in earnings, we separate this effect into two sources: moving to better-paid professions and higher wages within the same profession. Using the average occupation wage and jobs reported at childbirth, we estimate that the shift in women's professions alone can account for $533 greater earnings per year in mid-career (see Appendix Table A.12). This comes from higher hourly wages and increased employment. These estimates suggest that better occupations explain

[15] This exercise also relies on a standard assumption that career outcomes vary linearly with years of education.

85% of the $628 increase in earnings at childbirth (Appendix Table A.11), with the remaining 15% coming from higher pay *within* occupations.

Job amenities Incorporating improvements in job amenities increases these labor market returns. For each year of education, better amenities (for the twelve amenities for which we have WTP estimates) contribute the equivalent of a 3.3% annual increase in wages ($p < 0.06$). This implies that amenities increase labor market benefits by 7.5% over the 44% direct wage gains for workers, accounting for a small but important share of the value to workers. Figure 10 shows that the amenity value comes from an increase in jobs with greater social impact, less physical strain, and more teamwork, offset by less independent work. However, this does not incorporate benefits from all amenities; including potentially valuable ones, such as from better-educated coworkers (Jencks et al., 1988). Hence, the true amenity value may be much higher than 3.3%.

7.3 Intergenerational Transmission Over Time

Finally, to provide context for the effects on infant health, we examine whether these child benefits represent *new* gains from maternal education or continue well-established patterns. Since infant health predicts long-run child outcomes, tracking these health benefits over time can help predict future long-run gains from maternal education (Black et al., 2007; Royer, 2009; Bharadwaj et al., 2017). We estimate RD effects separately for moving windows of nearly thirty child cohorts starting in 1989, when maternal education data became available.

Appendix Figure A.12 shows a clear increase in birth weight effects over time. Children born from the mid-2000s onward experience 8-9 grams increases in birth weight, while estimates that include the 1990s cohorts are small and statistically insignificant. In contrast, maternal education effects remain relatively constant across periods. This suggests that the *transmission* of education to children is strengthening over time. In turn, this could indicate that maternal education investments will have even greater effects on children's upward mobility going forward.

8 Conclusion

This paper presents new evidence that increases in women's education generate a cascade of effects that transform outcomes across generations. Using a regression discontinuity design that compares the outcomes of women born just before versus after the school entry cutoff, we show that those born just before the cutoff attend college at higher rates, have higher employment and earnings, and hold more selective and higher impact jobs. Each of these effects strengthen at mid-career, indicating that women born before the cutoff also have a steeper career *trajectory*. Career improvements are also mirrored in their children's early health outcomes and prenatal inputs—including higher birth weight and reduced maternal smoking. We show that impacts on

women's careers are driven primarily by increased college attendance, implying substantial returns to schooling that extend across generations.

Our results have three new implications for the return to education. First, amenities meaningfully add to the labor market returns to education. Back-of-the-envelope estimates suggest that improved job amenities from one additional year of education provide as much value as a 3.3% increase in annual earnings. This increases the total labor market benefits by 6% beyond the 57% direct wage gains—a modest but meaningful addition to education's value to workers.

Second, the education's benefits for mothers grow over the life cycle, despite the well-established work challenges for this group. We show that this in part reflects a smaller child penalty, suggesting that education may strengthen women's labor force attachment or improve access to child care. Our results suggest that investments in women's college attendance may be among the most effective policies to improve maternal careers.

Third, the simultaneous timing of improvements in maternal careers and child outcomes suggest that these are complementary outputs from education. This finding contrasts with some previous evidence that maternal employment detracts from child outcomes (Baker et al., 2008, 2019, e.g.). In our context, however, mothers work in better jobs – with more-educated peers and higher earnings—rather than simply working more. This raises the possibility that job quality, not just employment, may be important for positive spillovers from maternal careers to children.

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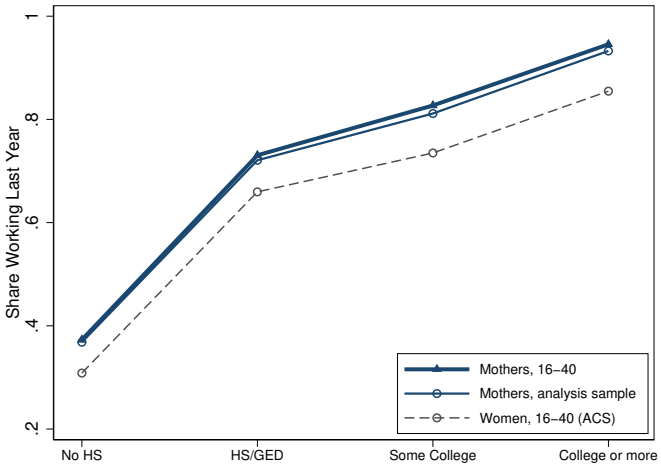
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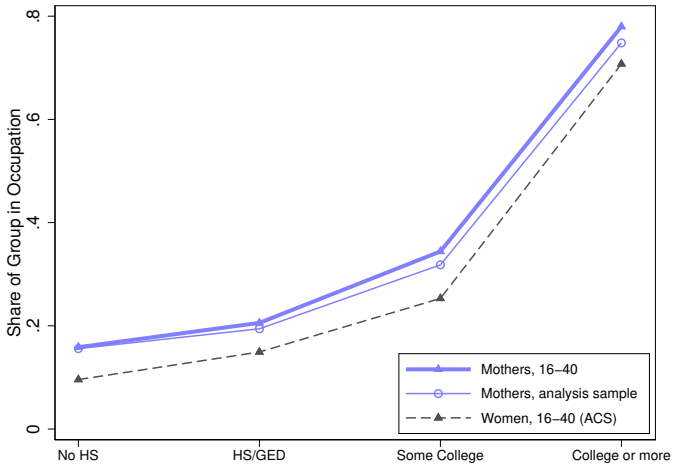
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Figure 1: Descriptive Links between Women's Education, Careers, and Child Health

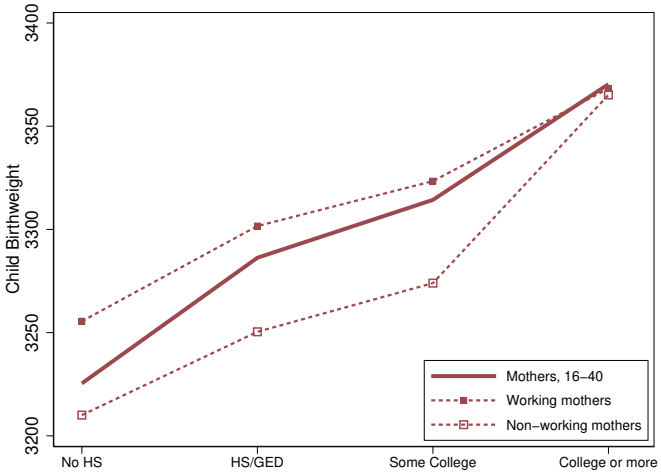
(a) Worked last year



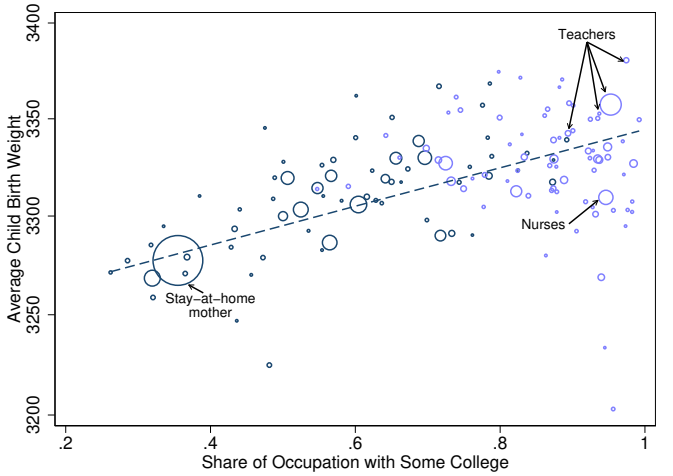
(b) Professional jobs



(c) Child birth weight by maternal employment



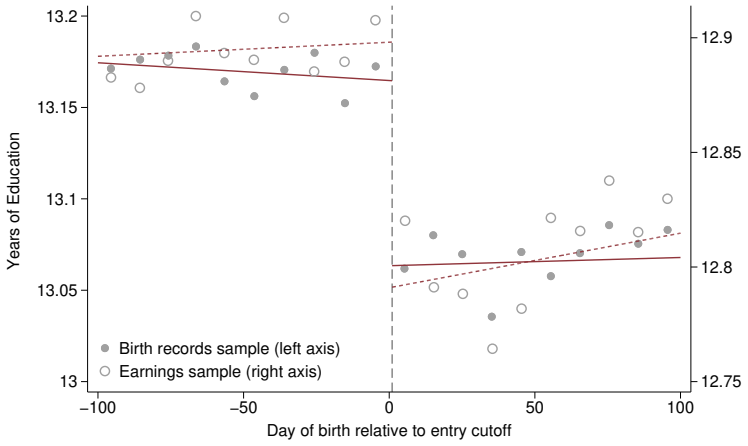
(d) Child birth weight by maternal occupation



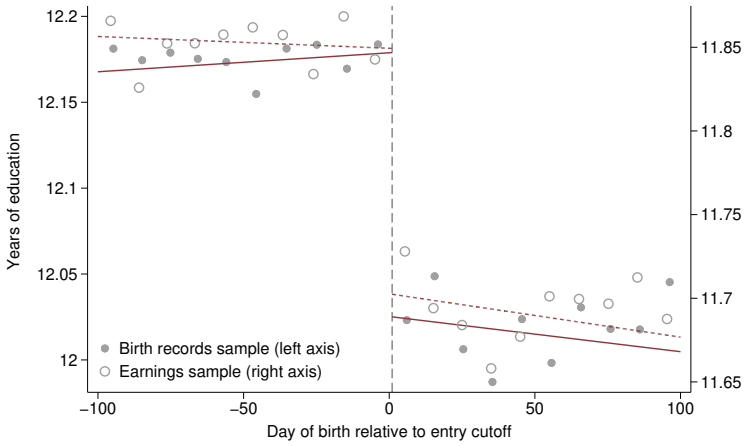
Notes: This figure presents the average labor market and child outcomes by women's (or mother's, for child outcomes) completed level of education: no high school diploma (No HS), high school diploma or GED only (HS/GED), some college attendance only, college degree or more. Panel A shows the share of women who worked in the last year; Panel B shows the share in professional occupations. Panel C shows average birth weight by maternal education (solid line), and by whether the mother worked in the previous year (dashed lines). Panel D shows average birth weight by maternal occupation, ordered by the share of mothers with some college. Professional occupations are highlighted in light blue markers; non-professional occupations are in black markers. The mothers sample includes all women with a first or second birth in California between 2007–2017; ACS estimates use 2007–2017 survey years.

Figure 2: Increase in Completed Education at School Entry Cutoff

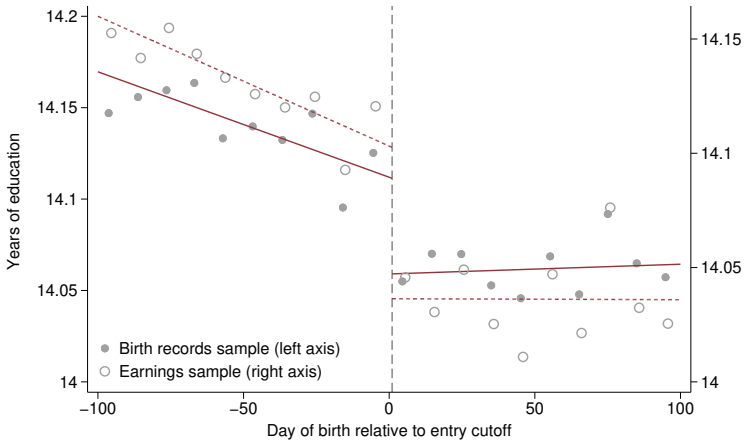
(a) All



(b) Younger than 25



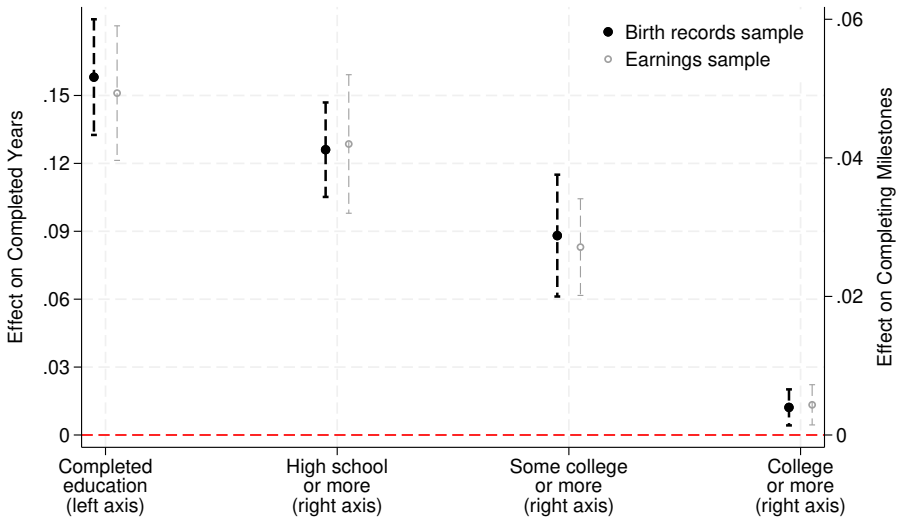
(c) Age 25 or older



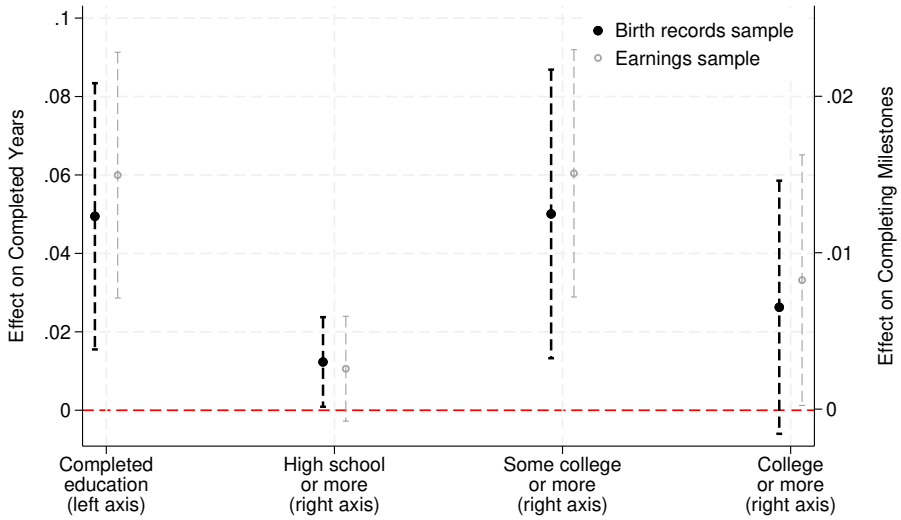
Notes: This figure presents the average years of education among women born within 100 days of the school entry cutoff. Panel A includes all women; Panel B includes early-career women; and Panel C includes mid-career women. Filled markers represent outcomes for the birth records sample, which are shown on the left axis; unfilled markers represent outcomes for the earnings sample, which are shown on the right axis. Outcomes are residualized of (recentered) year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Source: California birth records, 2007–2017 (birth records sample) or 1989-2017 (earnings sample).

Figure 3: Impacts Across Different Margins of Education Attainment

(a) Younger than 25

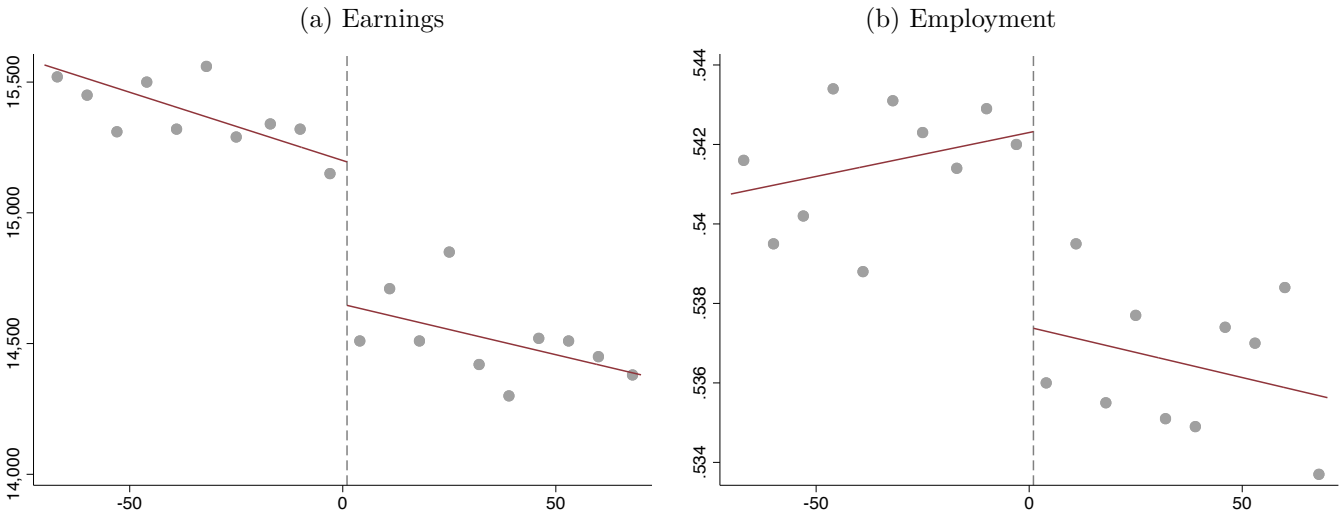


(b) Age 25 or older



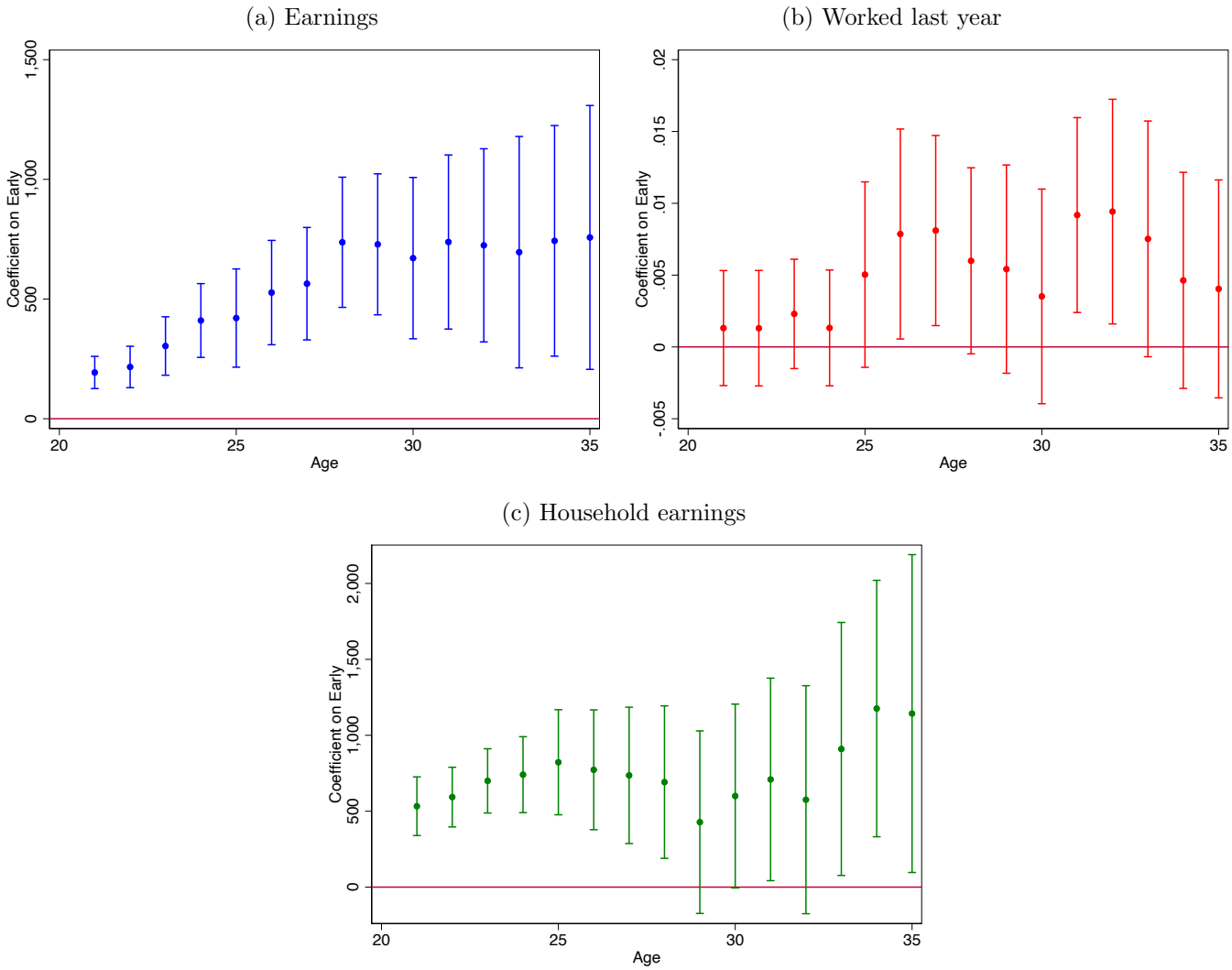
Notes: This figure presents the estimated effect (along with 95% confidence intervals) of being born just before the school entry cutoff on completed education, having a high school degree, completing some college or more, and completing college or more. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: California birth records, 2007–2017 (birth records sample) or 1989-2017 (earnings sample).

Figure 4: Discontinuity in Wages and Employment (Earnings Panel)



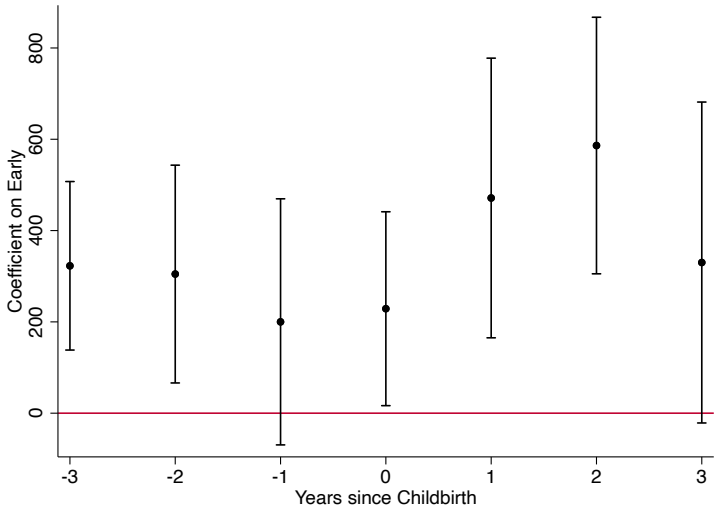
Notes: This figure presents average earnings (panel a) and employment rates (panel b) for women born around the school entry cutoff. Outcomes are residualized of (recentered) year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Sample includes women ages 16–40, born between 1977–1987, who satisfy other sample requirements. Source: Census administrative earnings records. See text for details.

Figure 5: Life-Cycle Improvements in Individual and Household Earnings (Earnings Panel)



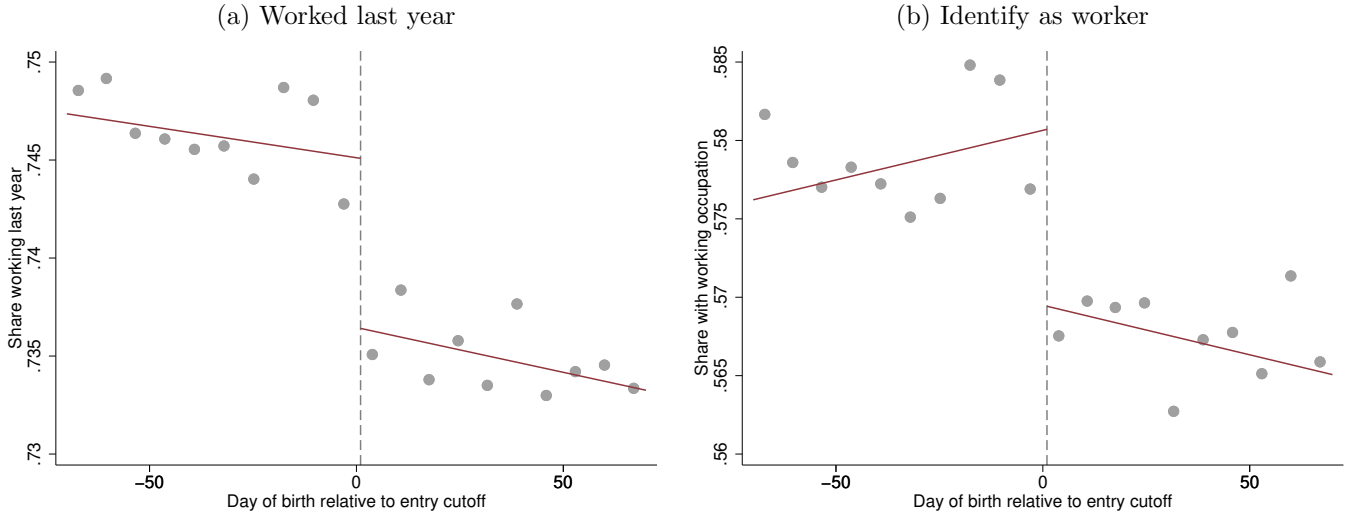
Notes: This figure presents the estimated effect (along with 95% confidence intervals) of being born just before the school entry cutoff on average earnings (Panel A) and the likelihood of working (Panel B) at each age between 25–35. Sample includes women born between 1977–1987, who satisfy other sample requirements. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: Census administrative earnings records. See text for details.

Figure 6: Discontinuity in Earnings around First Childbirth (Earnings Panel)



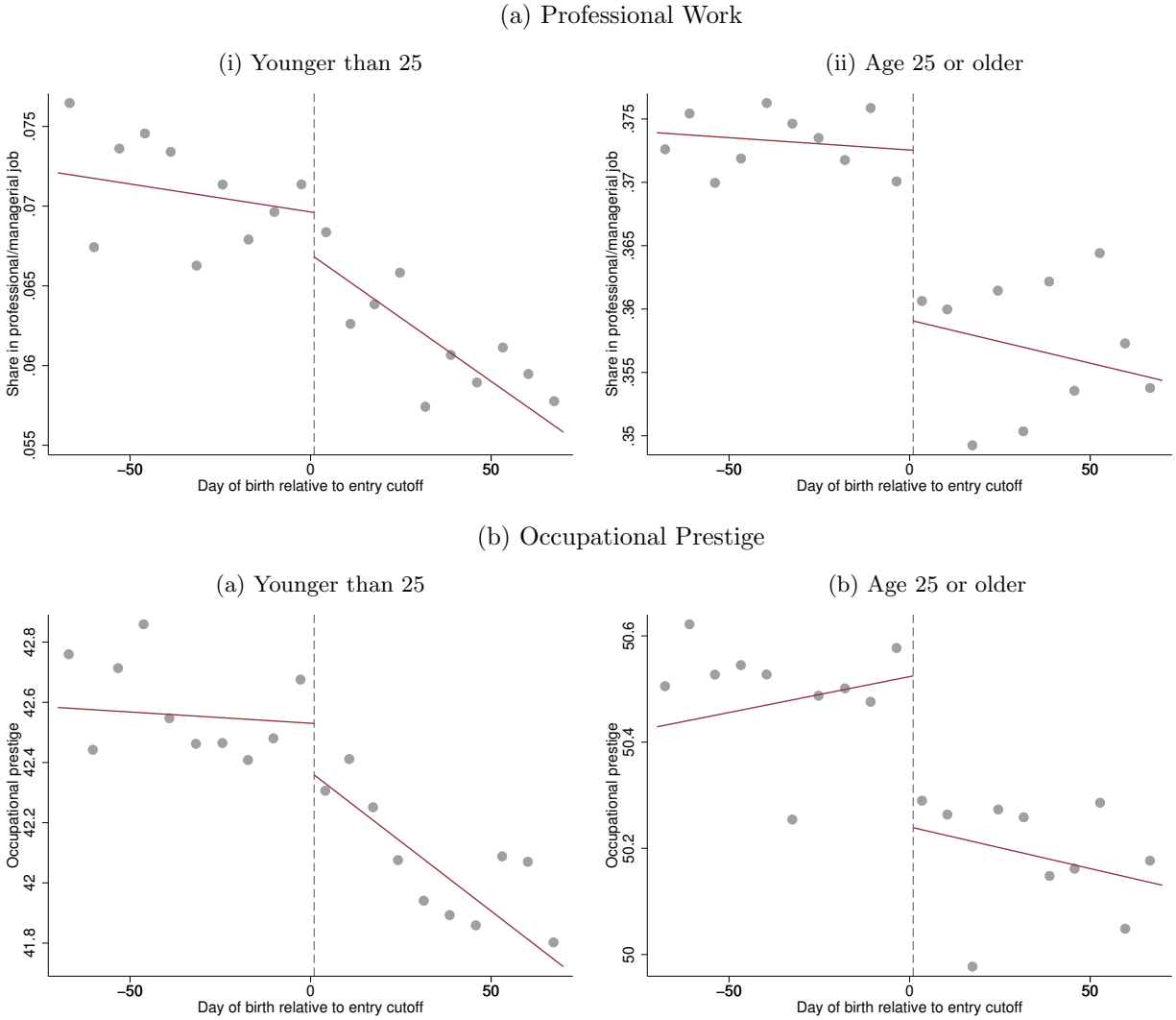
Notes: This figure presents the estimated effect (along with 95% confidence intervals) of being born just before the school entry cutoff on women's earnings in each year around the birth of a first child. Each estimate shown in the filled marker comes from a separate regression. Sample includes women ages 16–40, born between 1977–1987, who satisfy other sample requirements. See the notes of Figure 5 for details about the earnings panel and estimation.

Figure 7: Rise in Propensity to Work and Career Identity (Birth Records Sample)



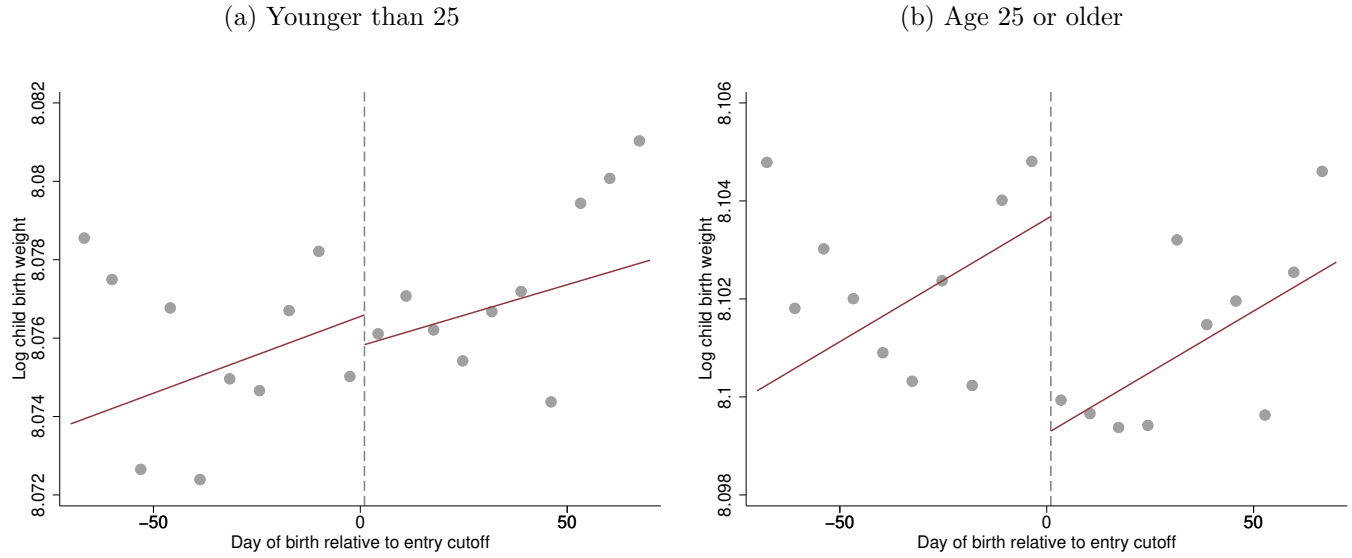
Notes: This figure presents the likelihood of working in the previous year (Panel A) and the likelihood of identifying as a worker (Panel B) for women born around the school entry cutoff. Outcomes are residualized of recentered year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Source: California birth records, 2007–2017.

Figure 8: Greater Professional Work and Occupational Prestige at Mid-Career (Birth Records Sample)



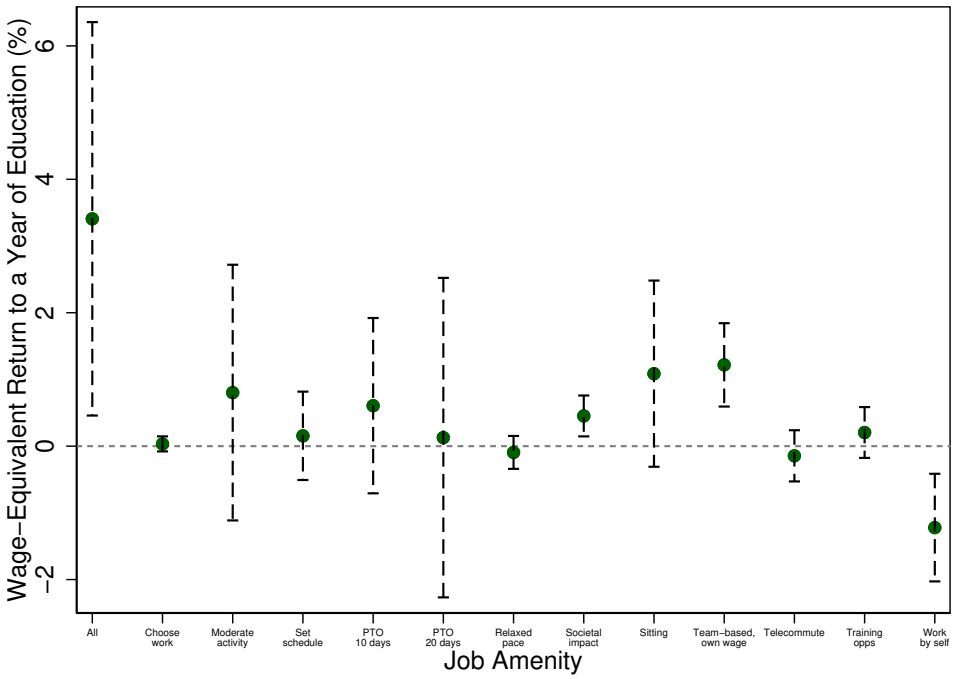
Notes: This figure presents the likelihood of having a professional occupation (Panel A) and average occupational prestige (Panel B) for women born around the school entry cutoff. Subpanel (i) shows outcomes in early-career and subpanel (ii) shows outcomes in mid-career. Outcomes are residualized of recentered year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Source: California birth records, 2007–2017.

Figure 9: Intergenerational Effects on Infant Health Grow over Time (Birth Records Sample)



Notes: This figure presents the average birth weight for mothers born around the school entry cutoff. The sample in Panel A includes mothers in early career; Panel B includes mothers in mid-career. Outcomes are residualized of recentered year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Source: California birth records, 2007–2017.

Figure 10: Wage-Equivalent Return to Education from Job Amenities (Birth Records Sample)



Notes: This figure presents the estimated effect (with 90% confidence intervals) of being born just before the school entry cutoff on mid-career job amenities, rescaled as the willingness-to-pay for the amenity value of one more year of education. The effect of the discontinuity on each job amenity is estimated using occupation-level averages calculated from the American Working Conditions Survey (Maestas et al., 2017), described in Appendix Section A.2. Each estimate (and standard error) is then multiplied by the willingness-to-pay for the amenity from Maestas et al. (2023), and divided by the effect of the cutoff on years of education (shown in Table A.6) to obtain the wage-equivalent return to education from each amenity (and standard error). The estimate shown in "all" is the sum across all twelve amenities. See the notes of Figure 3 for further details about the birth records sample and estimation.

Table 1: No Selection into Motherhood Around Cutoff

	Has a birth in the sample			Age at childbirth		
	Any	First	Second	All	First	Second
A. Earnings sample, all						
Born before cutoff	0.001	0.001	0.000	-0.025	-0.028	-0.022
	(0.001)	(0.001)	(0.001)	(0.032)	(0.034)	(0.035)
Mean above cutoff	0.157	0.097	0.060	24.059	22.702	26.158
Observations	3,197	3,197	3,197	569,472	345,516	223,956
B. Birth records sample, all						
Born before cutoff	0.002	0.001	0.001	-0.012	-0.025	0.002
	(0.002)	(0.001)	(0.001)	(0.016)	(0.022)	(0.024)
Mean above cutoff	0.191	0.118	0.073	24.798	23.208	26.929
Observations	3,197	3,197	3,197	501,574	287,086	214,488
C. Earnings sample, non-Black						
Born before cutoff	0.002	0.001	0.001	-0.025	-0.019	-0.034
	(0.002)	(0.001)	(0.001)	(0.034)	(0.036)	(0.037)
Mean above cutoff	0.153	0.094	0.059	24.200	22.844	26.263
Observations	3,197	3,197	3,197	512,390	309,089	203,301
D. Birth records sample, non-Black						
Born before cutoff	0.003	0.002*	0.001	-0.006	-0.016	0.004
	(0.002)	(0.001)	(0.001)	(0.016)	(0.023)	(0.024)
Mean above cutoff	0.185	0.114	0.071	24.884	23.295	26.994
Observations	3,197	3,197	3,197	456,588	260,524	196,064

Notes: This table presents the estimated effect of being born just before the school entry cutoff on having any children (column 1), a first child (column 2), or a second child (column 3); and on age at any childbirth (column 4), first childbirth (column 5) or second childbirth (column 6). The outcomes in columns 1–3 are fertility rates for cells defined by year-of-maternal-birth by birth-date-relative-to-the-cutoff, while the outcomes in columns 4–6 are individual-level maternal ages. Panel A includes all women in the earnings sample; Panel B includes all women in the birth records sample; Panel C includes non-Black women in the earnings sample; Panel D includes non-Black women in the birth records sample. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: Maternal ages and the numerators for fertility rates are from California birth records, 2007–2017 (birth records sample) or 1989-2017 (earnings sample). Denominator for fertility rates are from California birth records, 1960–2017.

Table 2: Discontinuity in Employment and Earnings Over a Career (Earnings Panel)

	All		Younger than 25		Age 25 or older	
	Working	Earnings	Working	Earnings	Working	Earnings
Born before cutoff	0.005**	555.7***	0.001	177.2***	0.007**	757.5***
	(0.002)	(130.4)	(0.001)	(26.0)	(0.003)	(194.0)
Mean above cutoff	0.537	14,560	0.232	2,686	0.697	20,800
Observations	5,979,000	5,979,000	2,061,000	2,061,000	3,917,000	3,917,000

Notes: This table presents the estimated effect of being born just before the school entry cutoff on women's employment and earnings. Columns 1–2 show impacts for all women (ages 16-40); columns 3–4 show early-career impacts (ages 16-24); and columns 5–6 show mid-career impacts (ages 25-40). See the notes of Figure 5 for details about the earnings panel and estimation.

Table 3: Discontinuity in Employment and Career Identity at Childbirth (Birth Records Sample)

	All		Younger than 25		Age 25 or older	
	Working last year	Identify as worker	Working last year	Identify as worker	Working last year	Identify as worker
Born before cutoff	0.011***	0.012***	0.014***	0.012***	0.008**	0.013***
	(0.003)	(0.003)	(0.004)	(0.004)	(0.003)	(0.004)
Mean above cutoff	0.737	0.570	0.582	0.391	0.881	0.742
Individuals	405,850	416,042	198,229	206,734	207,621	209,308

Notes: This table presents the estimated effect of being born just before the school entry cutoff on working in the previous year and identifying as a worker (having a "career identity"). Columns 1–2 show impacts for all women (ages 16-40); columns 3–4 show early-career impacts (ages 16-24); and columns 5–6 show mid-career impacts (ages 25-40). See the notes of Figure 3 for details about the birth records sample and estimation.

Table 4: Discontinuity in Job Selectivity, Social Impact, and Prestige Over a Career (Birth Records Sample)

	Job selectivity		Social impact		Social esteem		Co-worker quality
	Professional job	Job requires some college (no degree)	Pre-k or unspecified teacher	Pre-k teacher	Average prestige	Prestige ≥ 50th percentile	Share of peers with any college
A. Younger than 25							
Born before cutoff	0.003	0.004***	0.002*	-0.000	0.254	0.007	0.006*
	(0.002)	(0.001)	(0.001)	(0.000)	(0.169)	(0.008)	(0.003)
Mean above cutoff	0.063	0.017	0.006	0.001	42.100	0.229	0.620
Individuals	206,734	206,734	206,734	206,734	80,889	80,889	82,508
B. Age 25 or older							
Born before cutoff	0.013***	0.008**	0.008**	0.002**	0.267**	0.011***	0.005***
	(0.004)	(0.003)	(0.003)	(0.001)	(0.105)	(0.004)	(0.002)
Includes non-workers	Yes	Yes	Yes	Yes	No	No	No
Mean above cutoff	0.357	0.117	0.063	0.004	50.182	0.537	0.764
Individuals	209,308	209,308	209,308	209,308	150,704	150,704	156,580

Notes: This table presents the estimated effect of being born prior to the school entry cutoff on measures of one's position on the job selectivity (columns 1–2); social impact (columns 3–4); social esteem (columns 5–6) and co-worker quality (column 7) at mid-career. Column 1 presents effects on holding a professional occupation; Column 2 on having a job that requires some college and no college degree; Column 3 on being a pre-school or unspecified teacher; Column 4 on being a pre-school teacher; Column 5 on average occupational prestige, Column 6 on having above-median occupational prestige; and Column 7 on the share of individuals in the occupation who have at least some college. See the notes of Figure 3 for details about the sample and estimation, and Section 2 for information on the outcome construction.

Table 5: Discontinuity in Infant Outcomes and the Pregnancy Environment (Birth Records Sample)

	Child outcomes:			Maternal behaviors:		Public programs:	
	Log birthweight	Birthweight	Gestation	Any smoking	Prenatal visits	Any WIC	Medi-Cal insurance
A. Younger than 25							
Born before cutoff	-0.000	-0.026	0.296	-0.001	0.036	0.005	-0.004
	(0.002)	(4.457)	(0.511)	(0.002)	(0.038)	(0.004)	(0.004)
Mean above cutoff	8.077	3274.868	272.611	0.050	11.456	0.783	0.699
Individuals	223,393	223,393	223,380	220,644	217,721	221,151	222,919
B. Age 25 or older							
Born before cutoff	0.004**	9.126**	0.446*	-0.005***	0.007	-0.007**	-0.011***
	(0.002)	(4.433)	(0.252)	(0.002)	(0.040)	(0.004)	(0.003)
Mean above cutoff	8.100	3355.748	273.504	0.040	12.325	0.355	0.299
Individuals	230,671	230,671	230,640	229,066	227,219	228,606	230,475

Notes: This table presents the estimated effect of being born prior to the school entry cutoff on the birth outcomes of one's children and the pregnancy environment. Panel A includes women in early-career (ages 16-24); Panel B includes women in mid-career (ages 25-40). Columns 1–3 present effects on log birth weight, child birth weight (grams), and gestational age (days), respectively; column 4 on smoking during pregnancy or the preceding quarter; column 5 on the number of prenatal doctor visits; and columns 6–7 on receiving any WIC during pregnancy and using Medi-Cal as the primary payment for the birth delivery, respectively. See the notes of Figure 3 for details about the sample and estimation.

ONLINE APPENDIX:

Multigenerational Benefits of Women's Education

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September 4, 2025

A Data appendix

A.1 Assignment of Occupation Codes in the Birth Records

This section describes the process of converting parents' occupation strings to 2010 Census occupation codes. Appendix Table A.1 summarizes the output for mothers' occupations; match rates are similar for fathers.

We assign occupation codes in three steps. First, we parse strings with multiple jobs into component occupations (0.5% of women). This produces 19,310 unique maternal occupations, 13,967 of which contain meaningful occupation information.

Second, we worked with a team of research assistants to map each occupation string to an occupation code or classify it as "not in the labor force" (NILF). This becomes the "cleaned occupation" for each parent. We rely on the "Alphabetical Indexes of Occupations", which provides a crosswalk from specific occupations to occupation codes to ensure consistency with Census classifications (U.S. Census Bureau, 2019). For strings encompassing multiple occupations (e.g. "Teacher") we assign multiple occupation codes.[16] This yields cleaned occupations for 88% of women.

Third, we use information on parental industry to disambiguate additional strings. For example, the occupation "Civil" combined with industry "Engineering" becomes "Civil Engineer" (OCC 1360) in this step. This yields cleaned occupation codes for an additional 7% of women.

Overall, this process assigns a cleaned occupation to 95% of women who report any occupation. Of these women, 62% are in the labor force. For the 38% who are not in the labor force, the majority report being stay-at-home mothers.

Table A.1: Assignment of Census Occupation Codes

Group	N	% of Total	Unique
All	2,006,186	-	19,310
Reported any occupation	1,907,558	95%	13,967
Cleaned occupation	1,808,392	95%[a]	357
Used reported industry	134,672	7%[a]	202
Not working	687,083	36%[a]	12
Working	1,121,309	59%[a]	345

Notes: (a) As a share of the number reporting any occupation. Cleaned occupations include women who are assigned an occupation code or NILF. The sample consists of all mothers with a birth in California between 2007 and 2017.

A.2 Data Sources for Job Characteristics

The analysis draws on several supplemental data sources to obtain information on job characteristics. This section describes the data preparation to create and assign these measures.

[16] For example, for "Teacher" the group of occupations is "Preschool and Kindergarten Teacher" (OCC 2300), "Elementary and Middle School Teachers" (OCC 2310), "Secondary School Teachers" (OCC 2320), "Special Education Teachers" (OCC 2330), and "Other Teachers and Instructors" (OCC 2340).

Occupational prestige: We measure occupational prestige by taking the simple average of ratings from two large-scale surveys. The first rating comes from a 2016 survey of 3,113 workers on Amazon's Mechanical Turk in 2016 (Hughes et al., 2022). Respondents rated each occupation on a scale of 1 to 100, with "100" implying that the occupation is more prestigious than any other. To ensure awareness of job responsibilities, respondents were given job descriptions detailing tasks and required licensing for each occupation. The second rating comes from responses to the occupational prestige module of the General Social Survey (GSS) administered to approximately 1,300 individuals in 2012 (Smith and Son, 2014). Respondents ranked jobs (covering all 2010 Census occupations) on a scale of 1 to 9 along an "occupational prestige ladder," with "9" representing the highest possible social standing. We convert the GSS rating to a 100 point scale to make ratings comparable across surveys.

Schooling requirements: We obtain schooling requirements for each position in three steps. First, we use the the 2015-2018 basic monthly CPS data to identify occupations where the majority of workers have an active professional certification or state or industry license in California (Kleiner and Soltas, 2019). We refer to these occupations as requiring a license or credential. Second, we identify candidate occupations for having schooling requirements based on (i) high shares of workers with particular education levels and (ii) requiring a license or credential. We designate an occupation as a candidate for requiring some college (high school) if at least 20% of workers have exactly some college (high school). We designate an occupation as a candidate for requiring college if at least 50% of workers have a college degree. Third, we use public websites on licensing in California to validate the licensing and education requirements among candidate occupations from step 2. By default, we keep the CPS-based designation unless we find contradictory information online.

Hourly wages: We calculate hourly wages for each occupation using the 2007 to 2017 CPS Merged Outgoing Rotation Groups (MORG). For hourly workers, this is the reported hourly wage. For non-hourly workers, this is weekly earnings divided by usual hours per week. Wages are updated to 2016 dollars using the CPI from the Bureau of Labor Statistics. The sample consists of women between ages 25 and 49 in California (Flood et al., 2020).

Annual earnings and weeks of work: We measure annual earnings and weeks of work for each occupation using the 2007 to 2017 Annual Social and Economic Supplement (ASEC) of the CPS from IPUMS (Flood et al., 2020). Earnings are updated to 2016 dollars using the CPI from the Bureau of Labor Statistics (U.S. Bureau of Labor Statistics, 2017). The sample consists of women between the ages of 25 and 49 in California.

Other job characteristics in the monthly CPS: We calculate the occupation share with any college and average weekly hours of work using the 2007 to 2017 basic monthly CPS from IPUMS (Flood et al., 2020). The sample consists of women between the ages of 25 and 49 in California.

Working conditions used in Maestas et al. (2023): Maestas et al. (2023) experimentally elicit willingness-to-pay for twelve job amenities (see Table 2 for estimates). These include whether a job allows a worker to set her own schedule; telecommute; perform moderate physical activity (rather than perform heavy physical activity); sit (rather than perform heavy activity); choose how to do work; have 10 days paid time off (instead of none); have 20 days paid time off (instead of

none); be evaluated on your own work within a team; work alone; have training opportunities; and have frequent opportunities for social impact (instead of occasional opportunities). The working conditions manipulated in the experiment were chosen based on patterns observed in the American Working Conditions Survey (AWCS), a 2015 survey of workers fielded by the same group of researchers (Maestas et al., 2017). Thus, we use the AWCS to calculate analogous measures of these amenities for each Census occupation. Note that only 1,914 respondents in the AWCS report an occupation, so occupation-level characteristics (for the over 216 reported occupations) are likely measured with some noise.

Merge to birth records: Job characteristics from each data source are collapsed by the Census 2010 occupation code, and merged to parents in the birth records. Parents that have multiple occupation codes are assigned the simple average of job characteristics across jobs, if it is continuous, or the maximum across jobs, if it is binary.

Table A.2: No Selection into Motherhood Around Cutoff By Race (Birth Records Sample)

	Has a birth in the sample			Age at childbirth		
	Any	First	Second	All	First	Second
A. White, non-Hispanic						
Born before cutoff	0.000	0.001	-0.001	-0.003	0.009	-0.018
	(0.002)	(0.001)	(0.001)	(0.026)	(0.030)	(0.037)
Mean above cutoff	0.125	0.078	0.047	26.589	25.020	28.437
Observations	3,197	3,197	3,197	162,850	88,310	74,540
B. Hispanic						
Born before cutoff	0.003	0.002	0.002	-0.020	-0.035	0.002
	(0.003)	(0.002)	(0.001)	(0.021)	(0.024)	(0.035)
Mean above cutoff	0.273	0.164	0.110	23.728	22.194	25.916
Observations	3,197	3,197	3,197	267,489	157,247	110,242
C. Black						
Born before cutoff	-0.004	-0.005	0.001	-0.074	-0.118*	-0.023
	(0.006)	(0.004)	(0.003)	(0.047)	(0.066)	(0.059)
Mean above cutoff	0.213	0.136	0.077	23.946	22.380	26.243
Observations	3,197	3,197	3,197	44,986	26,562	18,424
D. Other						
Born before cutoff	-0.001	-0.002	0.001	0.078	0.019	0.151
	(0.004)	(0.003)	(0.002)	(0.071)	(0.075)	(0.121)
Mean above cutoff	0.125	0.078	0.047	26.059	24.664	27.955
Observations	3,197	3,197	3,197	26,249	14,967	11,282
joint p-value of zero effect	0.741	0.404	0.483	0.270	0.083	0.740
joint p-value, excl. Black mothers	0.670	0.557	0.339	0.454	0.357	0.580

Notes: This table presents the estimated effect of being born just before the school entry cutoff on the likelihood of having any children (column 1), a first child (column 2), or a second child (column 3); and on age at any childbirth (column 4), first childbirth (column 5) or second childbirth (column 6). The outcomes in columns 1–3 are fertility rates for cells defined by year-of-maternal-birth by birth-date-relative-to-the-cutoff, while the outcomes in columns 4–6 are individual-level maternal ages. Estimation is performed separately for women who are White, Hispanic, Black and Other (Panels A through D, respectively). Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: Maternal ages and the numerators for fertility rates are from California birth records, 2007–2017. Denominator for fertility rates are from California birth records, 1960–2017.

Table A.3: No Selection into Motherhood Around Cutoff By Race (Earnings Sample)

	Has a birth in the sample			Age at childbirth		
	Any	First	Second	All	First	Second
A. White, non-Hispanic						
Born before cutoff	0.003	0.002	0.001	0.004	0.027	-0.030
	(0.002)	(0.001)	(0.001)	(0.049)	(0.046)	(0.064)
Mean above cutoff	0.110	0.068	0.042	25.222	23.907	27.248
Observations	3,197	3,197	3,197	209,800	127,248	82,552
B. Hispanic						
Born before cutoff	0.002	0.001	0.001	-0.042	-0.051	-0.030
	(0.003)	(0.001)	(0.002)	(0.041)	(0.044)	(0.052)
Mean above cutoff	0.238	0.145	0.093	23.308	21.903	25.415
Observations	3,197	3,197	3,197	274,115	164,377	109,738
C. Black						
Born before cutoff	-0.007	-0.004	-0.003	-0.021	-0.096	0.105
	(0.005)	(0.003)	(0.003)	(0.084)	(0.087)	(0.112)
Mean above cutoff	0.178	0.116	0.062	22.817	21.513	25.139
Observations	3,197	3,197	3,197	57,082	36,427	20,655
D. Other						
Born before cutoff	-0.000	-0.001	0.001	-0.049	-0.029	-0.074
	(0.004)	(0.002)	(0.002)	(0.148)	(0.158)	(0.171)
Mean above cutoff	0.108	0.068	0.040	25.323	24.031	27.417
Observations	3,197	3,197	3,197	28,475	17,464	11,011
joint p-value of zero effect	0.515	0.402	0.659	0.845	0.488	0.779
joint p-value, excl. Black mothers	0.565	0.420	0.670	0.741	0.541	0.798

Notes: This table presents the estimated effect of being born just before the school entry cutoff on the likelihood of having any children (column 1), a first child (column 2), or a second child (column 3); and on age at any childbirth (column 4), first childbirth (column 5) or second childbirth (column 6). The outcomes in columns 1–3 are fertility rates for cells defined by year-of-maternal-birth by birth-date-relative-to-the-cutoff, while the outcomes in columns 4–6 are individual-level maternal ages. Estimation is performed separately for women who are White, Hispanic, Black and Other (Panels A through D, respectively). Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: Maternal ages and the numerators for fertility rates are from California birth records, 1989–2017. Denominator for fertility rates are from California birth records, 1960–2017.

Table A.4: No Selection into Motherhood within Early- and Mid-career Women (Birth Records Sample)

	Maternal traits					Grandparental traits	
	Any birth	Age at birth	Share White	Share Hispanic	Share Asian	Mother's age at childbirth	Father's age at childbirth
A. Younger than 25							
Born before cutoff	0.001	-0.023*	-0.005	0.006	0.001	0.062	-0.022
	(0.001)	(0.014)	(0.004)	(0.004)	(0.001)	(0.052)	(0.071)
Mean above cutoff	0.090	21.205	0.243	0.712	0.016	24.963	27.656
Observations	3,197	238,679	239,339	239,101	238,917	221,495	204,254
B. Age 25 or older							
Born before cutoff	0.001	0.016	-0.005	0.006	0.001	-0.071	-0.050
	(0.001)	(0.014)	(0.004)	(0.004)	(0.002)	(0.059)	(0.059)
Mean above cutoff	0.074	28.960	0.484	0.445	0.037	25.987	28.683
Observations	2,581	215,392	215,796	215,622	215,566	202,046	197,576

Notes: This table presents the estimated effect of being born just before the school entry cutoff on the likelihood of having any children, maternal traits, and grandparental traits. Panel A includes women in early-career (ages 16-24); Panel B includes women in mid-career (25-40). The outcome in column 1 is a fertility rate for cells defined by year-of-maternal-birth by date-relative-to-the-cutoff. The remaining columns show individual-level outcomes: maternal age (column 2), White, non-Hispanic indicator (column 3), Hispanic indicator (column 4), Asian indicator (column 5), maternal grandmother's age at the time of the mother's birth (column 6) and maternal grandfather's age at the time of the mother's birth (column 7). See the notes of Figure 3 for details about the birth records sample and estimation.

Table A.5: No Selection into Motherhood within Early- and Mid-career Women (Earnings Sample)

	Maternal traits					Grandparental traits	
	Any birth	Age at birth	Share White	Share Hispanic	Share Asian	Mother's age at childbirth	Father's age at childbirth
A. Younger than 25							
Born before cutoff	0.001	-0.045**	-0.002	0.002	0.002	-0.058	-0.150
	(0.001)	(0.021)	(0.007)	(0.007)	(0.001)	(0.086)	(0.103)
Mean above cutoff	0.063	20.872	0.341	0.615	0.014	24.927	27.676
Observations	3,197	284,406	285,130	284,477	285,059	108,560	105,037
B. Age 25 or older							
Born before cutoff	0.001	0.004	-0.003	0.004	0.000	-0.065	-0.056
	(0.002)	(0.016)	(0.005)	(0.005)	(0.002)	(0.068)	(0.071)
Mean above cutoff	0.106	28.661	0.498	0.433	0.036	25.997	28.689
Observations	1,529	222,550	222,929	222,734	222,745	190,739	187,266

Notes: This table presents the estimated effect of being born just before the school entry cutoff on the likelihood of being observed having any children, maternal traits, and grandparental traits. Panel A includes women in early-career (ages 16-24); Panel B includes women in mid-career (25-40). The outcome in columns 1 is a fertility rate for cells defined by year-of-maternal-birth by date-relative-to-the-cutoff. The remaining columns show individual-level outcomes: maternal age (column 2), White, non-Hispanic indicator (column 3), Hispanic indicator (column 4), Asian indicator (column 5), maternal grandmother's age at the time of the mother's birth (column 6) and maternal grandfather's age at the time of the mother's birth (column 7). See the notes of Figure 5 for details about the earnings panel and estimation.

Table A.6: Discontinuity in Educational Attainment and School Attendance (Birth Records Sample)

	Years completed	High school graduate	Any college	College graduate	In school
A. All					
Born before cutoff	0.104***	0.022***	0.021***	0.005**	-0.011***
	(0.013)	(0.002)	(0.004)	(0.002)	(0.002)
Mean above cutoff	13.072	0.843	0.511	0.175	0.095
Individuals	446,619	446,619	446,619	446,619	436,680
B. Younger than 25					
Born before cutoff	0.158***	0.041***	0.029***	0.004***	-0.020***
	(0.013)	(0.003)	(0.004)	(0.001)	(0.003)
Mean above cutoff	12.020	0.721	0.295	0.015	0.172
Individuals	219,362	219,362	219,362	219,362	212,929
C. Age 25 or older					
Born before cutoff	0.049***	0.003**	0.012***	0.007	-0.002
	(0.017)	(0.001)	(0.005)	(0.004)	(0.002)
Mean above cutoff	14.059	0.959	0.714	0.324	0.024
Individuals	227,257	227,257	227,257	227,257	223,751

Notes: This table presents the estimated effect of being born just before the school entry cutoff on education outcomes. Panel A includes all women (ages 16-40); Panel B includes women in early-career (ages 16-24); and Panel C includes women in mid-career (ages 25-40). Each column shows results for a different outcome: imputed years of education (column 1), the high school completion (column 2); some college completion (column 3); college graduation (column 4); and school attendance (column 5). See the notes of Figure 3 for details about the birth records sample and estimation.

Table A.7: Descriptive Statistics for Birth Records

	Mothers, 1^{st} or 2^{nd} child		Estimation samples		
	(1)	(2)	(3)	(4)	(5)
	All	CA natives	All	70 day window	1977-87 cohorts
Black	0.055	0.081	0.090	0.089	0.079
	(0.228)	(0.273)	(0.286)	(0.285)	(0.269)
White, non-Hispanic	0.316	0.376	0.333	0.326	0.428
	(0.465)	(0.484)	(0.471)	(0.469)	(0.495)
Hispanic	0.454	0.480	0.526	0.532	0.429
	(0.498)	(0.500)	(0.499)	(0.499)	(0.495)
Asian	0.132	0.034	0.024	0.024	0.033
	(0.338)	(0.181)	(0.152)	(0.153)	(0.178)
High school graduate	0.845	0.874	0.854	0.850	0.942
	(0.362)	(0.332)	(0.353)	(0.357)	(0.233)
Some college or more	0.594	0.581	0.515	0.513	0.668
	(0.491)	(0.493)	(0.500)	(0.500)	(0.471)
College graduate	0.334	0.259	0.172	0.169	0.283
	(0.472)	(0.438)	(0.378)	(0.375)	(0.450)
Age	27.765	26.234	24.285	24.286	27.047
	(6.239)	(6.034)	(4.603)	(4.621)	(3.325)
Year of birth	1,983.603	1,985.292	1,987.139	1,986.943	1,983.528
	(6.681)	(6.468)	(5.231)	(5.090)	(2.850)
Worked last year	0.776	0.805	0.775	0.769	0.867
	(0.417)	(0.396)	(0.418)	(0.422)	(0.339)
Infant birth weight (g)	3,286.217	3,297.666	3,299.212	3,300.700	3,338.343
	(556.8)	(567.0)	(557.2)	(556.1)	(561.5)
Observations	3,931,688	2,006,194	1,562,347	516,266	262,492

Notes: This table presents summary statistics for first-time and second-time mothers in California, moving from all mothers to the analysis sample. Column 1 includes all mothers; Column 2 limits to women born in California; Column 3 further limits to women with a first birth by age 31 and between the ages of 16-40; Column 4 focuses on women born within seventy days of the school entry cutoff; and Column 5 limits to women born between 1977-1987 (matching the earnings panel). Each observation is a childbirth event. Source: California birth records, 2007–2017 (birth records sample) or 1989-2017 (earnings sample).

Table A.8: Descriptive Statistics Across Estimation Samples

	All			Drop Black mothers		
	Birth Records	Birth Records Replication	Earnings Panel	Birth Records	Birth Records Replication	Earnings Panel
Black	0.09	0.09	0.10	0.00	0.00	0.00
White, non-Hispanic	0.32	0.32	0.37	0.35	0.35	0.41
Hispanic	0.54	0.55	0.48	0.59	0.60	0.53
Year of birth	1987	1987	1982	1987	1987	1982
Age at first childbirth	22.93	23.03	23.02	23.02	23.08	23.11
Observations	388,070	415,000	304,000	347,541	379,000	275,000

Notes: This table presents summary statistics for our three main estimation samples: mothers in the 2007-2017 birth records (columns 1 and 4); women in the linked IRS-Census records who meet the same sample criteria as the mothers in the 2007-2017 birth records (columns 2 and 5); and women in the earnings panel (columns 3 and 6). Each observation is a unique woman. See the notes of Figures 3 and 5 for details about the birth records and earnings panel samples.

Table A.9: Discontinuity in Educational Attainment and School Attendance (Earnings Sample)

	Years completed	High school graduate	Any college	College graduate
A. All				
Born before cutoff	0.109***	0.023***	0.022***	0.006***
	(0.011)	(0.003)	(0.003)	(0.002)
Mean above cutoff	12.833	0.794	0.455	0.166
Individuals	500,377	500,377	500,377	500,377
B. Younger than 25				
Born before cutoff	0.151***	0.042***	0.027***	0.004***
	(0.015)	(0.005)	(0.004)	(0.001)
Mean above cutoff	11.749	0.652	0.230	0.019
Individuals	264,927	264,927	264,927	264,927
C. Age 25 or older				
Born before cutoff	0.060***	0.003	0.015***	0.008**
	(0.016)	(0.002)	(0.004)	(0.004)
Mean above cutoff	14.035	0.952	0.705	0.329
Individuals	235,450	235,450	235,450	235,450

Notes: This table presents the estimated effect of being born just before the school entry cutoff on education outcomes. Panel A includes all women (ages 16-40); Panel B includes women in early-career (ages 16-24); and Panel C includes women in mid-career (ages 25-40). Each column shows results for a different outcome: imputed years of education (column 1), the high school completion (column 2); some college completion (column 3); and college graduation (column 4). We do not show effects for school attendance because this outcome is only available after 2007, once most of the sample is in mid-career. See the notes of Figure 5 for details about the earnings panel and estimation.

Table A.10: Discontinuity in Other Measures of Income Over a Career (Earnings Panel)

	All		Younger than 25		Age 25 or older	
	Earnings if working	Household income	Earnings if working	Household income	Earnings if working	Household income
Born before cutoff	771.0***	739.9***	713.6***	518.7***	789.0***	861.7***
	(168.3)	(224.6)	(95.0)	(59.6)	(191.9)	(331.3)
Mean above cutoff	27,130	31,340	11,580	7,751	29,850	43,740
Observations	3,223,000	5,979,000	481,000	2,061,000	2,742,000	3,917,000

Notes: This table presents the estimated effect of being born just before the school entry cutoff on women's earnings conditional on working and household earnings. Columns 1–2 present average impacts for all women (ages 16-40); columns 3–4 present impacts in early career (ages 16-24); and columns 5–6 present impacts in mid-career (ages 25-40). See the notes of Figure 5 for details about the earnings panel and estimation.

Table A.11: Discontinuity in Employment and Earnings in Year of Childbirth

	Working last year		Earnings	
	Earnings sample	Birth records sample	Earnings sample	Birth records sample
A. All				
Born before cutoff	0.005*	0.013***	340.2***	529.0***
	(0.003)	(0.002)	(120.4)	(104.9)
Mean above cutoff	0.480	0.669	11,410	13,150
Observations	419,000	599,000	419,000	599,000
B. Younger than 25				
Born before cutoff	-0.001	0.016***	145.9***	458.3***
	(0.003)	(0.003)	(47.5)	(68.0)
Mean above cutoff	0.256	0.590	2,994	6,080
Observations	224,000	331,000	224,000	331,000
C. Age 25 or older				
Born before cutoff	0.012***	0.009**	572.3**	628.4***
	(0.004)	(0.004)	(242.2)	(227.7)
Mean above cutoff	0.737	0.767	21,020	21,790
Observations	196,000	268,000	196,000	268,000

Notes: This table presents the estimated effect of being born just before the school entry cutoff on women's labor market outcomes in the year of childbirth. The odd columns show effects for the earnings sample, while the even columns show effects for the birth records sample, recreated in the Census administrative records. The outcomes are working in the last year (columns 1–2) and earnings (columns 3-4). See the notes of Figure 5 for details about the data and estimation.

Table A.12: Discontinuity in Occupation-level Earnings and Work Schedule (Birth Records Sample)

	Work schedule			Wage and salary earnings		
	Usual weekly hours	Inflexibility	Weeks last year	Real annual wage (incl 0's)	Log real hourly wage	Log real annual wage
A. Younger than 25						
Born before cutoff	-0.094*	0.001	0.071	392.610**	0.010*	0.010
	(0.049)	(0.002)	(0.073)	(161.723)	(0.005)	(0.007)
Mean above cutoff	34.640	0.732	44.179	11383.877	2.740	10.068
Individuals	80,835	82,197	81,303	205,815	80,703	81,216
B. Age 25 or older						
Born before cutoff	-0.031	0.002**	0.022	533.318**	0.007**	0.003
	(0.057)	(0.001)	(0.043)	(258.882)	(0.003)	(0.005)
Includes non-workers	No	No	No	Yes	No	No
Mean above cutoff	35.089	0.772	45.854	33171.102	3.059	10.438
Individuals	154,001	156,175	154,389	207,329	153,786	154,180

Notes: This table presents the estimated effect of being born just before the school entry cutoff on women's expected annual earnings and work schedule. Panel A includes women in early-career (ages 16-24); Panel B includes women in mid-career (age 25-40). Each column shows results for a different outcome: usual weekly hours (column 1); work inflexibility (column 2); weeks worked last year (column 3); real annual earnings, inclusive of 0's (column 4); log real hourly wage (column 5); and log real annual earnings (column 6). The outcomes in columns 1 and 3–6 are occupation-specific averages for workers in California calculated from the 2007–2017 Current Population Survey. Work inflexibility from Cubas et al. (2019), and is defined as 1 minus the typical share of hours worked during between 8 am and 5 pm for each occupation. See the notes of Figure 3 for details about the birth records sample and estimation, and Appendix A for descriptions and sources for the outcomes.

Table A.13: Robustness to Estimation Parameters, Inference, and Sample (Birth Records Sample)

	Baseline	Optimal bandwidth	Eicker-Huber-White SE's	Bootstrap education	Bias-corrected estimates	Include Black women
Years of education	0.049***	0.052***	0.049***	0.049***	0.056***	0.047***
	(0.017)	(0.018)	(0.016)	(0.017)	(0.022)	(0.015)
Share with any college	0.012***	0.012***	0.012***	0.012***	0.013***	0.012***
	(0.005)	(0.004)	(0.004)	(0.005)	(0.005)	(0.004)
Share with working occupation	0.013***	0.013***	0.013***	0.013***	0.014**	0.011***
	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)
Share working last year	0.008**	0.007**	0.008**	0.008**	0.007*	0.006**
	(0.003)	(0.003)	(0.003)	(0.003)	(0.004)	(0.003)
Professional	0.013***	0.012**	0.013***	0.013***	0.011*	0.013***
	(0.004)	(0.005)	(0.004)	(0.004)	(0.006)	(0.004)
Occupational prestige	0.267**	0.313**	0.267**	0.267**	0.338*	0.286**
	(0.105)	(0.144)	(0.126)	(0.105)	(0.173)	(0.121)
Log birth weight	0.004**	0.005**	0.004**	0.004**	0.006**	0.003*
	(0.002)	(0.002)	(0.002)	(0.002)	(0.003)	(0.002)
Child birth weight	9.126**	13.983**	9.126*	9.126**	16.460**	7.328
	(4.433)	(5.918)	(4.853)	(4.433)	(6.927)	(4.736)

Notes: This table presents the estimated effect of being born just before the school entry cutoff using alternative estimation methods and samples. Each column presents results for a different modification to the baseline specification: using the mean-squared-error optimal bandwidth for each outcome, implemented with STATA command rdrobust (column 2); using Eicker-Huber-White standard errors (column 3); bootstrapping the two-step estimation of years of education (column 4); bias-correcting the estimates, implemented with STATA command rdrobust (column 5), and including Black women (column 6). Each row presents results for a different outcome. See the notes of Figure 3 for details about the baseline birth records sample and estimation.

Table A.14: Bounds on Mid-Career Estimates, Taking into Account Potential Selection into Work (Birth Records Sample)

	Baseline	Lower bound	Upper bound
Occupational prestige	0.267**	0.255**	0.290***
	(0.105)	(0.105)	(0.105)
Peers with any college	0.005***	0.005***	0.005***
	(0.002)	(0.002)	(0.002)

Notes: This table presents Lee (2009) bounds on the effect of being born just before the school entry cutoff on working outcomes, accounting for selection. Column 1 presents baseline estimates. Column 2 presents the lower bound, dropping the top 1.9% of outcomes of non-college-educated women born before the cutoff. Column 3 presents the upper bound, dropping the bottom 1.9% of outcomes of non-college-educated women born before the cutoff. Each row presents a different outcome conditional on having an occupation. See the notes of Figure 3 for details about the birth records sample and estimation.

Table A.15: Results using Balanced Panel in Birth Records

	Mother outcomes (2007–2017)				Child outcomes (1989–2017)		
	Years completed	Working last year	Professional	Prestige	Ln birth weight	Birth weight	Gestation
A. First child ($\overline{age}$ = 23)							
Born before cutoff	0.110***	0.004	-0.002	0.159	-0.001	1.535	0.444
	(0.022)	(0.005)	(0.007)	(0.206)	(0.002)	(5.672)	(0.439)
Individuals	81,627	75,509	75,995	43,813	152,133	152,133	152,092
B. Second child ($\overline{age}$ = 27)							
Born before cutoff	0.070**	0.016***	0.015***	0.407**	0.003*	8.400	0.710*
	(0.027)	(0.004)	(0.005)	(0.166)	(0.002)	(5.254)	(0.370)
Individuals	86,522	78,286	80,822	47,618	152,052	152,052	152,021

Notes: This table presents the estimated effect of being born just before the school entry cutoff using a balanced panel of women with both first and second births. The sample is limited to mothers with at least two years between births in order ensure meaningful gaps between the timing of outcomes. Each column provides results for a different outcome: imputed years of education (column 1), working in the previous year (column 2), holding a professional occupation (column 3), average occupational prestige (column 4); child birth weight (column 5); log birth weight (column 6); child gestational age (column 7). Labor market outcomes are available from 2007–2017; while child outcomes are available from 1989-2017. See the notes of Figure 3 for further details about the birth records sample and estimation.

Table A.16: Testing for the Role of Non-Education-Based Channels (Birth Records Sample)

	All	Teachers				All
	Job requires college diploma	Grade ≥ kindergarten	Kindergarten	Elementary or secondary	Post-secondary	Job requires exam only
Born before cutoff	-0.002	-0.002	-0.000	-0.001*	-0.000	-0.001
	(0.002)	(0.001)	(0.000)	(0.001)	(0.000)	(0.001)
Mean above cutoff	0.029	0.010	0.001	0.006	0.001	0.011
Individuals	209,308	209,308	209,308	209,308	209,308	230,409

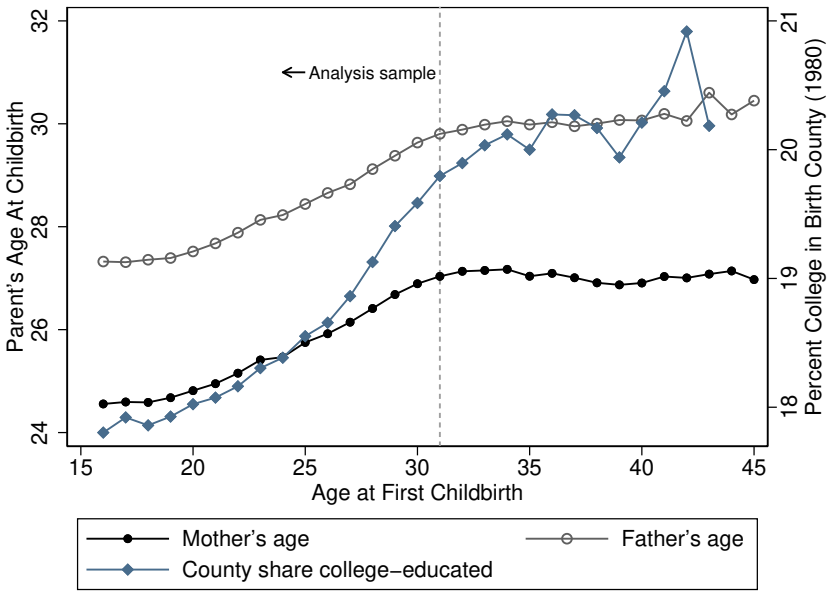
Notes: This table presents the estimated effect of being born just before the school entry cutoff on having an occupation requiring a college degree. Each column shows results for a different outcome: having a job that requires a college degree (column 1); being a teacher for a kindergarten or a higher grade (column 2); being a kindergarten teacher (column 3); being an elementary or secondary teacher (column 4); being a post-secondary teacher (column 5); or having a job that requires an exam only (column 6). See the notes of Figure 3 for details about the birth records sample and estimation.

Table A.17: No Discontinuity in Partner Characteristics at Mid-Career (Birth Records Sample)

	Present	High school graduate	Age	Working last year	Professional
Born before cutoff	-0.000	-0.000	0.124***	0.000	-0.000
	(0.002)	(0.002)	(0.045)	(0.001)	(0.005)
Mean above cutoff	0.956	0.927	30.371	0.979	0.383
Individuals	230,674	216,266	220,624	201,557	183,227

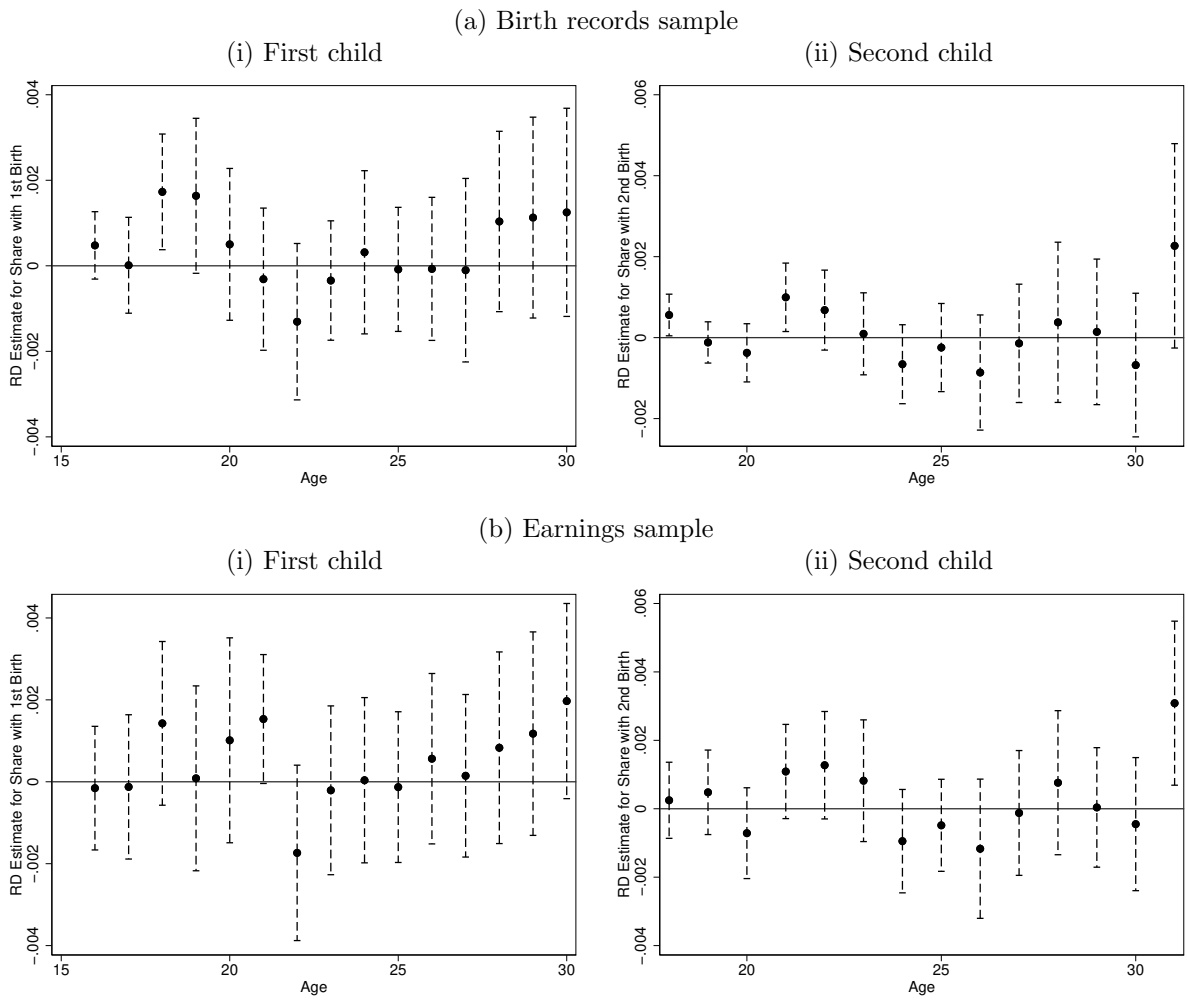
Notes: This table presents the estimated effect of being born just before the school entry cutoff on partner characteristics for women in mid-career (ages 25-40). Partner characteristics include: presence on the birth certificate (column 1), being a high school graduate (column 2); age (column 3); working in the past year (column 4); and having a professional job (column 5). See the notes of Figure 3 for details about the birth records sample and estimation.

Figure A.1: Background Characteristics by Age at First Childbirth (Birth Records Sample)



Notes: This figure presents the average background characteristics of women by age of first childbirth. Characteristics include mother's age at childbirth, father's age at childbirth (both on left y-axis), and the share college-educated in one's county of birth (right axis). The college-educated share is calculated from 1980 Census counts, via the IPUMS National Historical Geographic Information System (Manson et al., 2022).

Figure A.2: No Discontinuity in Likelihood of Giving Birth Across Age Distribution

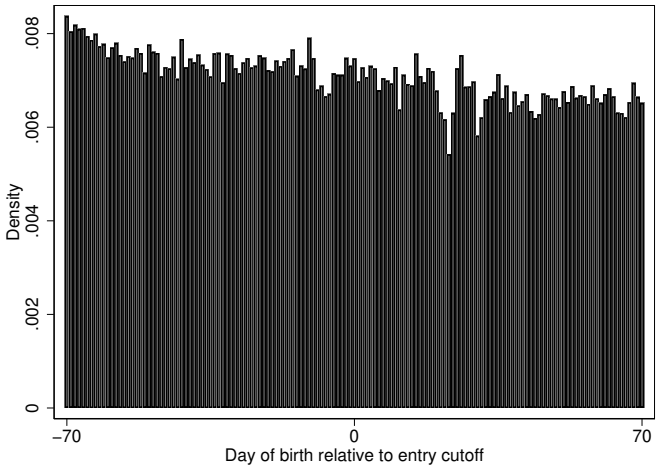


Notes: This figure presents the estimated effect (with 95% confidence intervals) of being born just before the school entry cutoff on having a first child (panel a) or second child (panel b) at each age. Panel A shows results for the birth records sample; Panel B shows results for the earnings sample. See the notes of Table 1 for details about the birth records and earnings samples and estimation.

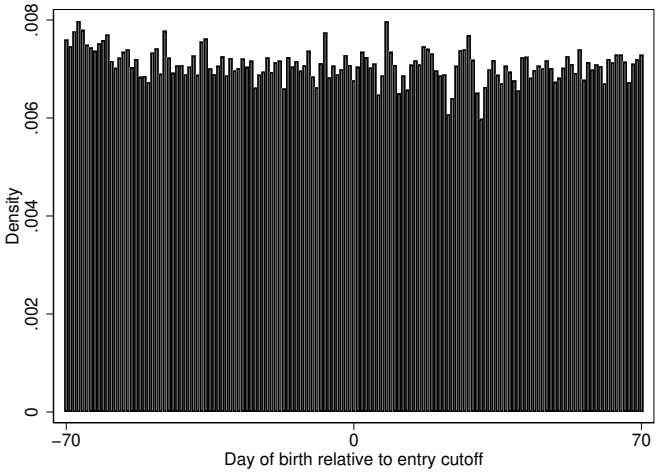
Figure A.3: Density of Birth Dates Around the Cutoff in Early- and Mid-Career

(a) Birth records sample

(i) Younger than 25

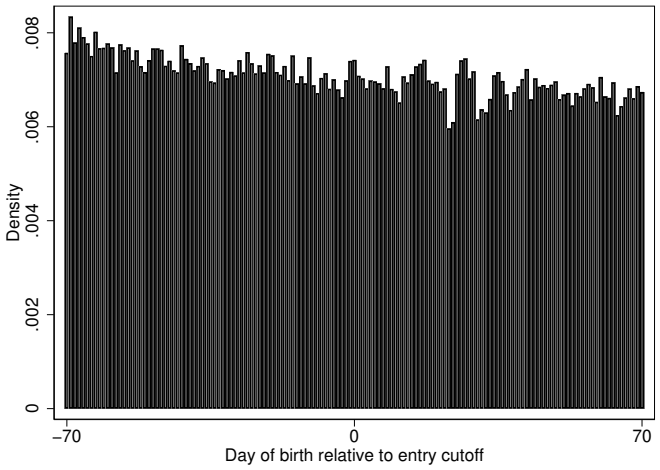


(ii) Age 25 or older

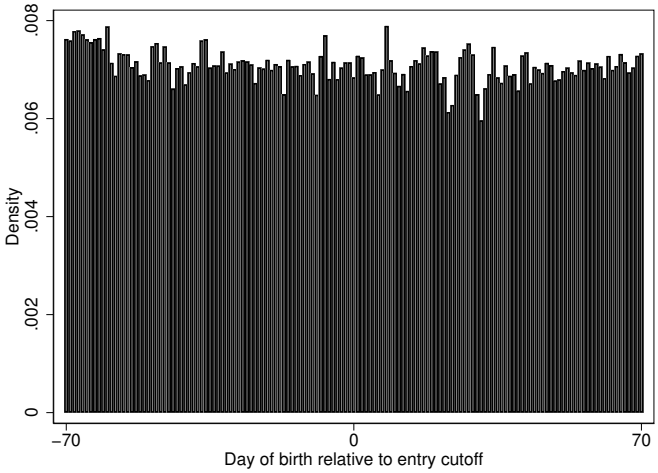


(b) Earnings sample

(i) Younger than 25



(ii) Age 25 or older



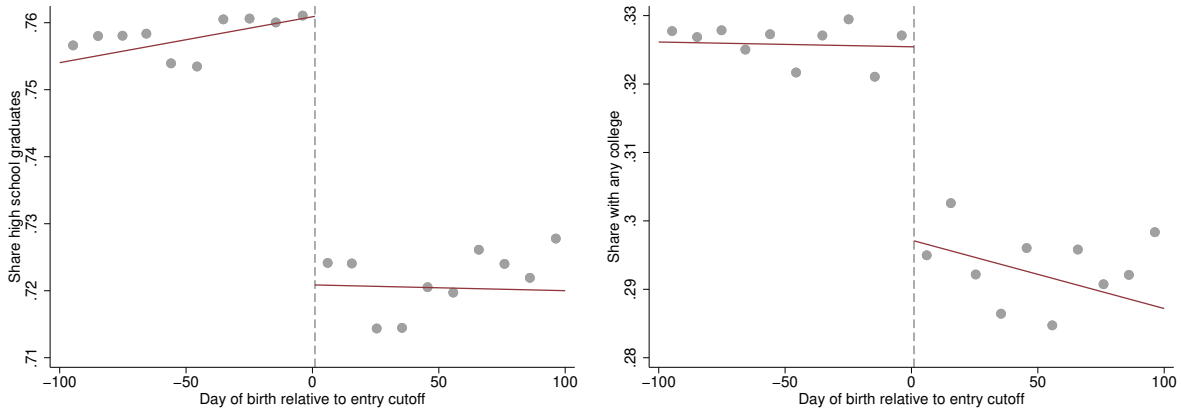
Notes: This figure presents the histogram of dates of birth around the cutoff. Panel A shows results for the birth records sample; Panel B shows results for the earnings sample. Subpanel (i) includes women in early career (ages 16-24); and subpanel (ii) includes women in mid-career (ages 25-40).See the notes of Table 1 for details about the birth records and earnings samples and estimation.

Figure A.4: Discontinuity in Secondary and Post-Secondary Education at School Entry Cutoff (Birth Records Sample)

(a) Younger than 25

(i) High school or more

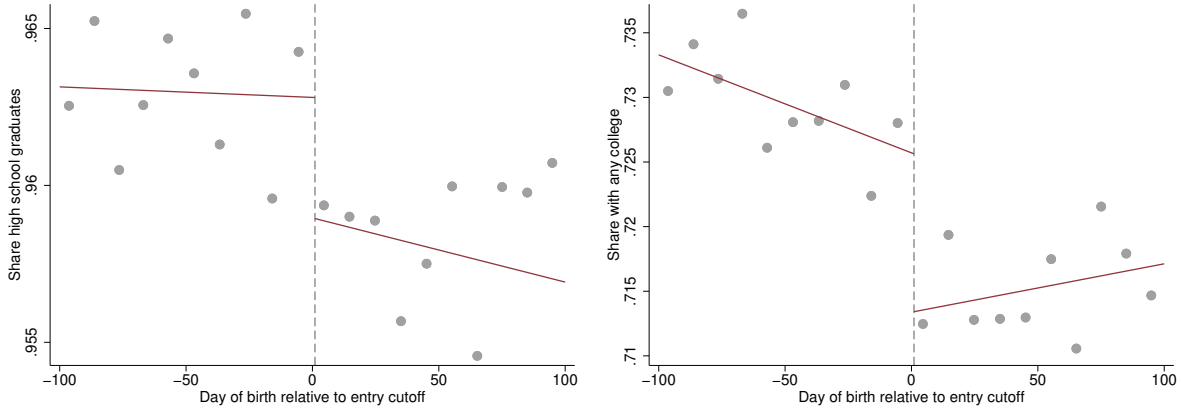
(ii) Some college or more



(b) Age 25 or older

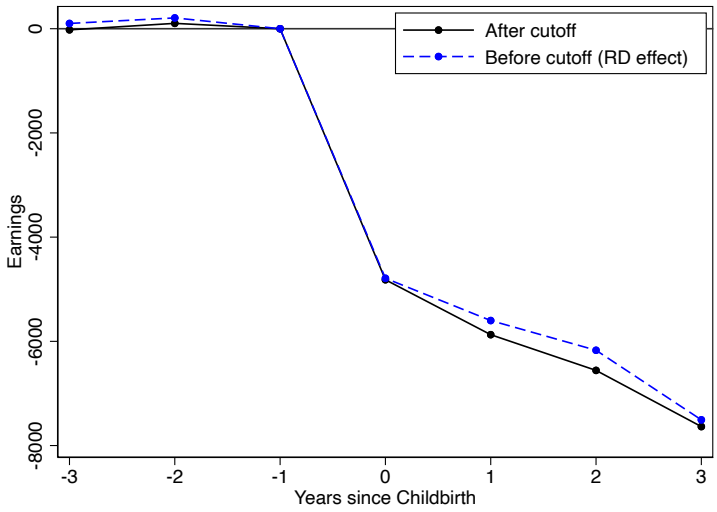
(i) High school or more

(ii) Some college or more



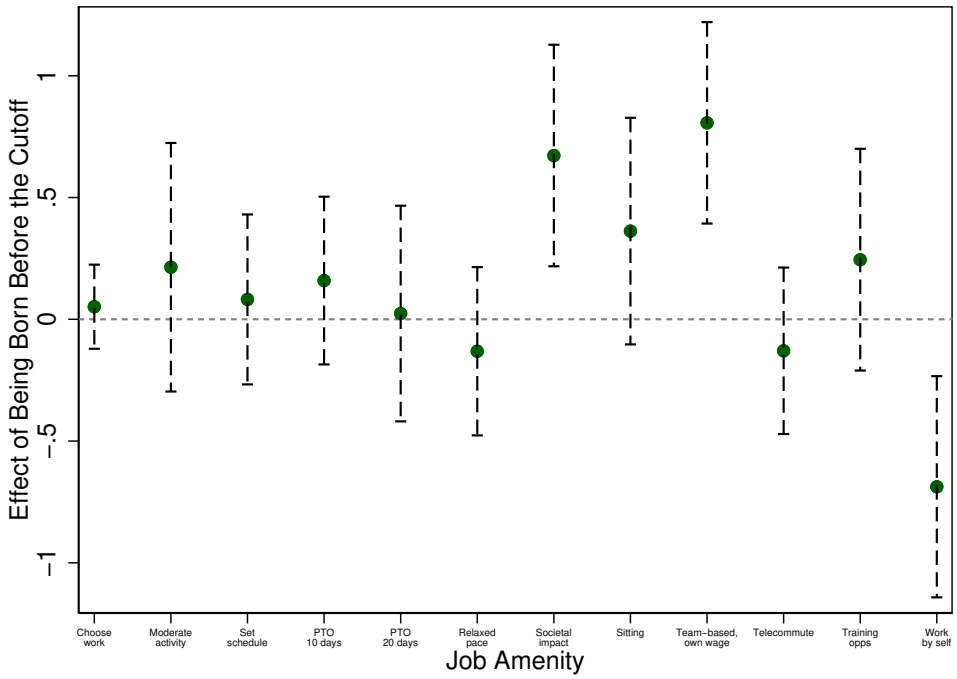
Notes: This figure presents the average secondary and post-secondary education outcomes for women born around the school entry cutoff. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Panel A shows outcomes for women in early-career (ages 16-24); Panel B shows outcomes for women in mid-career (ages 25-40). Outcomes include high school completion (subfigure i) and attendance of any college (subfigure ii). Source: California birth records, 2007–2017.

Figure A.5: Child Penalty in Earnings Among Women Born Around the Cutoff (Earnings Panel)



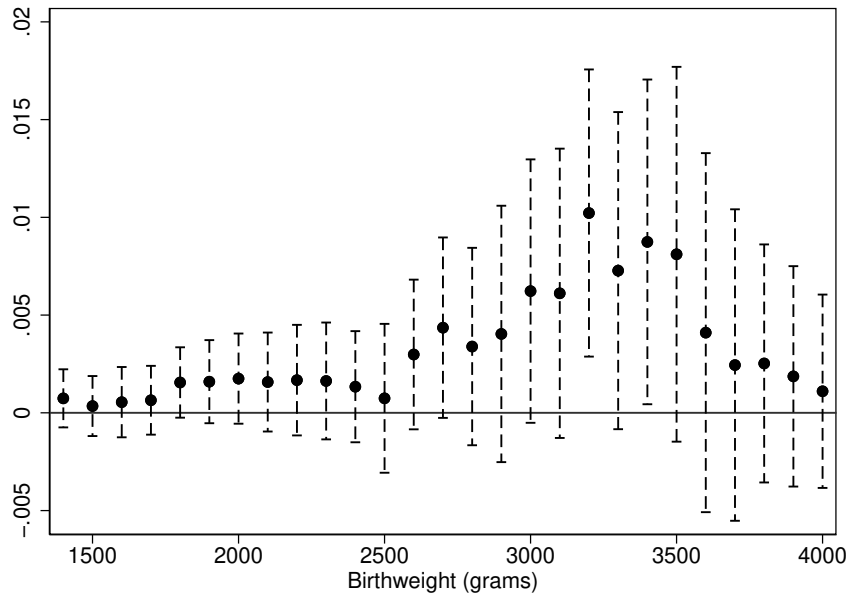
Notes: The solid black line in the figure presents the change in women's earnings in each year around a first birth, relative to the year before birth, among women born after the cutoff. The dashed blue line shows the counterfactual change for women born after the cutoff, by adding the RD treatment effect in each event year, normalized by the year prior to birth. See the notes of Figure 5 for details about the data and estimation.

Figure A.6: Impact of the Cutoff on Working Conditions (Birth Records Sample)



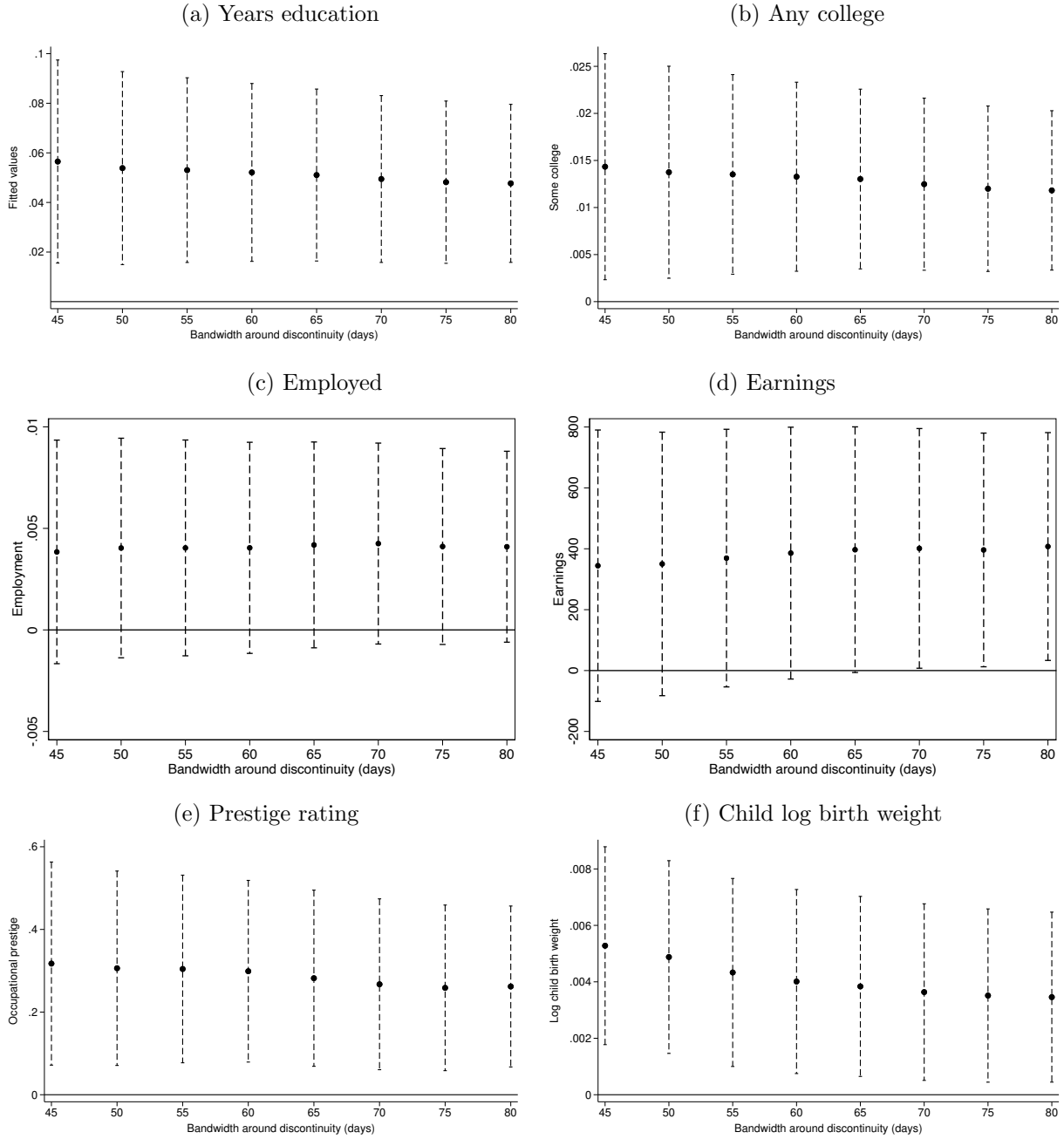
Notes: This figure presents the estimated effect (and 90% confidence interval) of being born just before the school entry cutoff at mid-career on job amenities. The outcomes are occupation-level average working conditions calculated from the American Working Conditions Survey (Maestas et al., 2017), described in Appendix Section A.2. See the notes of Figure 3 for further details about the birth records sample and estimation.

Figure A.7: Discontinuity in Distribution of Birth Weight at Mid-Career (Birth Records Sample)



Notes: This figure presents the estimated effect (and 95% confidence interval) of being born just before the school entry cutoff on the likelihood of having a child with a birth weight greater than X, where X is shown on the x-axis. See the notes of Figure 3 for details about the birth records sample and estimation.

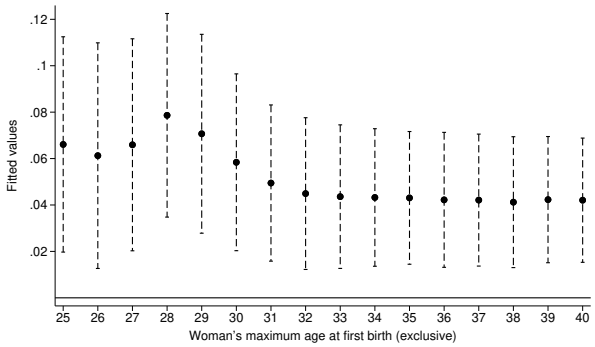
Figure A.8: Effects at Mid-Career using Alternative Bandwidths



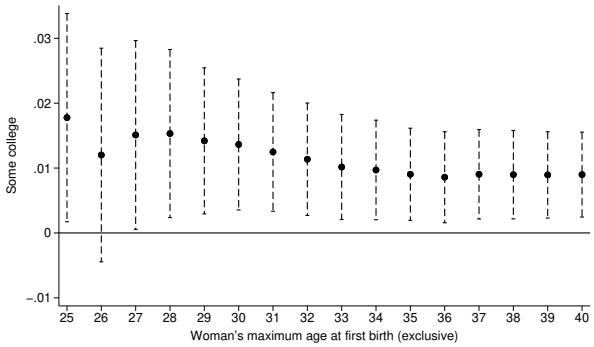
Notes: This figure presents the estimated effect of being born just before the school entry cutoff at mid-career across bandwidths, along with 95% confidence intervals. Each subfigure presents a different outcome: imputed years of education (Panel A), completion of some college (Panel B), working in the previous year (Panel C), average occupational prestige (Panel D), child birth weight (Panel E); log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation.

Figure A.9: Effects at Mid-Career using Alternative Age at First Birth Cutoffs

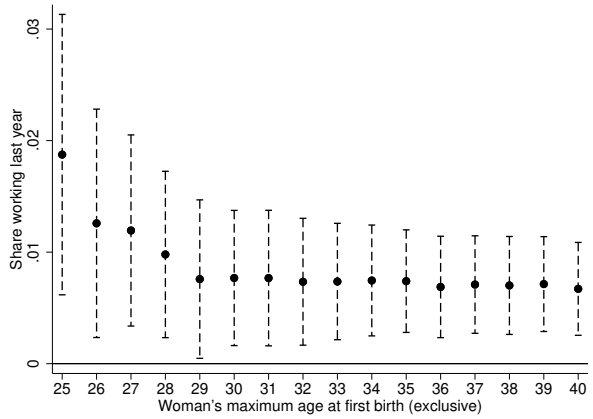
(a) Years education



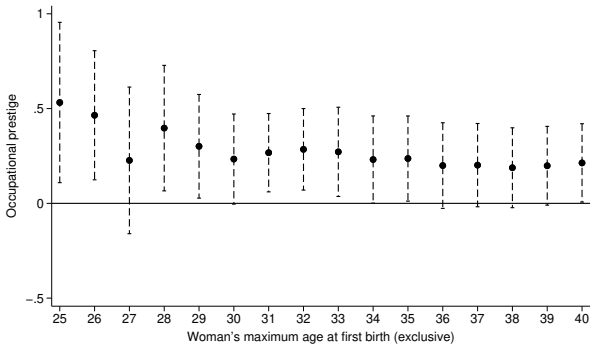
(b) Any college



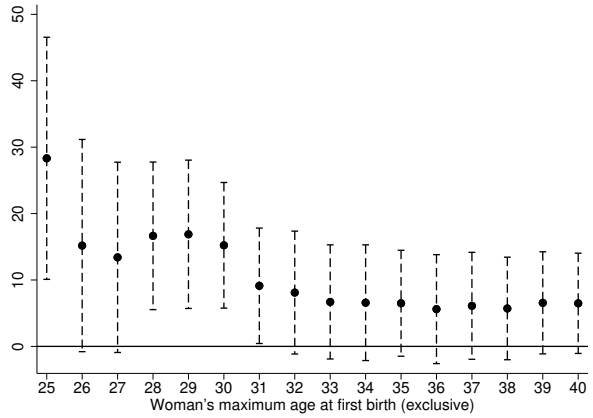
(c) Worked last year



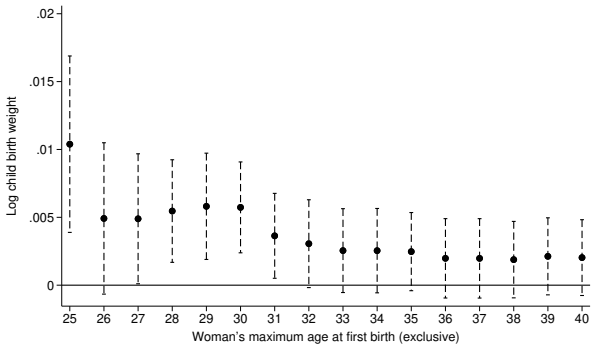
(d) Prestige rating



(e) Child birth weight

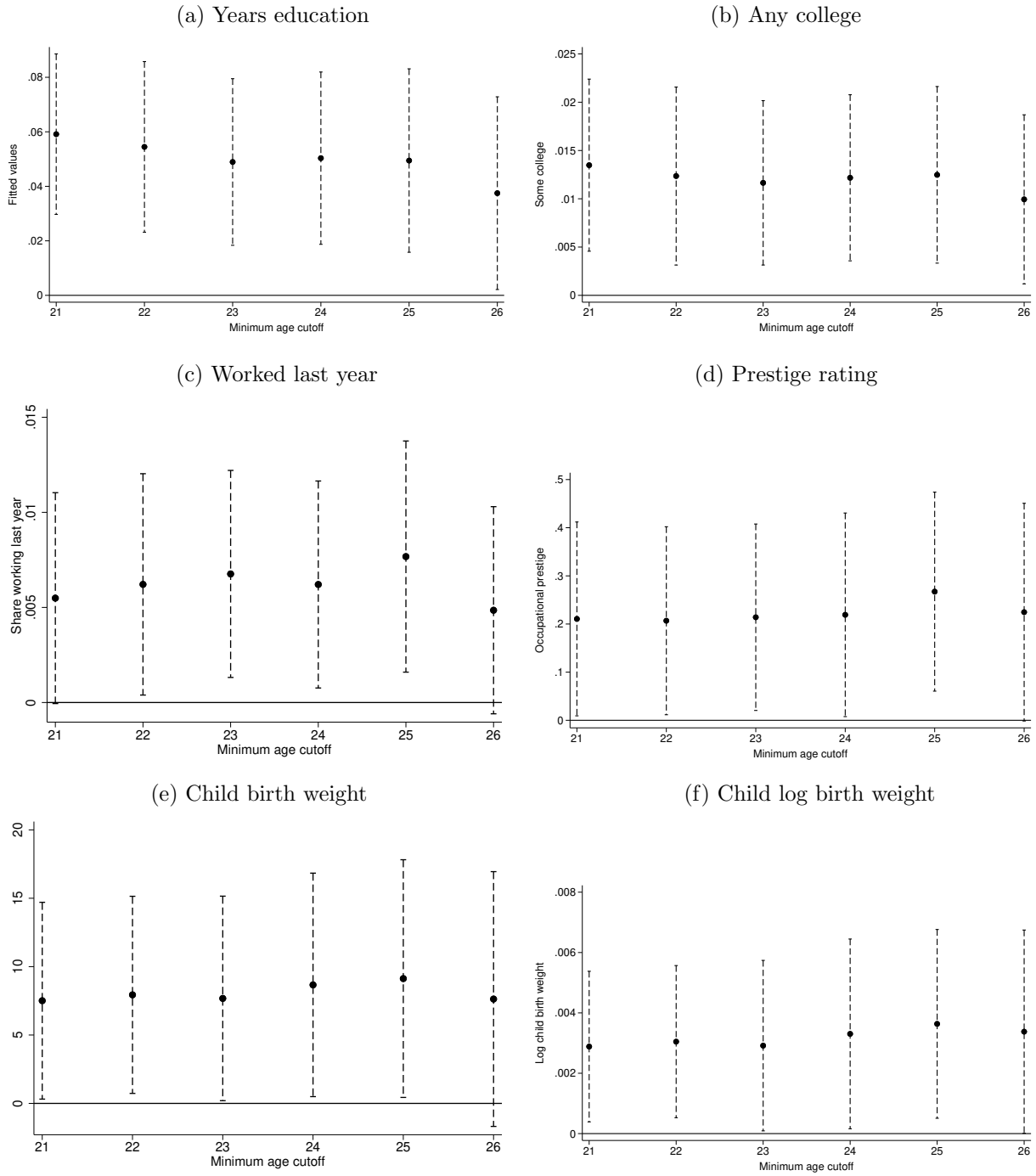


(f) Child log birth weight



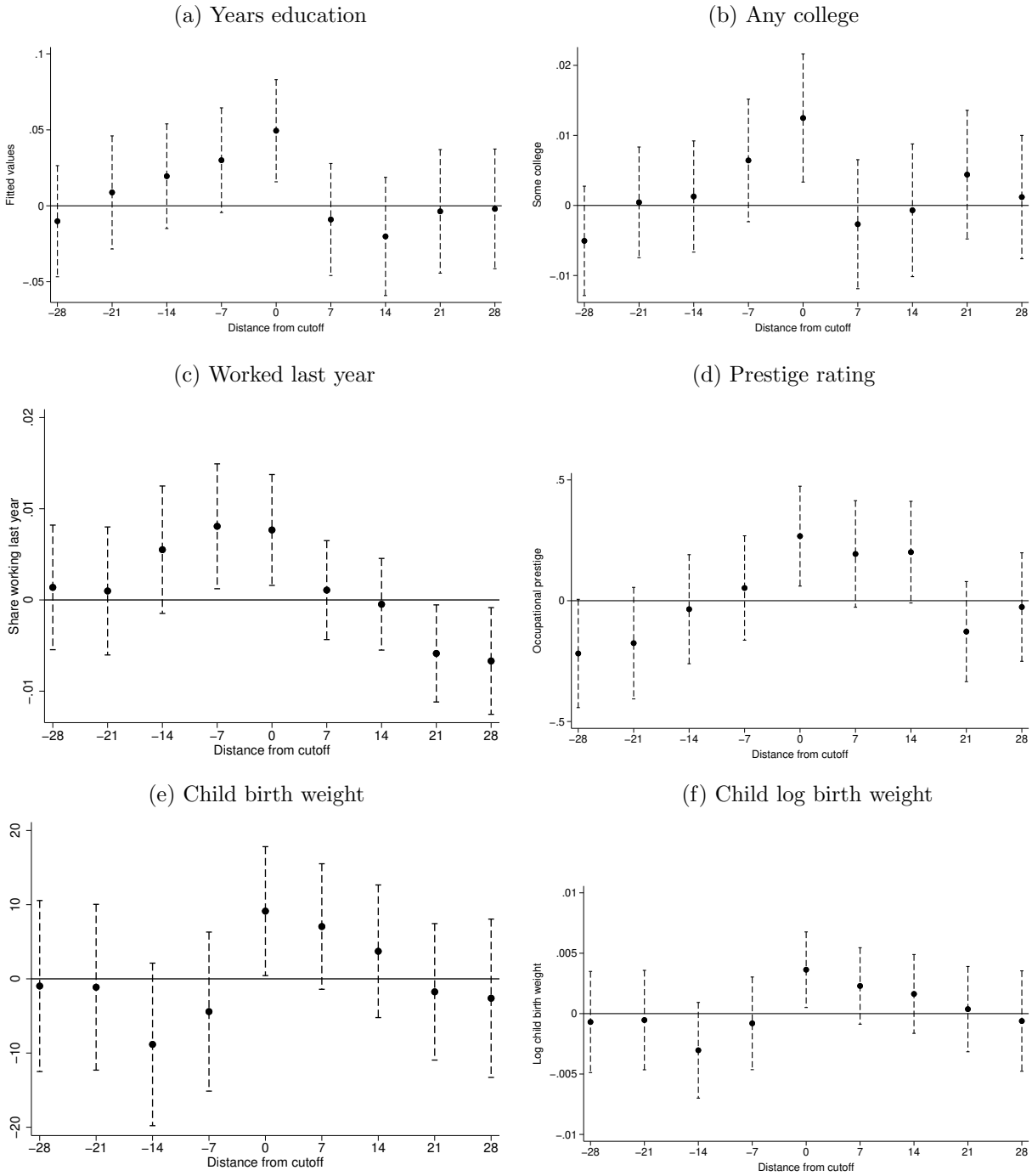
Notes: This figure presents the estimated effect of being born just before the school entry cutoff at mid-career using different sample cutoffs for age at first birth, along with 95% confidence intervals. Each subfigure presents a different outcome: imputed years of education (Panel A), completion of some college (Panel B), working in the previous year (Panel C), average occupational prestige (Panel D), child birth weight (Panel E); log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation.

Figure A.10: Effects Using Alternative Cutoffs for Mid-Career



Notes: This figure presents the estimated effect (with 95% confidence intervals) of being born just before the school entry cutoff at mid-career, varying the minimum age for mid-career. Each subfigure presents a different outcome: imputed years of education (Panel A), completion of some college (Panel B), working in the previous year (Panel C), average occupational prestige (Panel D), child birth weight (Panel E); log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation.

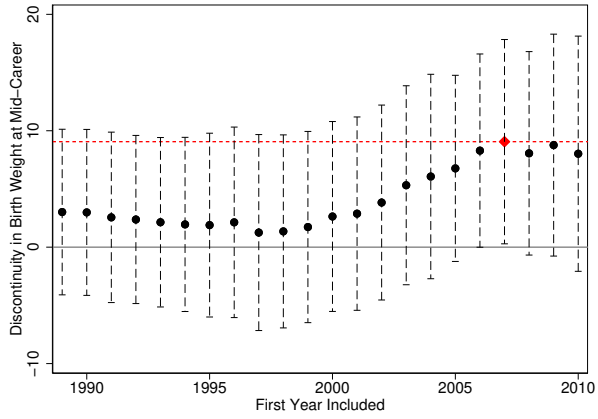
Figure A.11: Effects at Mid-Career using Placebo Cutoffs



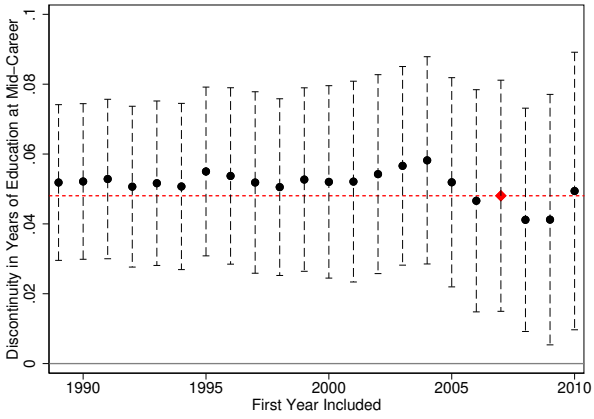
Notes: This figure presents the estimated effect of being born just before the school entry cutoff at mid-career for placebo school entry cutoffs, along with 95% confidence intervals. The placebo cutoffs are chosen within an eight-week window around the cutoff and spaced apart by two-weeks. Each subfigure presents a different outcome: imputed years of education (Panel A), completion of some college (Panel B), working in the previous year (Panel C), average occupational prestige (Panel D), child birth weight (Panel E); log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation.

Figure A.12: Intergenerational Transmission Increases over Time (Birth Records Sample)

(a) Child birth weight



(b) Years of education



Notes: This figure presents the estimated effect (with 95% confidence intervals) of being born just before the school entry cutoff on outcomes at mid-career, as we use increasingly more recent years of birth records. Each marker shows the estimate from a regression that includes the years from X until 2017, where X is shown on the x-axis. The baseline estimate is shown in the red diamond. Panel A shows effects on birth weight; Panel B shows effects on predicted years of education. See the notes of Figure 3 for further details about the baseline birth records sample and estimation.