Life-Cycle Effects of Women's Education on their Careers and Children

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Abstract

We study the causal effect of women's education on their wages, non-wage job amenities, and spillovers to children. Using a regression discontinuity at the school entry birthdate cutoff, we find that women born just before the cutoff are more likely to complete some college, and experience multi-dimensional career gains that grow over the life cycle: greater employment and earnings, as well as more professional and higher-status jobs, more socially meaningful work, and better working conditions. Children's early-life health and prenatal inputs improve in tandem with career improvements, consistent with professional advances spurring—not hindering—infant investments. Career gains are concentrated in jobs that require *exactly* some college, the same schooling margin shifted by the cutoff, which indicates that increased post-secondary education is the primary channel for these effects. Together, the results show that women's college attendance generates large career returns—from both wages and amenities—that strengthen over time and produce meaningful benefits for children.

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Over the last half century, women's college attendance has risen sharply alongside substantial shifts in their careers. This educational transformation is often credited for increasing wages; but, less noted, is that core *non-pay* aspects of work ("amenities")—such as the content of women's jobs, their working conditions, and their status in the office—have also improved (Goldin, 2006; Black and Spitz-Oener, 2010; Blau and Kahn, 2017). Despite these historical trends, the *causal* effect of education could involve higher wages at the expense of having lower-amenity jobs (e.g., due to higher-paying jobs being more demanding or stressful, or other forms of compensating differentials; Rosen, 1986). Either way, the value of these professional changes—positive or negative—may be substantive, as women value many amenities on par with moderate wage increases (e.g. Wiswall and Zafar, 2017; Maestas et al., 2023; Bottan et al., 2025). This raises the question of whether measuring schooling returns solely by wages accurately reflects the labor market value of education.

These impacts on women's careers—wages and non-wage amenities—may also shape investments in children—especially during sensitive stages of early development—but, again, the direction and mechanisms for this link remain unclear.[1] While greater income improves infant health, less is known about how other aspects of work change with education, and how these might influence child investments.[2] For example, better amenities can improve the prenatal environment—and, in turn, early child outcomes—by reducing stress or increasing exposure to healthier peers; while worse amenities may have the opposite effect (Jena et al., 2023).

This paper studies the impacts of greater schooling on women's earnings, job amenities, and their children's health over the life cycle. In the spirit of earlier work, we exploit a regression discontinuity that occurs at the school-entry birthdate cutoff, whereby younger entrants born just before the cutoff complete more schooling (e.g. Angrist and Krueger, 1991; McCrary and Royer, 2011). We combine employment surveys, administrative earnings, and confidential health records for mothers from California to estimate intent-to-treat effects of being born just before (versus after) the entry cutoff on the outcomes of women and their children, tracing these benefits from early-

[1] Prior intergenerational studies typically study infant outcomes or parental career effects in isolation, or examine longer-run child outcomes (e.g., Currie and Moretti, 2003; McCrary and Royer, 2011; Black et al., 2005; Oreopoulos et al., 2006; Björklund et al., 2006; Sacerdote, 2007; Akresh et al., 2022); we connect impacts on mothers' careers and child investments in a single life-cycle design.

[2] See Page (2024) for a recent review of links between income and child outcomes.

career (ages 16-24) through mid-career (ages 25-40). To isolate the role of schooling, we introduce and test a simple prediction: education-driven career effects should cluster in jobs requiring the affected schooling margin—not be uniform across skill levels—and should be precisely zero in jobs that require more schooling. Our data on detailed occupation titles facilitate this test.

To begin, we find that in line with past work, there are sharp jumps in completed education for women born just before the cutoff: 0.11-0.16 more years in early career, and 0.05-0.08 more years in mid-career. Early-career gains span the distribution, but the persistence in mid-career is mainly due to higher rates of *some college*—making college attendance the key margin for long-run outcomes. At the same time, balance tests show that women's pre-determined characteristics and fertility (both rates and timing) are smooth around the cutoff, supporting the exogeneity of the cutoff.

Following women over time, we document four reduced-form results. First, women born before the cutoff have higher employment and compensation: by mid-career, they are 0.7 percentage points (p.p.) more likely to be working and earn $783 more, with smaller effects early on. As careers progress, gains also shift up the earnings distribution, leading to larger increases in earnings above the poverty line in mid-career. While the well-documented child penalty could in principle erase the initial career gains to mothers, we find that these labor market gains do not fade after childbirth. Consequently, mothers have greater financial resources during pregnancy and into their children's early years.

Second, beyond earnings, we find that women shift towards higher status, more-selective jobs—particularly later in their careers. By mid-career, women born just before the cutoff are 1.3 p.p. (3.6%) more likely to hold professional roles and 0.8 p.p. (6.8%) more likely to have jobs that require a college credential. Occupational prestige (à la North and Hatt, 1947, and the subsequent literature that uses this rating to measure social status) also increases, with women being more likely to have jobs in the top-half of the prestige distribution. These status gains may add to the intrinsic value of work (Bottan et al., 2025), and could also generate more tangible benefits—most notably, better medical care (Schwab and Singh, 2024).

Third, working conditions also improve, as women are more likely to work in jobs with higher

peer quality and a stronger public service orientation. At mid-career, women born before the cutoff are in jobs with more college-educated coworkers (by 0.7%), higher social impact (by 1.3%), and greater teamwork (by 1.4%), with little to no impact on typical hours of work, scheduling flexibility, or the pacing of work. The rise in social impact is at least partly explained by an 0.8 p.p. (12.5%) increase in the likelihood of holding educator positions, particularly in early-childhood settings. These shifts further increase the value of work (Maestas et al., 2023), and suggest new channels for intergenerational spillovers—for example, if better peers facilitate access to health resources, or work-sharing reduces stress. Again, these effects are typically smaller in early career. To the extent that these effects reflect greater schooling (as discussed further below), these results are consistent with increasing returns to education over the life cycle.

Fourth, children's birth outcomes and women's prenatal inputs improve in tandem with careers. Children born in mid-career to women born before the cutoff weigh 0.4 percent (9 grams) more and are born 0.45 days later. In contrast, there are no effects in early career, consistent with the smaller effects on earnings and jobs. Effects on birth weight occur throughout the distribution, indicating broad gains in child health. Alongside health gains, smoking during pregnancy declines by 0.5 p.p. (12.5%), and mothers are less likely to rely on safety net programs (Medi-Cal and WIC), in line with the decline in poverty. The fact that improvements in child health coincide with women's career gains (and particularly declines in poverty), suggest a positive link between women's professional advances and infant outcomes, which may be mediated by the rise in prenatal investments.

We provide evidence that these differences around the school-age cutoff are driven by education, rather than other explanations such as the possible benefits of being younger than one's classmates. Mid-career effects are concentrated in jobs that require *some college*—the exact margin shifted by the cutoff—which account for 62% of the increase in employment. In contrast, we find precise zero effects on jobs requiring more schooling (a BA) or less (an exam). These stark credentialing patterns only match the predicted effects of education. We also find little effect on partner characteristics, ruling out changes in spousal selection. Further, we infer that marginal students likely attend two-year colleges, suggesting that career gains may come at a relatively low cost.

Building on this schooling-based interpretation of our results, we use our mid-career estimates to

derive two broader implications: (i) the implied per-year schooling return and (ii) the contribution of working conditions, focusing on twelve core job attributes (e.g., social impact, teamwork) for which we have willingness-to-pay (WTP) estimates from Maestas et al. (2023). We estimate that the implied wage return per year of schooling is 33.3%, and that improvements among the 12 monetized amenities contribute an additional 3.3%, raising the total labor market value by 10 percent. Further, because workers plausibly value job features outside the monetized set that also increase with education (e.g., social status and peer quality), this is likely a loose lower bound for the full amenity-based value of schooling. Our estimates also suggest meaningful spillovers to children: birth weight rises by 5% per year of schooling, which prior work suggests could raise their adult earnings by about 0.5%.

Our results are visually transparent and robust. The discontinuities in outcomes are visible in scatter plots and occur precisely at the entry cutoff, consistent with the sharp policy change at the cutoff. Changes in estimation methods or sample do not affect our results. This includes applying alternative bandwidths and inference procedures, using Lee (2009) bounds to address potential selection into employment, or limiting to a balanced panel of mothers. We also use placebo cutoffs to rule out the possibility that our results could be spurious. Last, we show that our employment results are virtually unchanged across two independent data sources (birth records and tax records), which ensures that our findings are not artifacts of idiosyncratic data features.

This paper builds on and extends multiple literatures. First, we bring together the literature on the wage returns to two-year credentials (e.g. Jacobson et al., 2005; Stevens et al., 2019; Grosz, 2020; Mountjoy, 2022; Minaya and Scott-Clayton, 2022) and the literature on the effects of maternal education on children's health (Currie and Moretti, 2003; Lindeboom et al., 2009; McCrary and Royer, 2011; Schulkind and Yan, 2025).[3] These two strands have proceeded in isolation, leaving open the question of how education influences maternal careers specifically (since, e.g., if education induces marginal students to enter high-hours or inflexible occupations, career gains may not persist

[3]While existing studies find that women who attend college have higher wages, impacts on their infants are more mixed: Currie and Moretti (2003) find positive effects, while McCrary and Royer (2011) find none. This raises a puzzle about whether changes in income and other working conditions are heterogeneous across mothers or do not consistently pass through to infant health. Differences in maternal age may also matter: McCrary and Royer (2011) focus on early-career births, while Currie and Moretti (2003) focus on mothers more broadly. By following women over time, we show that infant impacts are smaller in early career.

once they have children; Goldin, 2014; Cubas et al., 2019), and whether positive impacts on maternal careers translate to positive impacts on children. Our paper is the first to bring together data that allows us to jointly study the impacts of education on *maternal* careers—wages and non-wage amenities—and the subsequent spillovers to children.

By analyzing women's careers and infant health in a single framework, we uncover three key insights. First, greater education generates persistent earnings gains for mothers, leading to greater financial resources for families when kids are young (and possibly afterward). Second, the benefits for maternal careers extend beyond wages—into higher-status roles, greater social meaning, and valued working conditions—channels that plausibly improve the prenatal environment and child health. Third, prenatal investments grow alongside better maternal careers, indicating that women's employment is not a barrier—and may be a catalyst—to infant development. While prior work explores aspects of this link (e.g., the impact of maternal income in Hoynes et al., 2015 or physician's long hours in Jena et al., 2023), we identify education as an upstream policy lever, map a portfolio of wage and non-wage pathways (and show that these tend to move together), and track dynamics over the life cycle.[4] Taken together, these results suggest that maternal careers may be an important conduit for education's long-run benefits for children (e.g., Black et al., 2005).

Second, we extend the research on the ties between education and non-pay aspects of work. This small body of work finds that college diplomas shift men into elite occupations in France (Maurin and McNally, 2008) and selective college majors increase employment in related industries (Bleemer and Mehta, 2022; Grosz, 2020). We broaden this work by (i) examining effects on a large set of job attributes spanning private and social aspects of work, including social status, social impact, and working conditions and (ii) monetizing improvements in working conditions using WTP estimates from Maestas et al. (2023). We show that post-secondary education shifts women into more meaningful jobs—an amenity highly valued by women—and does not come at the cost of greater hours, less flexibility, or more fast-paced work. These effects on non-wage amenities also contribute materially to the return to schooling.

[4]The prenatal complementarity that we document contrasts with evidence that maternal employment and child investments can trade off in early childhood (Baker et al., 2008, 2019), perhaps because maternal time is a more crucial input at that stage.

Third, we build on the work on long-run labor market effects of school entry cutoffs (Dobkin and Ferreira, 2010; Black et al., 2011; Fredriksson and Öckert, 2013; Oosterbeek et al., 2021). Unlike previous U.S. evaluations that find null career effects (Dobkin and Ferreira, 2010), we examine a richer set of outcomes—including labor force participation, administrative earnings, occupations, and workplace amenities—which provides a more complete assessment of policy impacts. We also focus on a sample of recent mothers, which allows us to test for the persistence of the effects after having children. We find that early school entry has substantial positive effects on women's professional development. Further, we introduce a simple diagnostic that uses occupational skill requirements to separate the effects of younger age-for-grade from greater schooling, offering a useful tool for future work on the causal effects of education. We implement this test using novel occupation data for all California mothers, which provides a substantially larger sample than typical job surveys.

1 School Entry Policies and Conceptual Framework

To study the multigenerational effects of women's schooling, we exploit a sharp discontinuity in completed education at California's school entry cutoff: individuals born just before the cutoff complete more schooling than those born just after. This regression-discontinuity design follows prior work on returns to schooling (e.g., Dobkin and Ferreira, 2010; McCrary and Royer, 2011). In this section, we first summarize the policies in California that generate this discontinuity, and then outline a simple framework that we use to consider the potential channels for downstream effects.

1.1 California School Entry and Exit Laws

California has two age-based school policies that help explain the break in education at the entry cutoff. First, children may enroll in kindergarten only 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).[5] This entry rule leads to a sharp

[5]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.

jump in expected entry age: 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, indicating strong, but imperfect compliance with this policy (Elder and Lubotsky, 2009). Notably, girls comply with these policies more than boys, implying that the discontinuity may be particularly pronounced for our sample (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 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).

1.2 Expected Effects of Early School Entry on Careers

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

Greater years of schooling First, early entry may shape 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 than those who do not (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, licensed jobs (Kleiner and Soltas, 2019). Greater schooling could also affect non-wage aspects of jobs, such as working conditions and coworker interactions. Descriptively, more education is associated with a higher quality work environment— more flexibility, less-physically-strenuous tasks, and greater social meaning (Maestas et al., 2023). However, evidence from firm movers suggests that higher pay can come bundled with more demanding conditions (Humlum et al., 2025). These patterns produce an ambiguous prediction about the causal effect of education on job amenities.

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 less clear. On 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 more likely 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 Third, schooling or relative age could shift partner selection, 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.

1.2.1 Empirical tests of the education mechanism

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 compare how the cutoff shifts (i) the education credentials required by women's jobs and (ii) women's own completed schooling. The logic behind this is straightforward: if schooling is the primary channel, these effects should align closely. For example, if the cutoff primarily increases completion of some college (as we find), career impacts should be concentrated in jobs that require some college. Effects may also spill over to jobs that require less than some college, since added education may raise workers' attractiveness more broadly; however, the cutoff should not affect employment in jobs that require a college degree. In contrast, if younger age-for-grade is the main channel, job impacts should be spread more evenly across skill levels. We implement this test by contrasting effects on jobs whose requirements *match* versus *do not match* the schooling margin, and use their ratio 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 peer composition, and partner's completed education, employment, and occupation, as evidence of changes in assortative mating. These detailed measures help isolate different sources for changes in partners and their scope to affect careers.

2 Data

Our analysis draws on two main data sources: (i) microdata of birth certificates from California and (ii) administrative tax records merged with Census surveys. The birth records provide information on women's education and occupation (which we use to proxy job amenities) as well as children's early-life health at the time of each delivery. The linked tax data allow us to track women's earnings annually over the life cycle. Below we describe the data construction and key measures by source. Additional details on the data sources and outcome definitions are available in Appendix B.

Vital Statistics birth records The California birth certificates data span from 1960–2017, and record detailed characteristics of parents and children at the time of delivery. Our main analysis uses records from 2007–2017—the period when parental labor market outcomes are available—and, where needed we draw on earlier records (e.g., to calculate fertility rates and determine age at first birth).[6]

For parents, we observe a variety of background characteristics (name, date of birth, state of birth, race), completed education—binned, and used to impute years of schooling—and recent work history.[7,8] Professional details include usual occupation and industry, and last month worked. Job titles are unstructured, free text, and often include roles outside of the labor force (e.g., homemaker or student). For tractability, we harmonize titles to the 2010 Census SOC codes (see Appendix B1

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

[7]State of birth is not available for fathers, which prevents us from studying the effects of the cutoff for men.

[8]We use earlier records (from 1989 to 2005) to predict years of education. 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.

for details).[9] We show that these labor market measures closely track employment in national surveys and administrative records (see Sections 3 and 5.2).

For children, we observe precise markers of early-life health and prenatal care. As indicators of child well-being, we use birth weight and gestational age, due to strong correlations with later-life cognition and labor market outcomes (Black et al., 2007; Figlio et al., 2014). Maternal health inputs include smoking during or preceding pregnancy, prenatal doctor visits, and participation in public programs.

Census and IRS records We use Internal Revenue Service (IRS) tax records compiled in the Opportunity Databank (Chetty et al., 2018). Individual earnings are available from 2005-2021, and come from wages reported by employers on IRS W-2 forms. Household earnings are available from 1998-2021, and are measured using the total wage and salary income reported on Form 1040 tax filings, when available, otherwise as the sum of spouses' W-2 earnings. We adjust all amounts to 2016 dollars using the CPI (U.S. Bureau of Labor Statistics, 2018).

We add demographic characteristics to the panel by drawing on 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 Census Numident file to attach the exact date and location of birth for each individual.

Job characteristics To characterize the non-wage amenities of parents' occupations, we assemble a wide range of average job characteristics for U.S. workers. See Appendix B2 for details. We study impacts across three domains of amenities: (i) social status; (ii) orientation towards public service; and (iii) working conditions—each of which may add labor market value to women and affect the prenatal environment of children.

Our measures of social status include an indicator for being a professional (since professionals rank near the top of occupational prestige scales; Treiman, 1977) from Autor and Dorn (2013);

[9] We use month last worked to create an indicator for having worked in the last year (excluding students). We set this to missing for those who report a date a year or more earlier (1%), as many of these dates appear unrealistic.

the minimum schooling required to obtain a license or credential (given the strong link between education and prestige; Hughes et al., 2024); and a percentile ranking of occupational prestige (from Hughes et al., 2024; Smith and Son, 2014).[10] Importantly, occupational prestige ratings—which are closely related to the other status measures—are highly stable over time, suggesting they capture a shared understanding of social standing (Hughes et al., 2024).

Our measures of social impact include the share of workers who report having frequent opportunities to serve the public, as in Maestas et al. (2023), and an indicator for being in a "high impact" profession (discussed in Section 5.2).

Our measures of working conditions include the eleven job amenities besides social impact evaluated and monetized in Maestas et al. (2023). These are designed to capture core job attributes valued by workers, including scheduling flexibility, physical demands, job pacing, teamwork, and paid time off.[11] To capture other dimensions of the work environment, we use the share of workers in an occupation who attended some college, as a proxy for peer quality; the degree of daytime scheduling inflexibility, defined as the 1 minus the typical share of hours worked between 8 a.m. and 5 p.m. (from Cubas et al., 2019); and the average hours worked per week. When available, we use job characteristics specifically for California workers.

Analysis samples We use two main 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 to entry cutoffs (see McCrary and Royer, 2011, and Section 6).[12] Within this sample, we focus on women ages 16 to 40, based on data availability. We apply these criteria separately in the birth and

[10]The prestige rating is a survey-elicited measure of occupational status, and is not directly linked to income—unlike other occupation-based measures of status, such as the Duncan Socioeconomic Index (Duncan, 1961).

[11]The specific amenities include: choosing how to do one's work (vs. well-defined tasks); setting one's own schedule (vs. having a schedule set by a manager); telecommuting; having moderate physical activity (vs. heavy physical activity); sitting (vs. heavy activity); relaxed pace (vs. fast pace); 10 days PTO (vs. no PTO); 20 days PTO (vs no PTO); teamwork evaluated on one's own performance (vs. on team's performance); working by oneself (vs. teamwork evaluated as a team); training opportunities (vs. already having skills); and frequent opportunity to serve the public (vs. occasional opportunity to serve).

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

earnings records, which allows us to analyze these sensitive data without linking them directly.

To improve balance across women, our final analysis samples impose one additional criteria. For the birth records, we limit to each woman's first two births (the complete fertility history for 80% of mothers), so analytical weights are roughly balanced across women. For the earnings records, we limit to the 1977–1987 cohorts, for whom we observe household earnings at every age from 21–34, and individual earnings at every age from 28-34. This ensures a relatively balanced panel when we estimate age-specific earnings effects (for ages 20-34).[13] We bridge the results across datasets by recreating these samples across sources; the "recreated earnings sample" in the birth records and the "recreated birth records sample" in the earnings records.[14,15] We show that results are robust to alternative sample criteria in Section 6.

Appendix Table A.2 compares our estimation samples to the broader population of California mothers (columns 1-2) using characteristics from the birth records. Our birth records sample (columns 3–4) has similar rates of high school graduation (85%) and employment (77%), but is younger and less college-educated. Our sample aligns more closely with non-college-educated mothers in California, 92% of which are included at baseline. The recreated earnings panel (column 5) has similar demographics to the birth records sample.

3 Descriptive Evidence

Figure 1 presents descriptive patterns relating women's completed education to their careers and their children's health—illustrating the scope for education-induced changes in professions to affect child well-being. The career patterns are generated using mothers in the birth records (where we observe education); however, we validate that they extend to non-mothers using women ages 16-40 years in the American Community Survey (ACS) for overlapping years (Ruggles et al., 2010).

Panels A and B of Figure 1 show steep education gradients in women's employment (Panel A) and professional occupations (Panel B), across four levels of education: no high school diploma,

[13] Patterns are unchanged when we restrict to a fully balanced panel (see Section 5.2).

[14] We use a the full span of education data in the birth records (1989 onward) to analyze the recreated earnings sample, allowing us to observe outcomes at younger ages.

[15] Appendix Table A.1 shows that the main and recreated samples match on demographics and sample sizes.

high school diploma/GED, some college, and college or more. Rates of work rise substantially from 37% (no high school) to 72% (high school), 81% (some college), and 93% (college). Professional employment also increases with education, from 16% of workers (no college) to 31% (some college) and 75% (college). Including non-workers widens gaps further (25% vs. 6% for some college vs. no high school). Reassuringly, patterns are similar in the ACS.

Panels C and D link maternal education to infant health, and show how careers may mediate that link. Panel C demonstrates that child weight increases with maternal education and employment. Birth weight differs by nearly 100 grams between least- and most-educated mothers. Further, *within* most education groups, children of working mothers weigh about 50 grams more than children of non-working mothers. This suggests that children of higher-educated mothers may, in part, benefit from having a working mother (e.g., via greater financial resources and prenatal inputs).

Panel D reveals that infant weight also correlates with maternal *profession*. Each marker is a maternal occupation, ordered by its college attendance rate. 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 birth dates around the entry cutoff. Specifically, we estimate intent-to-treat effects of being born just 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 DOB_i is the number of days between i's birth date and her cohort's entry cutoff; and BeforeCutoff_i indicates

$\text{DOB}_i < 0$. To increase precision, we include a fixed effect for birth year, ρ_b; and a vector of other individual covariates, X_{ia}, including fixed effects for a woman's age and race and, for child outcomes, for child parity and having multiple births. We recenter birth year and age around the cutoff, so individuals born nearby around the cutoff share the same calendar time and age support.[16]

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 early career (ages 16-24) and mid-career (ages 25-40). Our baseline estimates use triangular kernel weights, a 70-day bandwidth (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 be violated in two ways. First, there may be selection at *birth*: the types of women who are born on opposite sides of the cutoff may differ. This could occur if birth timing correlates with parent characteristics (e.g., seasonal jobs or health) (Buckles and Hungerman, 2013). Second, there may be selection into *motherhood*: 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 finds little evidence of these responses (McCrary and Royer, 2011).

We assess these concerns with two tests. First, we use binned scatter plots to show that outcomes change precisely at the cutoff. This allows us to rule out threats related to seasonal birth patterns, which would not create such sharp effects. To mirror the main variation, we residualize outcomes of cohort fixed effects (but no other controls). This allows us to provide greater transparency into the underlying data; while avoiding conflation with cohort trends.

[16] This recentering avoids confounding treatment effects with differences in calendar years or ages. We also trim endpoints of the data to balance age within recentered cohorts (e.g., trim the first six months in the birth records— Jan.-June 2007— because we only observe birthdays to the right of the cutoff for that recentered year.

Second, we test for balance in women's fertility and covariates around the cutoff. To measure fertility *rates*, for each value of the running variable, we divide the number of mothers by the number of females born in California on that date. Because fertility responses may vary by 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 results for two dimensions of fertility: the likelihood of having a birth and maternal age. Across the two samples (Panels A and B), we find no significant effects of the cutoff on the likelihood of having any birth, a first birth, or second birth or on average age at any, first, or second birth. By race (Appendix Tables A.3 and A.4), 47 of 48 estimates are null; the only marginally significant effect is on 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 1); on the distribution of maternal ages (Appendix Figure A.2); and demographic characteristics (see Appendix Tables A.5 and A.6). 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.1 reports summary statistics for this analysis sample, which consists of 347,541 mothers in the birth records sample and 290,000 mothers in the earnings panel.

5 Results

5.1 Effects on Completed Education

Figure 2 plots binned averages of years of education for women born within 100 days of the cutoff, with separate markers for the birth records (dark gray) and earnings (light gray) samples. Panels A–C show all women, early-career women (ages 16-24) and mid-career women (ages 25-40). Appendix Tables A.7 and A.8 report the regression estimates.

Women born just before the cutoff consistently have more schooling, with discrete jumps at the

cutoff at every career stage. In early career, the increase is 0.16 years in the birth records sample and 0.15 years in the earnings sample. By mid-career, the effect tapers to 0.05 and 0.06 years across samples, yet each of these is still statistically precise and evident in the figure. The nearly-identical estimates across the birth-records and earnings samples reflect their substantial overlap. One point worth noting is that these education estimates are specific to birthing mothers at each career stage; which corresponds well to infant outcomes, but may not be representative of the earnings panel (since impacts on education may vary with age at childbirth). When we adjust our estimates to produce the average effect across mothers (see Section 7.2), we obtain a slightly smaller, 0.11 year increase in early career, and a slightly larger, 0.08 year increase in mid-career.

Figure 3 shows that the sustained effects on schooling primarily occur on the "some college" margin. In early career, high school completion, some college, and college graduation all increase. By mid-career, only the effect on some college remains sizable, statistically significant across samples, and visibly apparent at the cutoff (Appendix Figure A.4). The estimates indicate that college attendance increases by between 1.2 p.p. (birth records sample) and 1.5 p.p. (earnings sample), or by about 2%. Further, because the effects remain at mid-career, they are likely permanent.[17] Effects on other margins are smaller and never significant in both the earnings and birth records samples.

5.2 Effects on Careers

We assess impacts on women's careers in two steps. First, using the earnings panel, we estimate effects on earnings and employment over the life cycle. Second, using the birth records, we examine how professions and job amenities evolve over time.

Earnings and employment Figure 4 shows career-average annual earnings (Panel A) and employment (Panel B) for women born around the cutoff. Similar to the effects on education, both measures increase sharply just before the cutoff, producing a visually striking gap in outcomes. Table 2 summarizes the effects. Earnings increase by $748 and employment rises by 0.7 p.p. annually

[17] We also find no effect on mid-career school attendance, implying that there is no ongoing catch-up in schooling (Appendix Table A.7).

(Columns 1-2 of Table 2). These changes correspond to a 4% increase in earnings and 1% increase in employment relative to baseline, indicating a moderate change even in the reduced form.

To study when these effects emerge, and whether they persist, we estimate age-specific effects of the cutoff (for ages 20-34). These age patterns can also be informative of the mechanisms. If early school entry primarily increases women's early career experience—by jump-starting careers— then earnings effects may be large in early career, but fade as experience effects diminish in later career. However, if greater education is a main mechanism, the rise in earnings would be more likely to persist or even grow over time as women enter more stable professional roles (e.g. Lagakos et al., 2018).

Panel A of Figure 5 presents age-specific impacts on earnings, along with 95% confidence intervals. Women born before the cutoff have higher earnings at every age, with increasing gains over the life-cycle. Impacts on earnings begin around \$600 in the early 20's, and increase moderately during the mid-to-late 20's, before stabilizing between \$750-\$800 around age 30.[18] On average, women born before the cutoff earn \$601 (6.9%) more in early career; which grows to \$783 (3.8%) by mid-career (Columns 3 and 5 of Table 2). This implies that women's earnings gains expand by 30% over the start of their careers. Earnings conditional on working rise to a similar degree, accounting for a large share of the overall effect (Appendix Table A.9).[19]

Effects on earnings also shift higher in the distribution over time. Appendix Figure A.6 shows that early effects are concentrated at or near the poverty threshold ($\approx$ \$25K annual income). By mid-career, gains are centered around \$40K—roughly median income for women (U.S. Census Bureau, 2025)—and are more diffuse, extending as high as \$100K. Impacts on being above the poverty line roughly double (1.4 vs. 0.7 p.p.) This suggests that women are more likely to reach financial stability by mid-career.

Panel B of Figure 5 shows similar increasing effects on employment over women's careers. While impacts on employment are near-zero at age 20, estimates rise to 0.5-0.8 p.p. by the early 30's.

[18] We find similar effects when we use a fully balanced panel (Appendix Figure A.5), although the confidence intervals are slightly wider due to the smaller sample.

[19] The most comparable estimate, Dobkin and Ferreira (2010), is a -0.5% (se: 1.5%) effect on wages at ages 30-39, pooling CA men and women; which is statistically distinguishable from our 2.8% (se: 0.6%) mid-career effect on conditional earnings at the 5% level. This difference could reflect our focus on mothers or the younger cohorts in our analysis.

Overall, women born before the cutoff are 0.7 p.p. (1%) more likely to be working in mid-career, with slightly smaller impacts at the start of careers (Columns 4 and 6 of Table 2).

Panel C of Figure 5 shows that effects on household income also grow with age—from $617 at age 20 to roughly $800-$1,100 in the early 30's. Notably, these estimates are very close to the individual earnings effects, implying little to no effect on spousal earnings (see Appendix Table A.9).

Pre vs. post motherhood To further study the persistence of labor market gains, we analyze whether effects change after motherhood—a critical career transition. Such changes could arise for several reasons: for example, one concern could be that gains would fade after childbirth because marginal jobs are not compatible with children. But this is just one mechanism: gains could also increase if mothers can better afford child care, or remain unchanged if the cutoff does not affect career responses to motherhood. We test for these competing possibilities by comparing impacts in the years around a first childbirth—the largest shock to women's careers (Kleven et al., 2019).

Figure 6 shows RD effects on earnings and employment for the three years before and four years after a first childbirth using a balanced panel (shown in the black filled markers). We find stable effects on both outcomes, with no statistically significant shift after childbirth: earnings are consistently about $750 higher in each year, and employment shows the same flat pattern. Further, year-of-childbirth effects in the full sample are similar in magnitude, but slightly larger and more precisely estimated (as shown in the grey unfilled markers).[20] This means that mothers born before the cutoff have greater financial resources during pregnancy and into early childhood.[21]

Professions and job amenities To complement the evidence of earnings gains, we next examine whether there are accompanying shifts in women's job amenities—and whether these effects reinforce or detract from wage improvements. We group non-wage amenities into three dimensions: (i) social status; (ii) orientation towards public service; and (iii) working conditions—which capture both private and social aspects of work, and may affect children's in-utero environment.

[20] We focus on the year of childbirth because (i) it maximizes the sample of mothers and (ii) using a single event year avoids having to censor the data to keep the composition of mothers stable across estimates.

[21] This pattern also implies that the cutoff does not change the "motherhood penalty" (Appendix Figure A.7), consistent with the observationally similar earnings drops at childbirth across education groups (Kleven, 2022).

Because these results rely on the birth records, we begin by estimating baseline effects on employment, which we use as a benchmark for impacts on professions. Figure 7 studies impacts on employment in the past year (shown in Panel A) and identifying as a worker (shown in Panel B), pooling all ages.[22] We find sharp increases in both measures: on average, employment rises by 1.1-1.2 p.p., with similar effects in early- and mid-career (Table 3). These effects are slightly larger than in the earnings panel, suggesting stronger impacts for younger cohorts. Reassuringly, we find similar, or slightly larger effects for this sample in the tax records (Appendix Table A.10).

Social status We first analyze effects on women's social status and prestige. Greater status has intrinsic value because it raises both self image and social image, leading to greater satisfaction (Bottan et al., 2025; Georgellis et al., 2022); but it may also generate tangible benefits, like better health care (Schwab and Singh, 2024). We use three proxies for status (as discussed in Section 2): (i) having a professional job; (ii) having a job that requires post-secondary schooling and (iii) occupational prestige ratings. The first two proxies are unconditional, to avoid selecting on employment; prestige ratings are only measured for women with an occupation.

We find positive effects on all three measures, which grow larger across career stages. Panel A of Figure 8 shows a clear mid-career increase in being a professional, which amounts to a 1.3 p.p. (3.6%) higher share of women in these roles (Column 1 of Table 4). Relative to the overall increase in jobs (1.3 p.p.), this implies that the increase in professional jobs can account for essentially all of the overall rise in work. In contrast, the figure and regression estimates show no effect in earlier career, indicating that jobs improve along this dimension over time—mirroring the strengthening effects on earnings.

Impacts are similarly meaningful on other status proxies. By mid-career, women born just before the cutoff are 0.8 p.p. more likely to have a job that requires some college, which can explain about 62% of marginal jobs (with confidence intervals that extend up to 100%) (Column 2 of Table 4). Occupational prestige also increases by around 0.27 points (roughly 1%), reflecting shifts into jobs in the top half of the prestige distribution (Columns 3–4 of Table 4). Panel B of Figure 8 shows

[22] While related, these measures contain independent signals about work: 40% of non-working women identify as a worker, and 20% of working women identify as stay-at-home mothers.

that prestige jumps exactly at the cutoff.

Social impact Second, we study effects on the social meaning of jobs. For women—who place especially high value on this amenity—moving from a job with few opportunities to make a positive social impact to one with many is worth about a 7.7% wage increase (Kesternich and De Schouwer, 2023). We measure the overall effect on social meaning using average worker ratings of having a frequent positive societal impact in their job (Maestas et al., 2023). Next, to provide transparency into these effects, we identify pre-school educators as a "high social impact" profession that may be relevant for marginal students. In particular, pre-school educators have the highest impact rating among jobs that do not require a college diploma—matching the margin of education effects at mid-career. We thus test for effects on binary indicators for being a pre-school teacher, defined narrowly as reporting a pre-school educator (or closely related) position, or broadly to include unspecified teachers.[23]

Across both measures of social impact, we find broad increases in meaningful work, particularly later in careers. At mid-career, women born before the cutoff are 0.7 p.p. (1.3%) more likely to have a job that workers perceive as having a positive social impact (Appendix Figure A.8). This is at least partly due to greater work in early-childhood education: the likelihood of being a pre-school teacher rises by 0.2 p.p. (50%) under the narrow definition and 0.8 p.p. (13%) under the broad definition (Columns 5–6 of Table 4).[24][25]

Working conditions Finally, we examine impacts on working conditions, including peers, work schedule, physical demands, and team work. Overall, we find the strongest shifts in mid-career peer quality and colleague interactions. In particular, women born before the cutoff are more likely to have co-workers who attended college (Column 7 of Table 4) and to hold jobs that involve more team-work (Appendix Figure A.8). We find smaller and less precise effects on other working

[23] Using the American Working Conditions Survey (Maestas et al., 2017), we calculate that over 90% of pre-school teachers think they have a positive impact most/all of the time.

[24] The rise in educators occurs precisely at the cutoff (Appendix Figure A.9).

[25] Notably, educator positions often include other desirable amenities—like breaks during children's holidays and a high share of mothers as co-workers (Price and Wasserman, forthcoming)— which could also influence children's birth outcomes.

conditions—such as paid vacation days, being involved in less-physically-strenuous tasks, the ability to telecommute, and access to training opportunities—though the estimates are typically positive.

At the same time, we don't find evidence of a more stressful work environment or reductions in work-life balance. The pace of work is unchanged, indicating that jobs are not more demanding. We also do not find changes in typical weekly hours of work or weeks of work (Appendix Table A.11). In terms of work flexibility, we find no impact on ability to set one's own schedule, although the share of typical hours worked outside of 8 a.m. and 5 p.m. decreases very slightly (by 0.2%)—possibly reflecting the fixed daytime schedules of educators.

Together, these results show that career shifts extend beyond earnings to non-wage amenities—including status, social meaning, and working conditions—and that these effects increase over a career. We show in Section 6 that these effects can not be explained by changes in worker selection resulting from increases in women's employment (i.e., they reflect true upgrades). We quantify the monetary value of amenity improvements in Section 7.2.

5.3 Effects on Infant Health and Prenatal Investments

Shifting from maternal careers to child spillovers, we analyze effects on infant health and prenatal investments, and whether the timing of these aligns with increasing professional advances over time. Career and child effects should move together if professional advances spur prenatal investments—implying *larger* child effects at mid-career—and should move inversely if they hinder investments—implying *smaller* child effects at mid-career. To test this, we continue to analyze outcomes separately in early and mid-career; Table 5 presents our regression estimates.

Panel A of Figure 9 shows binned scatters of log birth weight for children of mothers born around the cutoff. At mid-career (subfigure ii), children to the left of the cutoff clearly weigh more: the estimates shift discretely upward precisely at the threshold. In line with this, columns 1–2 of Table 5 show that mid-career births are 0.4 percent, or about 9 grams, heavier. In contrast, we do not find any effect in early career (subfigure i): birth outcomes are continuous across the cutoff, and the point estimate is roughly zero and insignificant. Thus, impacts on child health appear to move in parallel with career gains.

To understand the source of these weight gains, we measure mid-career impacts across the birth weight distribution. Appendix Figure A.10 shows that the largest effects are on children reaching or slightly exceeding median child weight (3,200–3,500 grams), with smaller and less significant effects at the upper and lower tails. Children are less likely to have very low birth weights (1,500–2,500 grams), indicative of a decline in poor infant health; but these effects are relatively minor compared to the improvements at higher weights. We do not find increases in at-risk higher-weight births (e.g., greater than $4,000$ grams), consistent with weight gains reflecting better infant health. Overall, these patterns suggest that children's growth reflects incremental changes in weight that accumulate across the distribution. This is consistent with the broad improvements from other recent maternal interventions, such as greater access to insurance and moves to better neighborhoods (Miller et al., 2022; Chyn and Shenhav, 2025).

Accompanying the rise in child weight, we also find suggestive increases in gestational age that increase over time. Column 3 of Table 5 shows that children born to mid-career mothers to the left of the cutoff are born 0.45 days later ($\approx 0.2\%; p < 0.10$), indicating that longer gestation may contribute to larger infant size. Further supporting this interpretation, these results become more precise when we expand the sample (by including higher parity births) in Section 6. As with birth weight, there is no effect on gestational age in early career.

As potential channels for the improvement in child health, we examine maternal health investments, including prenatal smoking and medical visits, and financial resources (proxied by use of public assistance). The most striking result is a large reduction in maternal smoking at mid-career: women born before the cutoff are 0.5 p.p. less likely to smoke during pregnancy (Column 4 of Table 5)—a 12.5 percent reduction. This does not appear to reflect greater monitoring by medical providers, as we find no change in the quantity of prenatal visits (column 5 of Table 5). However, other aspects of medical care—such as provider quality or relationships—may improve.

We also find a decline in participation in means-tested public programs at mid-career, indicating that women are more likely to meet basic needs independently. This includes a 0.7 p.p. (2%) decrease in use of nutritional assistance (WIC) and a 1.1 p.p. (3.6%) reduction in use of public health insurance (Medi-Cal). Given the decline in women with earnings below or near the poverty

line, this likely reflects greater self-sufficiency and, in turn, lower program eligibility (Appendix Figure A.6).

Importantly, there is a clear pattern of increasing maternal inputs over the life cycle, in step with gains in maternal careers and infants' health. This is consistent with professional advances facilitating greater infant investments: as careers improve, poverty declines, health inputs expand, and infant health follows. While we cannot fully rule out other possibilities, this throughline is supported by research showing a direct link from maternal income and employment to the same prenatal inputs improved in our setting (e.g., smoking) and birth weight (Hoynes et al., 2015). The close timing of the reduction in poverty and mothers' decline in use of public programs also aligns with this explanation. In contrast, greater education alone could not explain these increasing infant effects, since the gap in schooling is declining over time.

6 Robustness

We conduct a battery of robustness exercises that vary our methods and sample, and test for spurious effects. For brevity, we focus on the mid-career effects, where our patterns are strongest; but we have similarly validated the (largely null) early-career results.

First, we assess the stability of our estimates across a range of bandwidths and inference methods (Appendix Figure A.11 and Appendix Table A.12). Our results are similar across a wide span of pre-selected bandwidths (45–80 days) or when we use the mean-squared-error optimal bandwidth (Calonico et al., 2014). We also arrive at similar levels of statistical significance when we use 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), or bias-corrected estimates and confidence intervals (Calonico et al., 2014).

Second, we check whether our results hold when we broaden the sample (Appendix Figures A.12–A.13; and Appendix Table A.12). Adding Black women slightly attenuates the education effect, but has little impact on other outcomes. Including older first-time mothers also does not meaningfully change our findings (although effects are slightly larger for younger first-time mothers); nor does changing the age window we use for mid-career.

Notably, when we include third births, our infant health results become more precise. Panel B of Figure 9 shows that the mid-career discontinuity in log birth weight becomes visually sharper, consistent with the larger sample (50% more children), although the estimated coefficient is essentially the same (Appendix Table A.13). Likewise, we obtain tighter confidence intervals around the gestational age effect (now significant at the 5% level), and more generally for other outcomes.

Third, we evaluate whether our results could have arisen by chance by re-estimating our effects using placebo cutoffs. Specifically, we assign pseudo-cutoffs at two-week intervals around the true cutoff (8 total), and use those to estimate Equation 1. If our effects reflect noise or seasonality in outcomes, we would expect this to yield similarly large and precise estimates. Instead, of the 48 placebo estimates, only two are statistically significant, and both have the opposite sign of the main estimates (Appendix Figure A.14). The rest are typically small, inconsistently-signed, and uncorrelated across outcomes, making it highly unlikely that our findings reflect statistical noise.

Fourth, we use Lee (2009) bounds to gauge the role of selection into work for our estimated effects on job amenities. This bounding is necessary because we find effects on the extensive margin of employment, which may alter observed amenities through changes in who works and, in turn, the composition of the workforce. The results show that composition changes cannot explain our effects: we obtain tight bounds around our main estimates (Appendix Table A.14).

Finally, we limit our analysis to a balanced panel of mothers (two observations per mother) to assess the role of changing composition of mothers in our results.[26] Our results are very similar to baseline: we treatment effects are positive, and increase when the *same* mother is older. This suggests that life-cycle effects, not selection, contribute to the strengthening effects over time (Appendix Table A.15).

7 Establishing the Role of Education and Quantifying its Returns

To move from the reduced-form effects to broader implications, we now return to examine the mechanisms behind the lasting gains for women and children, particularly the role of schooling. We

[26] This 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.

then use this evidence to contextualize our results and provide quantitative estimates of policy-relevant parameters.

7.1 The Role of Education for Labor Market Gains

As discussed in Section 1.2, early school entry could affect careers because women complete *more* education or because they finish schooling *sooner*. To separate these channels, we now implement our proposed empirical test from Section 1.2.1. Specifically, we compare the effects on jobs that require exactly *some college*—the education margin shifted by the cutoff—versus those on jobs that require a *college degree*. We also study impacts on partner selection, as a potential mediator of these effects.

The credentialing patterns strongly match the predicted effects of education. We reported earlier (in Section 5.2) that jobs requiring exactly some college can account for the majority of the new jobs at the cutoff: 62%, based on the mean effect, with confidence intervals that include shares as high as 100%. In contrast, Appendix Table A.16 shows that we do not find any effect on jobs that require more schooling (a BA) or less (an exam). Moreover, the null effects on college jobs are precisely estimated: we can rule out even a 0.2 p.p. increase in degreed jobs—equivalent to 16% of the total increase in jobs—with 95 percent confidence. We also find a precise zero effect on subsets of degreed jobs that may be of greater interest to mothers, such as teaching positions. These sharp credentialing patterns are difficult to reconcile with younger age-for-grade, which would be expected to have a more uniform effect across job skill levels.

While we don't observe the exact institution that marginal students attend, our estimated rise in pre-school educators together with the impacts on some college suggest that many enroll in community colleges. Specifically, pre-school teachers with some college are disproportionately likely to study fields that are commonly acquired in two-year (not four-year) programs (Kim et al., 2022). This pattern suggests that at least part of the relevant postsecondary investment may occur in two-year programs. To the extent that community college enrollment is typical, the associated schooling costs (e.g., tuition and foregone earnings) are likely to be relatively low, so the subsequent earnings gains can be interpreted largely as a net increase in resources.

We find little effect on partner selection, ruling this out as a likely mechanism. Women born before the cutoff are not more likely to have a partner nor to have a more "successful" partner (based on education, employment, or professional status) (Appendix Table A.17). The only significant impact is a small increase in partner age, consistent with school-cohort matching. However, this cannot have generated the main career results. The relatively minor effects on partner selection are consistent with evidence that women with some college tend to choose partners from the same pool as lower-educated women (Lundberg et al., 2016).

7.2 Implied Return to Education

If we interpret these tests as evidence that all of our effects are due to the impacts on education, we can use our estimates to obtain two key inputs for education policy: (i) the return to additional schooling and (ii) the sources of these benefits. We perform this analysis in three steps. First, we update our mid-career effect on education to make it more representative of the average effect. Second, we calculate the implied return to a year of schooling and the relative contribution of amenity improvements.[27] Third, we discuss sources of earnings gains and improvements in infant health. Throughout, we focus on mid-career effects to capture the longer-run benefits of schooling.

Average education effects To calculate the labor market return, we need the mid-career effect on education for the earnings sample—not just for birthing mothers. Appendix Figure A.15 shows that: (i) the mid-career education sample underrepresents women with younger first births (ages 16-20 or 21-25) vs. older first births (ages 26-30); and (ii) the RD effect on education is larger for women with earlier births. To adjust for this discrepancy in shares, we reweight birth-age-specific treatment effects by the overall sample shares. For the two younger groups, we proxy for the mid-career effect using the treatment effect at second birth (the latest observation); for the older group, we use the average effect. This reweighting exercise produces an implied 0.084 increase in years of education at mid-career (vs. 0.060 at baseline).[28]

[27] This calculation relies on the standard assumption that career outcomes vary linearly with years of education.

[28] We also reweight the estimates to obtain a more representative early career estimate using a similar procedure (using the treatment effect at first birth for the two older groups, and the average effect for the younger group). This produces an implied 0.11 increase in years of education in early career.

Labor market return Using this average education effect, we calculate the labor market return by rescaling the cutoff's impact on career outcomes by its impact on years of schooling. This yields that each year of additional schooling increases women's likelihood of working by 8.3 p.p. ($\frac{0.7 \text{ p.p.}}{0.084 \text{ years}}$), total earnings by 45.2% ($\frac{3.8\%}{0.084 \text{ years}}$), and earnings conditional on working ("wages") by 33.3% ($\frac{2.8\%}{0.084 \text{ years}}$). For comparison, the wage return for attending community college ranges from 10% per year for a typical student, to 45% per year for higher-demand credentials (Stevens et al., 2019; Grosz, 2020; Minaya and Scott-Clayton, 2022). Our estimates fall in the range of these prior returns.

To incorporate amenity improvements, we use women's WTP for a subset (twelve) of the job amenities that we study to convert changes in job characteristics to the equivalent wages increases (Maestas et al., 2023). This allows us to quantify the welfare gains from changes in these amenities, and to compare this with the rise in earnings. The WTP estimates are mainly available for measures of job content and the work environment (e.g., social impact, teamwork), and not for other job features where we find large effects, such as social status and peer quality. To the extent that these job aspects outside the monetized set are desirable (e.g., Bursztyn et al., 2017; Bottan et al., 2025; Jencks et al., 1988), our valuation provides a loose lower bound on the total amenity value of work.

Figure 10 shows that the value of better working conditions is equivalent to a 3.3% rise in annual wages ($p = 0.06$) per year of education, adding 10% on top of the direct wage returns. This primarily reflects gains from more meaningful work (equivalent to a 0.5% wage increase), less physical strain (equivalent to a 1.9% wage increase), more teamwork with performance-based pay (equivalent to a 1.2% wage increase), and more paid time off (equivalent to a 0.6% wage increase) partly offset by the disutility of less independent work (equivalent to a 1.2% wage decrease). The contribution of each of these factors varies, but no single element provides outsize value; suggesting that workers benefits arise from the totality of the improvements in jobs.

Sources of earnings gains To explain the mid-career increase in earnings, we separate this effect into across-occupation upgrading and higher within-occupation pay. Using reported occupations at childbirth, we estimate that occupation shifts can account for $533 of the increase in earnings based on differences in average pay (see Appendix Table A.11). Thus, across-occupation shifts can

explain 88% of the $603 increase at childbirth, with the remaining 12% reflecting higher pay *within* occupations. The high explanatory power of occupation-level pay also suggests that women's pay is close to the average, indicating that average job characteristics may also be a reasonable proxy for job amenities.

Infant health return Performing the same rescaling exercise for infant health, we find that birth weight increases by roughly 5% or 136 grams per year of education using our more conservative estimates (including third births). The size of this effect is consistent with Currie and Moretti (2003), who estimate a 5% reduction in low birth weight per year of education (largely from greater attendance of four-year-colleges), and is roughly twice the birth weight impact of gaining access to Medicaid during pregnancy among immigrants (Miller et al., 2022).

In addition to these sizable improvements in early life, past work based on twin comparisons suggests that these impacts on infant health translate into meaningfully better adult outcomes, including in completed education, health, and earnings (Royer, 2009; Figlio et al., 2014; Black et al., 2007; Bharadwaj et al., 2017). For example, our 5 percent increase in child weight could be expected to raise full-time annual earnings in adulthood by 0.5 percent (Black et al., 2007). While a full accounting of these benefits is beyond the scope of this paper, our results suggest that increased prenatal investments may help explain the positive link between maternal education and children's future economic prospects.

Sources of birth weight gains We benchmark the role of different mechanisms in the rise in child weight using past estimates of their efficacy. Overall, we find that the changes in prenatal investments that we document can explain at least 53% of the birth-weight effects. Higher earnings during pregnancy alone can explain 3.9 grams (43%) based on a 6.4 gram effect per $1,000 of income (Hoynes et al., 2015). Reduced smoking can explain another 0.9 grams (10%), 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 pre-pregnancy (12) (Permutt and Hebel, 1989). The remaining 47% of the effect may in part reflect earnings gains *prior* to pregnancy, which may also boost infant health (e.g., from improved housing or reduced exposure to environmental hazards), as well as unmeasured

changes in investments such as better nutrition.

8 Conclusion

This paper shows how education reshapes mothers' careers—both wages and non-wage amenities—and their children's outcomes over the life cycle. Using a regression discontinuity design leveraging the school birthdate cutoff, we show that women born just before the cutoff attend college at higher rates and, as a result, experience sizable career gains: higher employment and earnings, greater social status, more social impact, and better working conditions. These improvements are smaller early on, and expand at mid-career. Children's health and prenatal inputs improve in tandem with careers. Thus, women's college attendance yields large career returns—from wages and amenities—that strengthen over time and generate positive spillovers to children.

Our results have three new implications for the return to women's education across generations. First, standard wage-based metrics understate mothers' labor market returns to education, and potentially overlook mechanisms for child benefits. We show that amenity-based improvements contribute materially to the labor market value of schooling. We also show that amenity gains are broad and could generate child spillovers, such as through greater prestige, which can affect one's social image and subsequent care, or by having a more meaningful job, which may raise job satisfaction.

Second, although motherhood is a well-documented career hurdle, education-induced labor market gains do not fade after childbirth. This suggests that more-educated mothers have greater financial resources not only during pregnancy, but also for greater investments during a child's critical first years of development.

Third, we find that maternal labor market gains and child outcomes move in parallel, rather than trading off, and that greater prenatal investments may mediate this positive link. Thus, sustained improvements in women's careers appear to catalyze—rather than hinder—infant investments. Importantly, these gains reflect not only higher employment and income but also meaningfully *better* jobs—an often-missing distinction in prior work. In turn, occupational upgrading—not just employment or income per se—may contribute to how gains from maternal education transmit to

their children. We leave this possibility for further investigation in future work.

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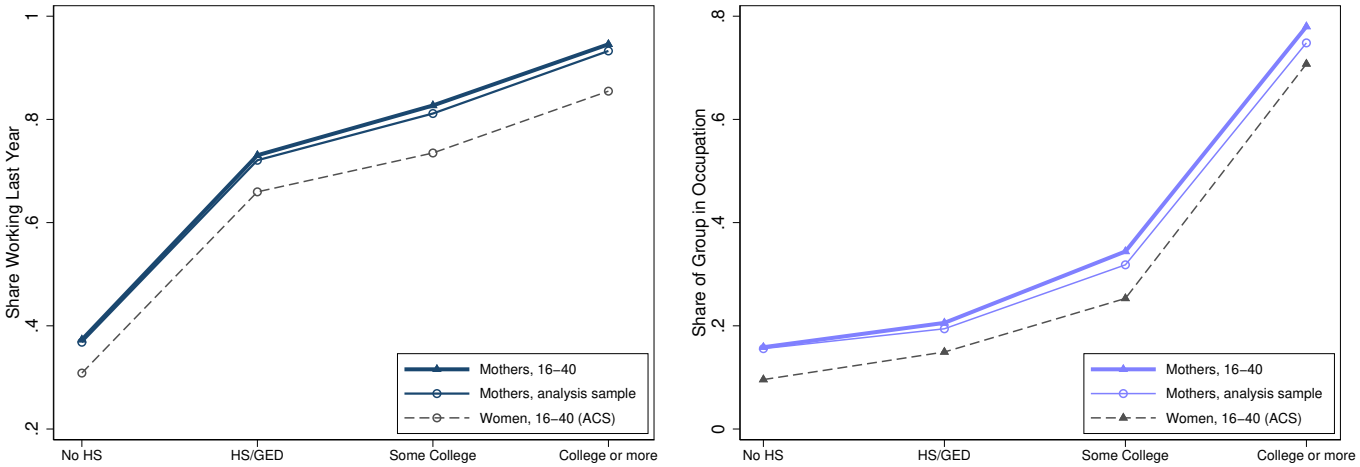
Figure 1: Descriptive Links between Women's Education, Careers, and Child Health

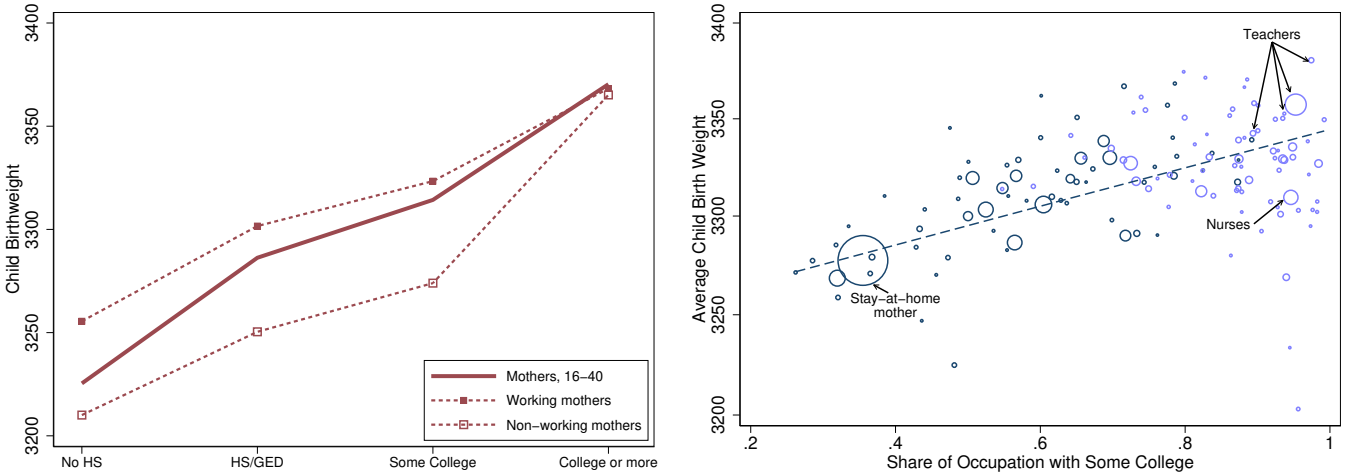
(a) Worked last year

(b) Professional jobs



(c) Child birth weight by maternal employment

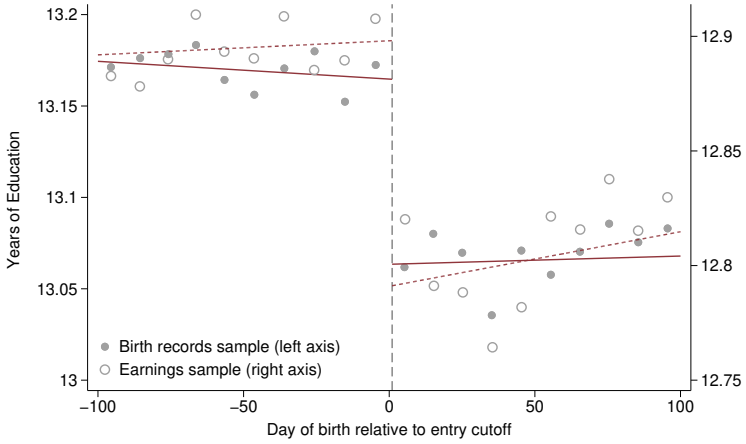
(d) Child birth weight by maternal occupation



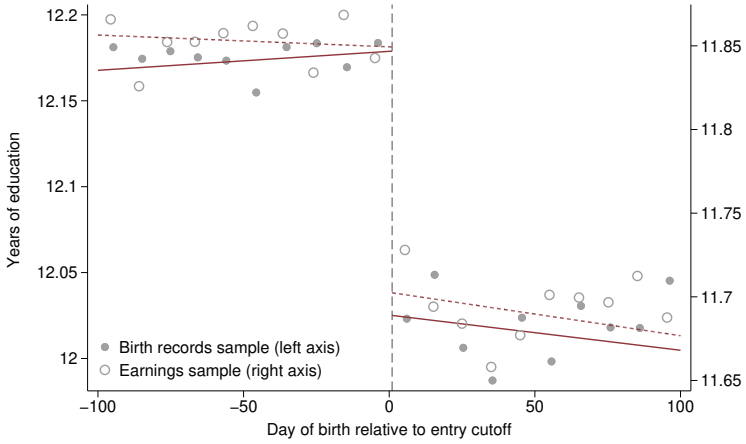
Notes: This figure presents the average labor market and child outcomes by women's (or for child outcomes, mother's) completed level of education: no high school diploma (No HS), high school diploma or GED only (HS/GED), some college attendance only, college degree or higher. Panel A shows the share of women who worked in the last year; Panel B shows the share employed 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 with light blue markers; non-professional occupations are shown with black markers. We display statistics for three samples: (i) all mothers between the ages of 16–40 with a first or second birth in California between 2007–2017 (in the markers labeled "Mothers, 16-40"); (ii) the subset of mothers who also meet the analysis sample criteria (in the markers labeled "Mothers, analysis sample"); and (iii) all women in the 2007–2017 ACS surveys, mothers and non-mothers, between the ages of 16-40 (in the markers labeled "Women, 16-40 (ACS)"). Panels A–B include samples (i)–(iii); while Panels C–D focus on sample (i).

Figure 2: Increase in Completed Education at School Entry Cutoff

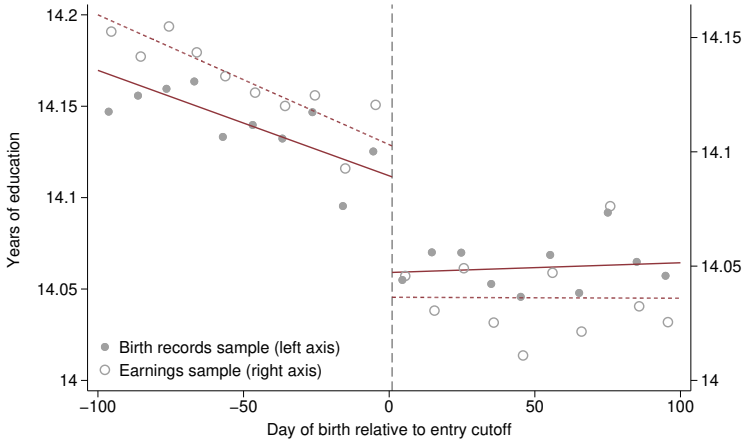
(a) All



(b) Younger than 25



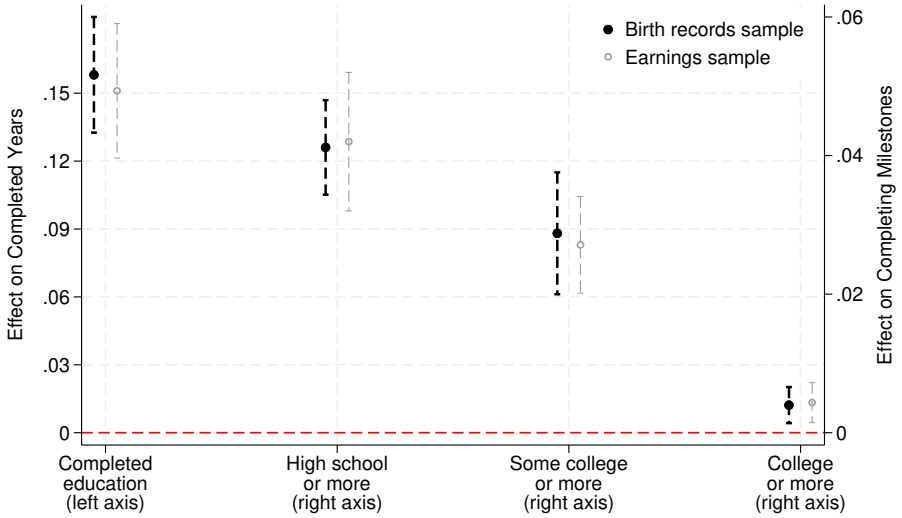
(c) Age 25 or older



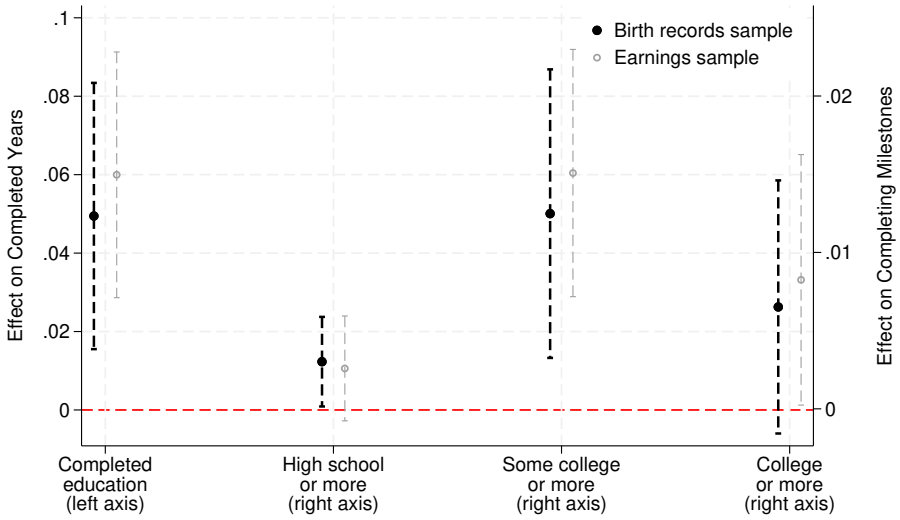
Notes: This figure presents the average years of education for women born within 100 days of the school entry cutoff. Panel A includes all women; Panel B includes early-career women; and Panel C includes mid-career women. Filled markers represent outcomes for the birth records sample, which are shown on the left axis; unfilled markers represent outcomes for the earnings sample (recreated in the birth records), which are shown on the right axis. Outcomes are residualized with (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. Earnings sample includes women born between 1977–1987, who satisfy other requirements for the birth records sample. 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

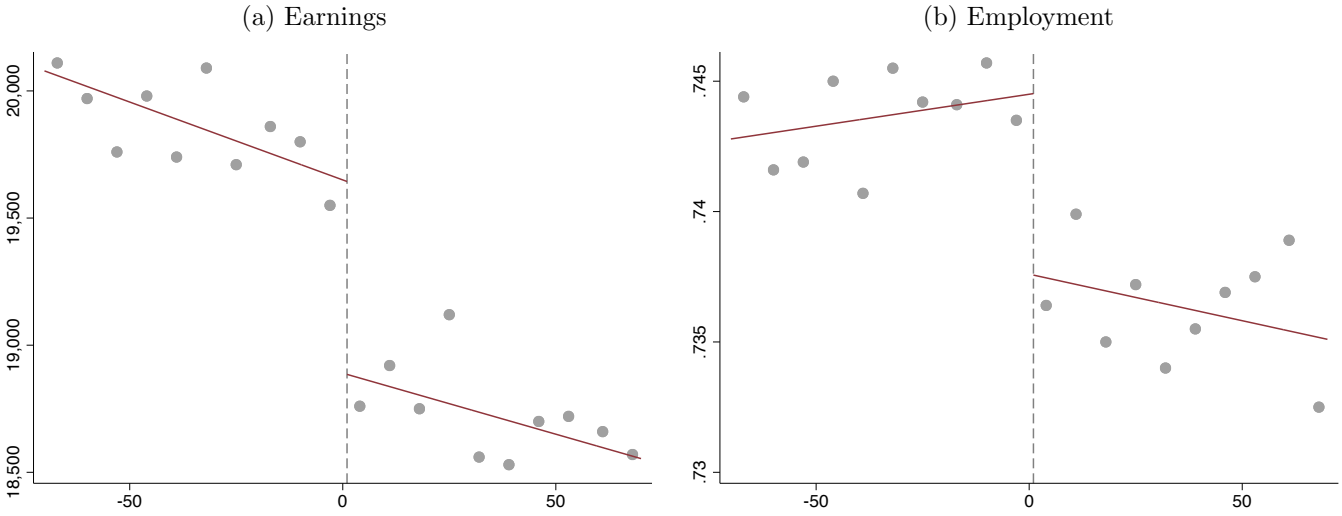


(b) Age 25 or older



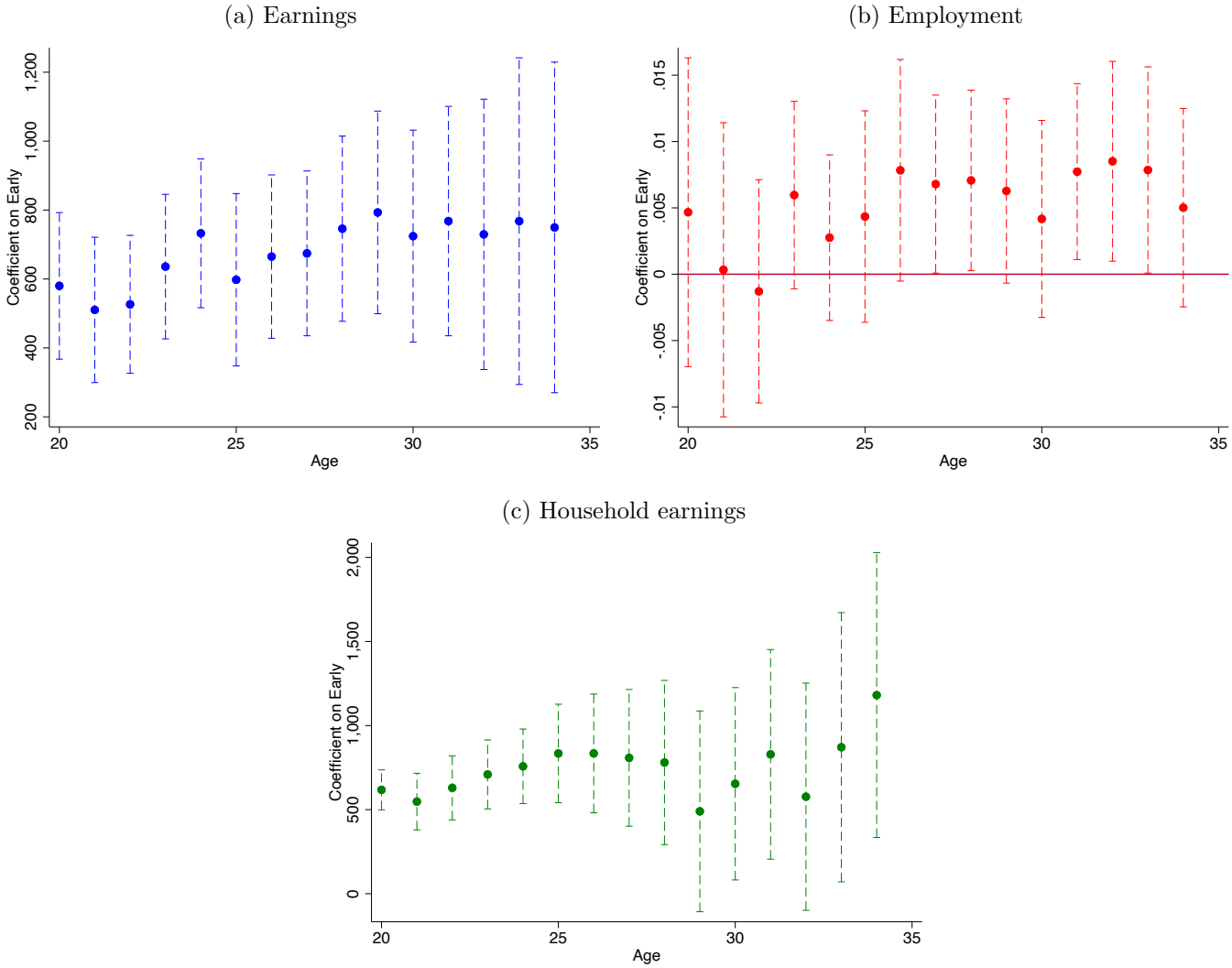
Notes: This figure presents the estimated effect (along with 95% confidence intervals) of being born just before the school entry cutoff on completed years of education, having a high school degree, completing some college or more, and completing college or more. Panel A includes early-career women and Panel B includes mid-career women. Filled markers show estimates for the birth records sample; unfilled markers show estimates for the earnings sample recreated in the birth records. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered by 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 Earnings and Employment (Earnings Panel)



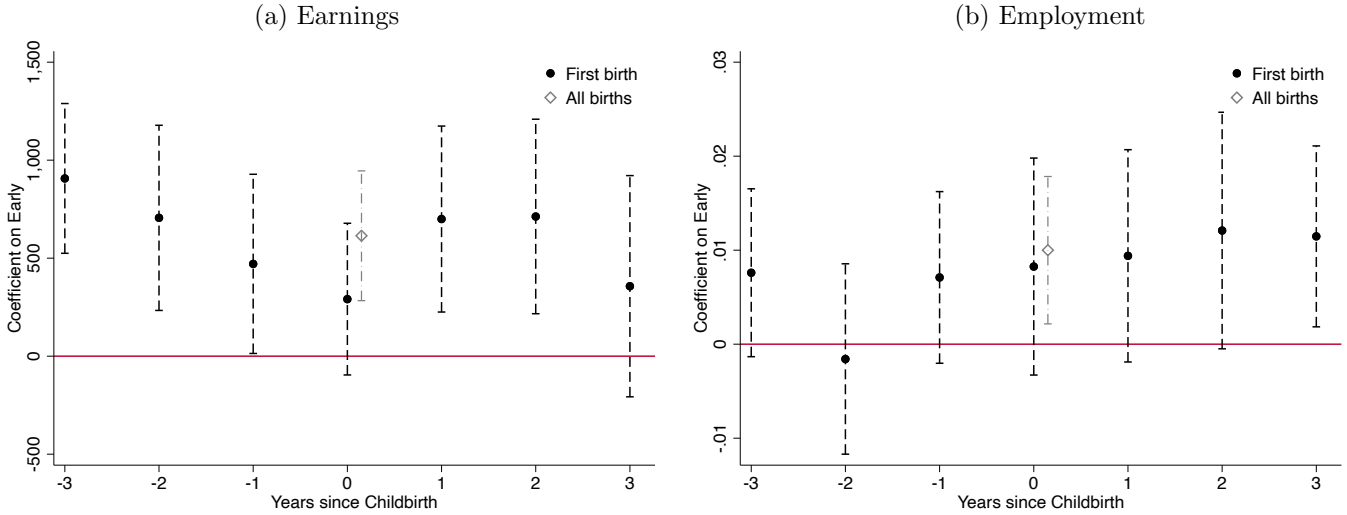
Notes: This figure presents average earnings (panel A) and employment rates (panel B) for women born around the school entry cutoff. Outcomes are residualized with (recentered) year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on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

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



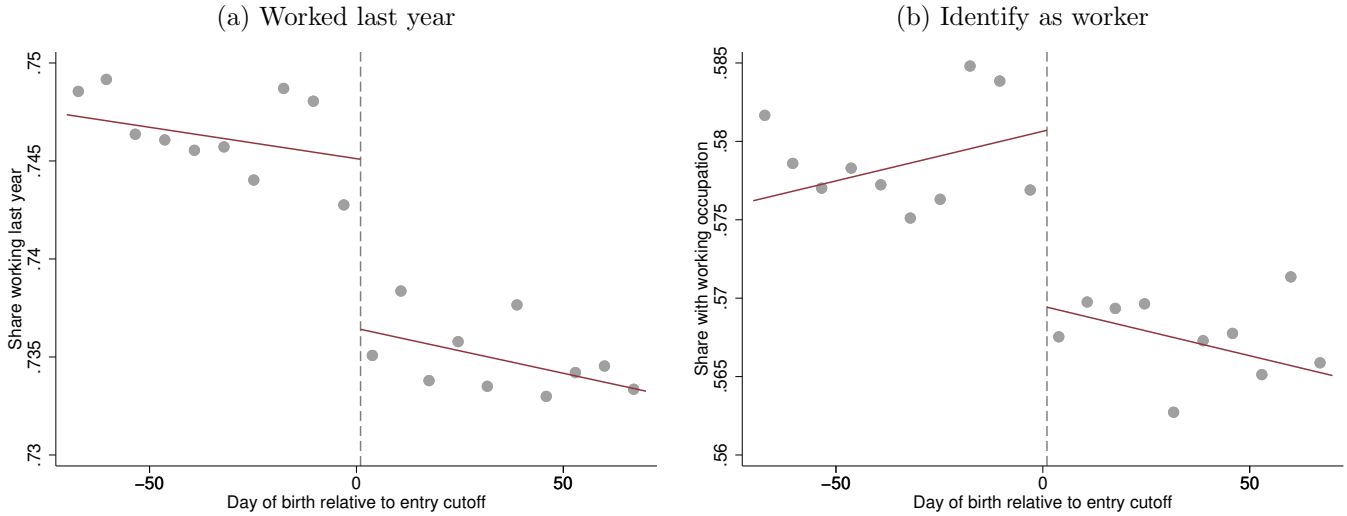
Notes: This figure presents the estimated effect (with 95% confidence intervals) of being born just before the school entry cutoff on average earnings (Panel A), the likelihood of working (Panel B), and household earnings (Panel C) at each age between 20–34. Earnings panel includes women born between 1977–1987, who satisfy other sample requirements. Estimates are weighted using a triangular kernel with a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: Linked IRS–Census administrative earnings records. See text for details.

Figure 6: Discontinuity in Earnings and Employment around Childbirth (Earnings Panel)



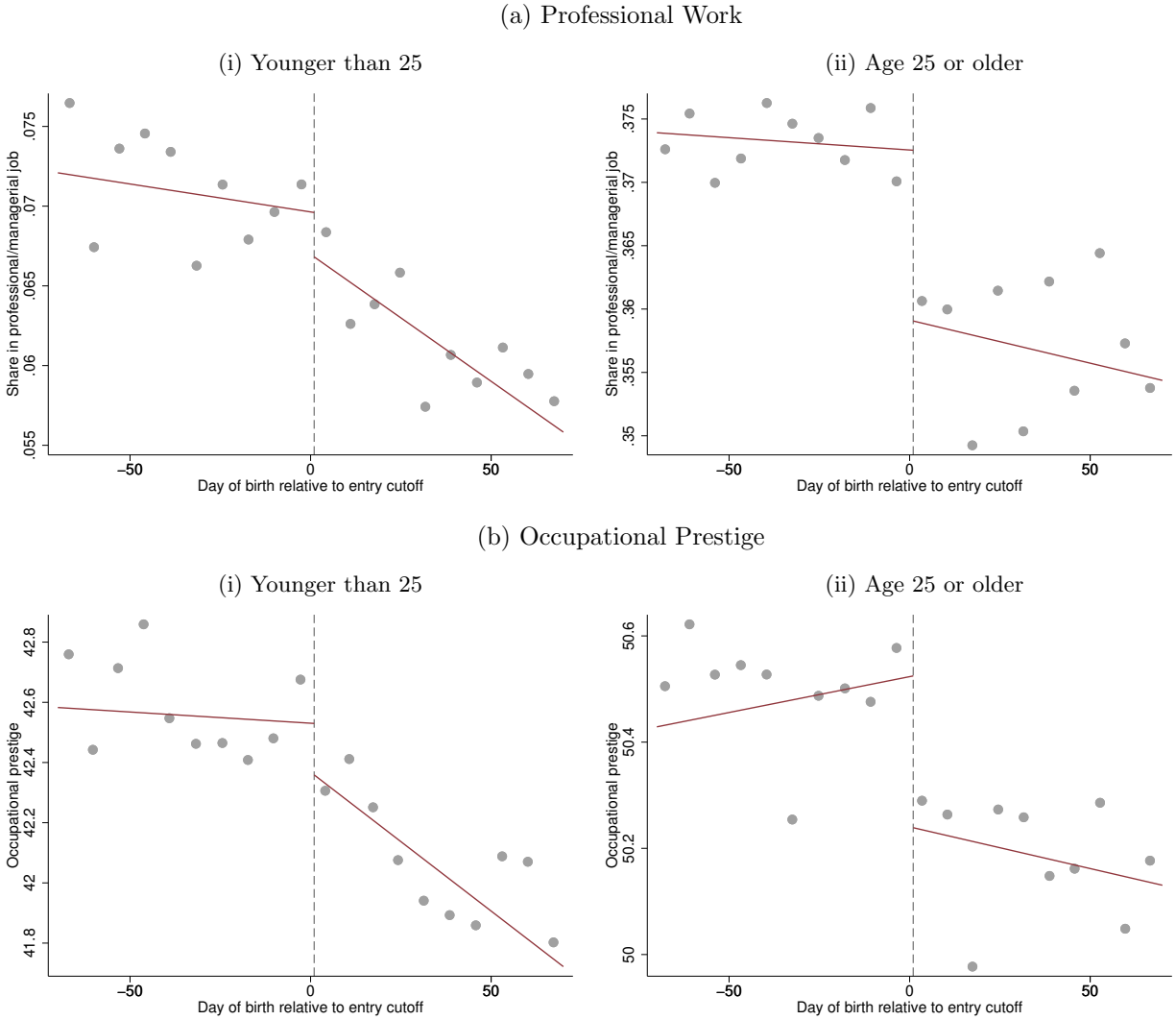
Notes: This figure presents the estimated effect (with 95% confidence intervals) of being born just before the school entry cutoff on women's earnings (Panel A) and employment rates (Panel B) around the birth of a child. The black filled markers show the estimates for a first childbirth using a balanced panel. The grey unfilled markers show the estimates for all childbirths using the full earnings panel. Each estimate shown comes from a separate regression. The earnings panel includes women ages 16–40, born between 1977–1987, who satisfy other sample requirements. Estimates are weighted using a triangular kernel with a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: Linked IRS–Census administrative earnings records. See text for details.

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 with 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 Prestige at Mid-Career (Birth Records Sample)

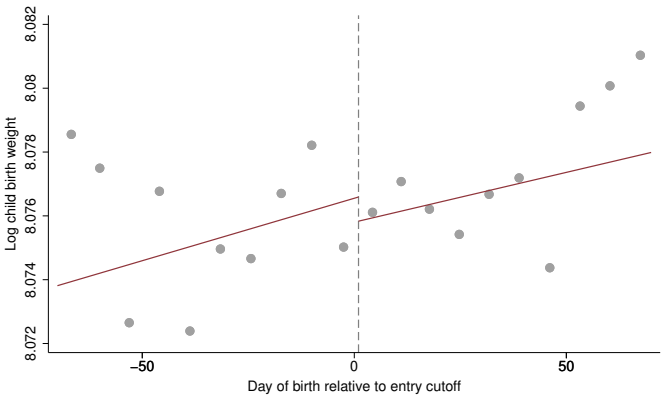


Notes: This figure presents the likelihood of having a professional occupation (Panel A) and average occupational prestige (Panel B) for women born around the school entry cutoff. Subpanel (i) shows outcomes in early-career and subpanel (ii) shows outcomes in mid-career. Outcomes are residualized with 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.

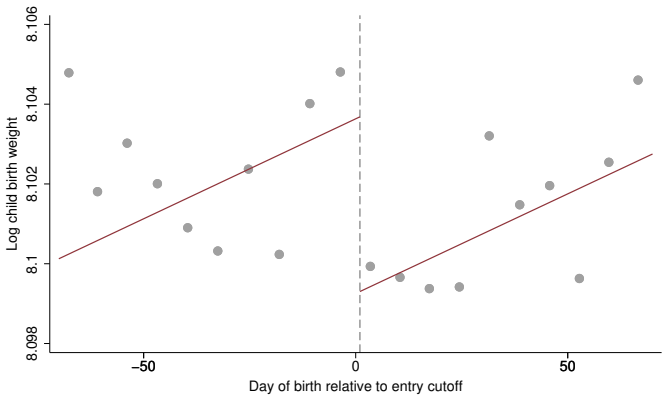
Figure 9: Intergenerational Effects on Infant Health Grow over Time (Birth Records Sample)

(a) Main sample

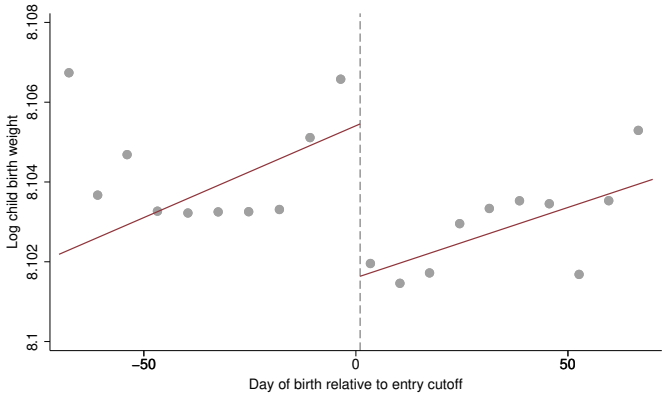
(i) Younger than 25



(ii) Age 25 or older

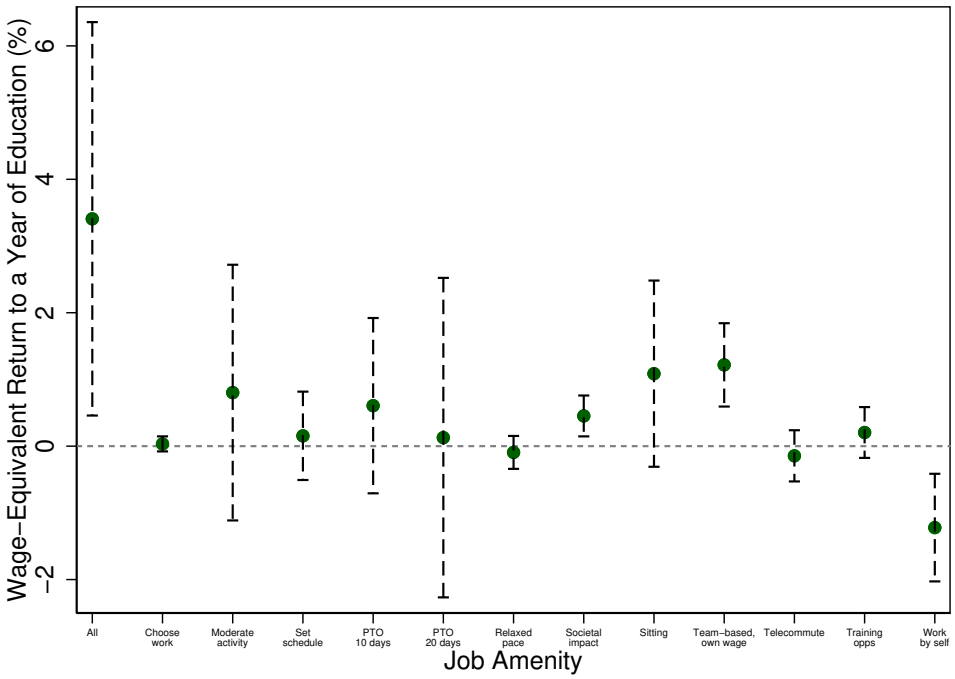


(b) Add third births; age 25 or older



Notes: This figure presents the average birth weight for mothers born around the school entry cutoff. Panel A shows results for the main sample: subfigure (i) includes mothers in early career; subfigure (ii) includes mothers in mid-career. Panel B shows results for mothers in mid-career when we expand the sample to include third births. Outcomes are residualized with 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 B2. 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.7) to obtain the wage-equivalent return to education from each amenity (and standard error). The estimate labeled "all" represents the sum across all twelve amenities. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered by day of birth relative to the school entry cutoff. Source: California birth records 2007–2017.

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	
	Earnings	Employment	Earnings	Employment	Earnings	Employment
Born before cutoff	747.8***	0.007**	600.8***	0.006**	782.8***	0.007**
	(160.4)	(0.003)	(82.8)	(0.003)	(187.5)	(0.003)
Mean above cutoff	18,780	0.737	8,755	0.793	20,850	0.725
Observations	4,667,000	4,667,000	798,000	798,000	3,870,000	3,870,000

Notes: This table presents the estimated effect of being born just before the school entry cutoff on women's earnings and employment. 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). Values are rounded in accordance with Census disclosure standards. Estimates are weighted using a triangular kernel with a bandwidth of 70 days around the cutoff. Standard errors are clustered on day of birth relative to the school entry cutoff. Source: Linked IRS–Census administrative earnings records. See text for details.

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). Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered by day of birth relative to the school entry cutoff. Source: California birth records 2007–2017.

Table 4: Discontinuity in Social Status, Social Impact, and Working Conditions Over a Career (Birth Records Sample)

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

Notes: This table presents the estimated effect of being born prior to the school entry cutoff on measures of job characteristics: social status (columns 1–4); social impact (columns 5–6); and co-worker quality (column 7). Panel A includes women in early-career (ages 16–24); Panel B includes women in mid-career (ages 25–40). Column 1 presents effects on holding a professional occupation; Column 2 on having a job that requires some college but no college degree; Column 3 on average occupational prestige; Column 4 on having above-median occupational prestige; Column 5 on being a pre-school or unspecified teacher; Column 6 on being a pre-school teacher; and Column 7 on the share of individuals in the occupation who have at least some college. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered by day of birth relative to the school entry cutoff. See Section 2 for information on the outcome construction. Source: California birth records 2007–2017.

Table 5: Discontinuity in Infant Outcomes and the Prenatal 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 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. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered by day of birth relative to the school entry cutoff. Source: California birth records 2007–2017.

ONLINE APPENDIX:

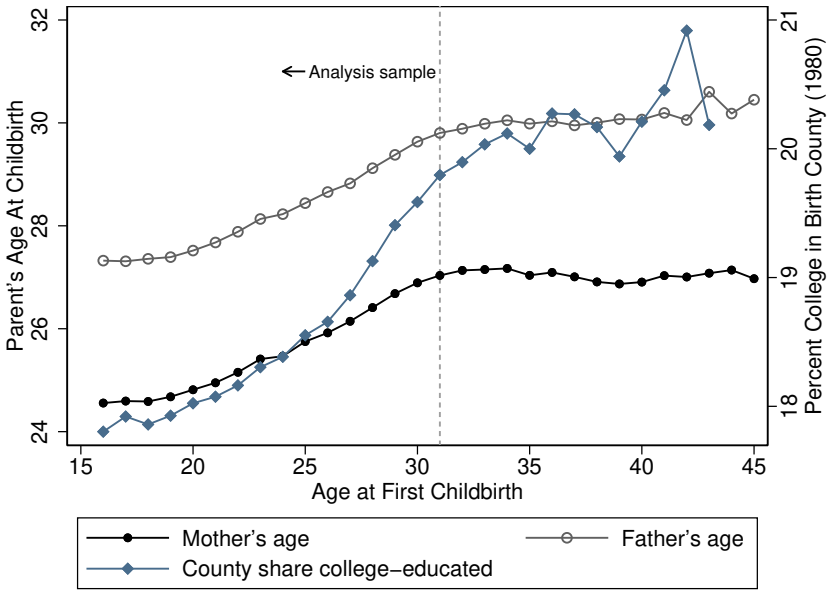
Multigenerational Benefits of Women's Education

Na'ama Shenhav and Danielle H. Sandler

January 21, 2026

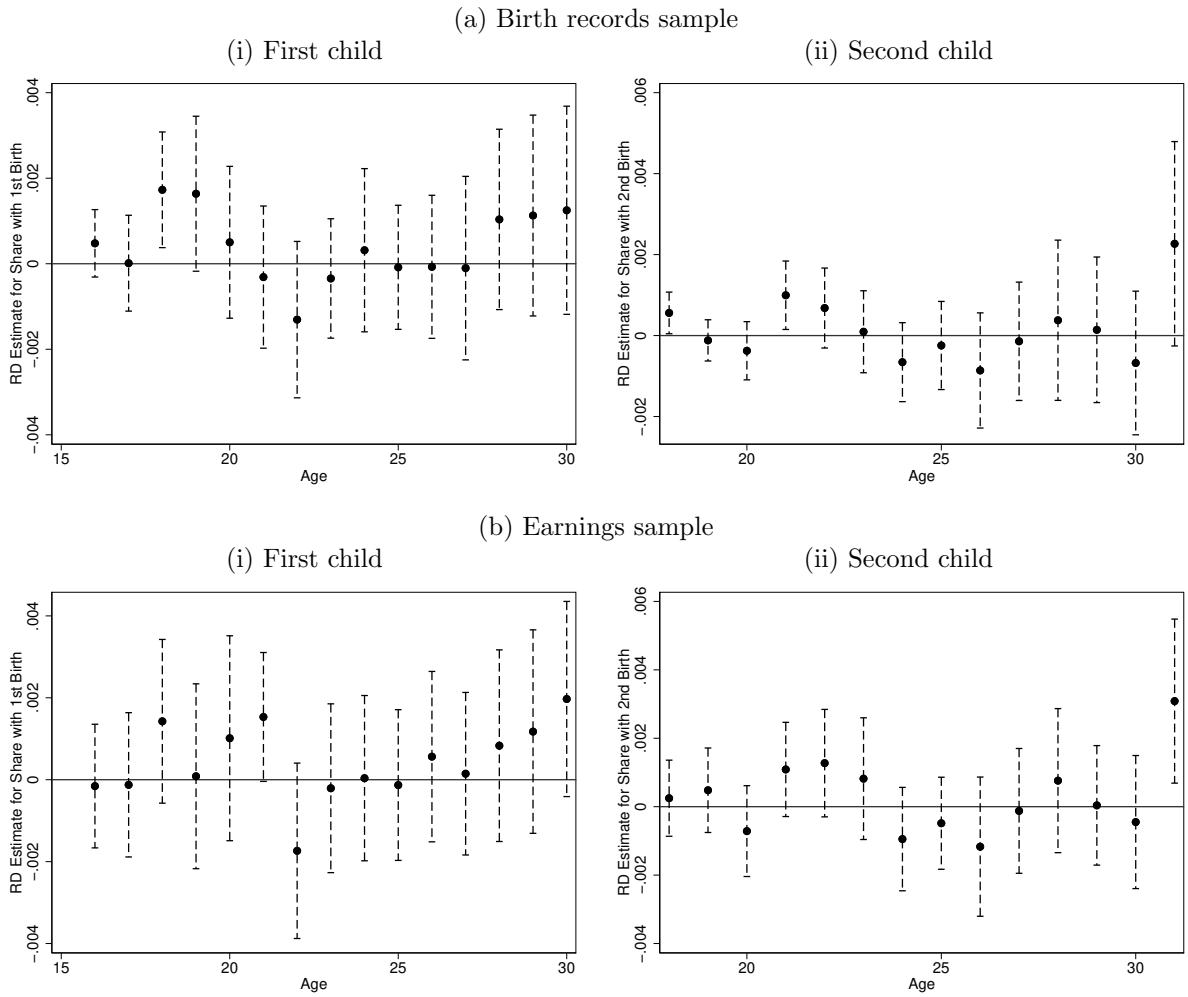
A Appendix Figures and Tables

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 and 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

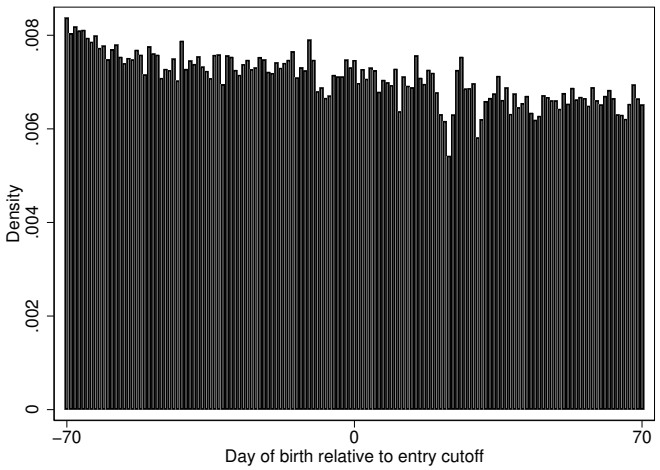


Notes: This figure presents the estimated effect (with 95% confidence intervals) of being born just before the school entry cutoff on having a first child (subfigure i) or second child (subfigure ii) at each age. Panel A shows results for the birth records sample; Panel B shows results for the earnings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

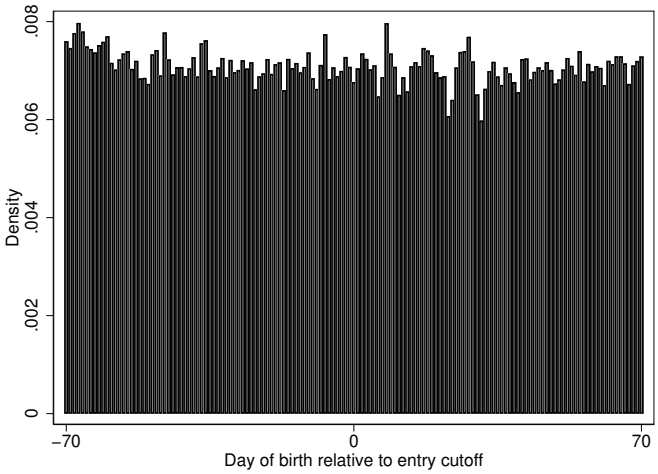
Figure A.3: Density of Birth Dates Around the Cutoff in Early- and Mid-Career

(a) Birth records sample

(i) Younger than 25

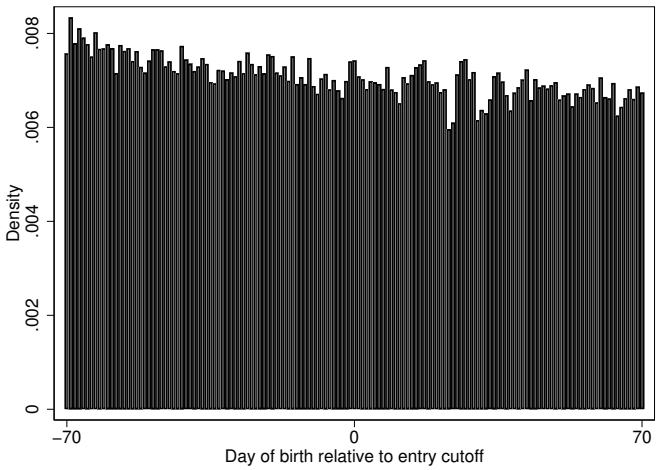


(ii) Age 25 or older

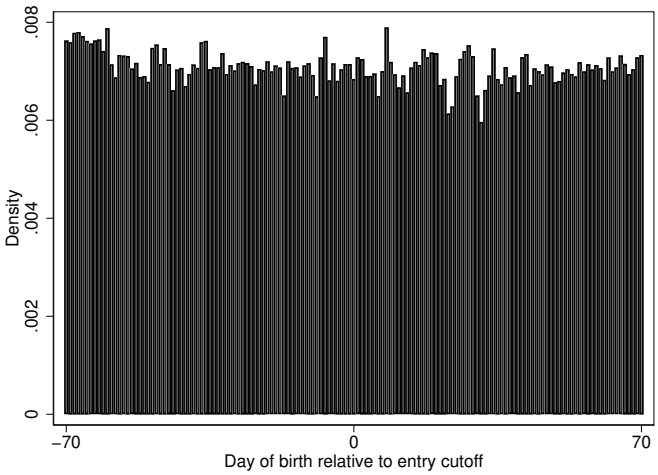


(b) Earnings sample

(i) Younger than 25



(ii) Age 25 or older

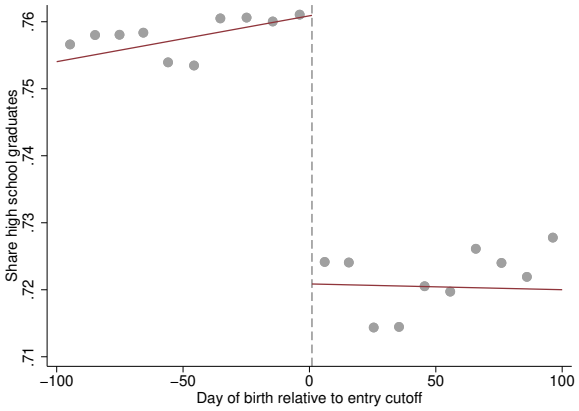


Notes: This figure presents the histogram of dates of birth around the school entry cutoff. Panel A shows results for the birth records sample; Panel B shows results for the earnings sample recreated in the birth records. Subfigure (i) includes women in early career (ages 16–24); and subfigure (ii) includes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

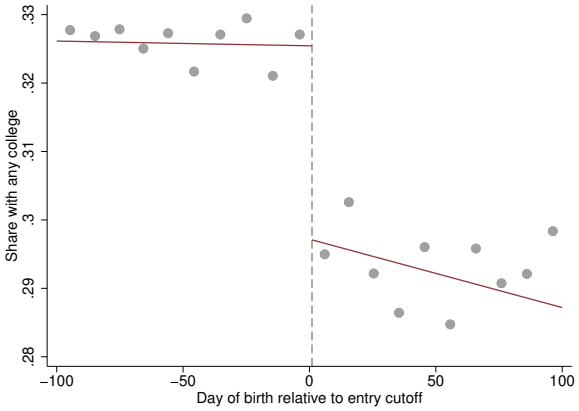
Figure A.4: Discontinuity in Secondary and Post-Secondary Education at School Entry Cutoff (Birth Records Sample)

(a) Younger than 25

(i) High school or more

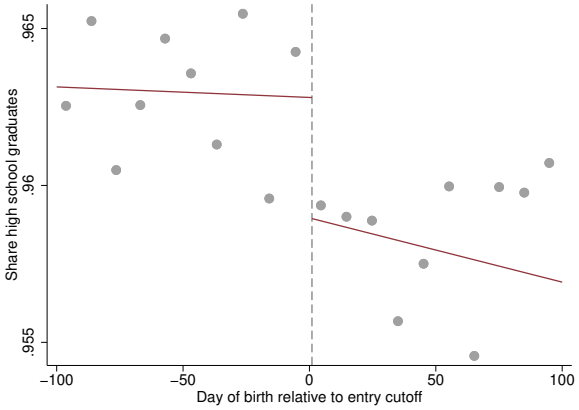


(ii) Some college or more

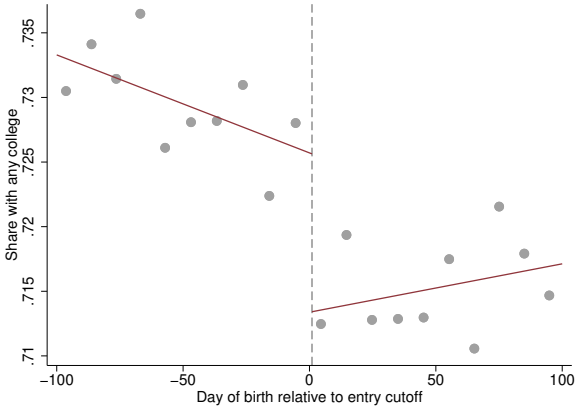


(b) Age 25 or older

(i) High school or more



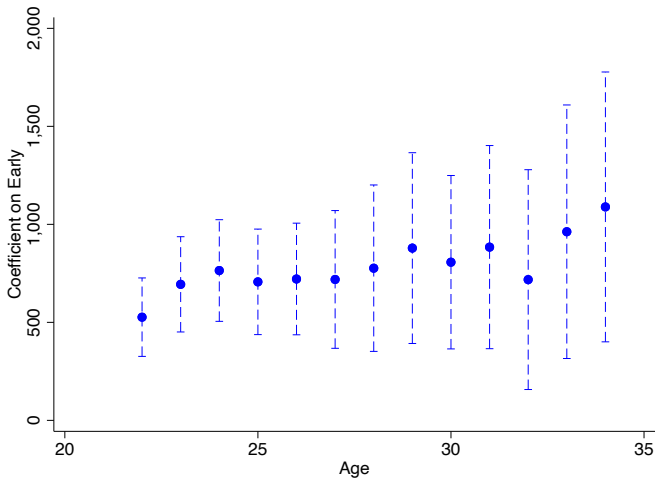
(ii) Some college or more



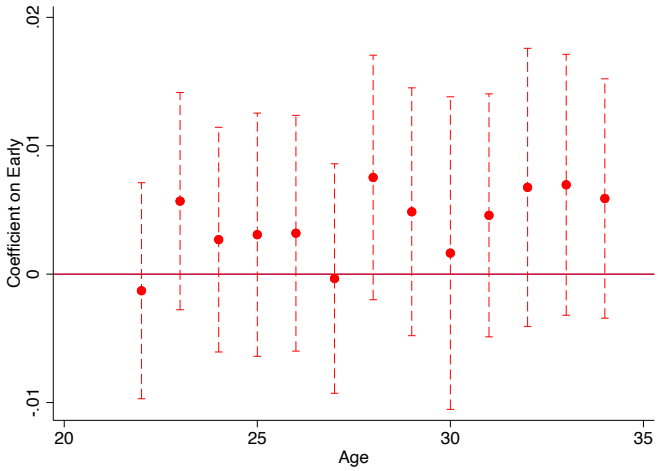
Notes: This figure presents the average secondary and post-secondary education outcomes for women born around the school entry cutoff. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Panel A shows outcomes for women in early-career (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: Life-Cycle Improvements in Earnings using Balanced Panel (Earnings Panel)

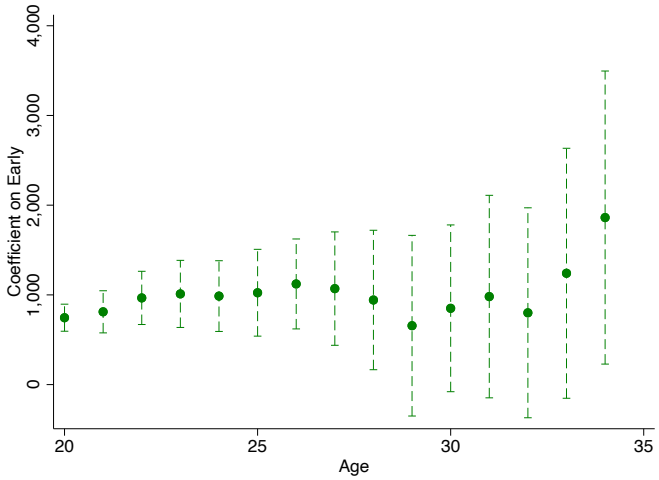
(a) Earnings



(b) Employment

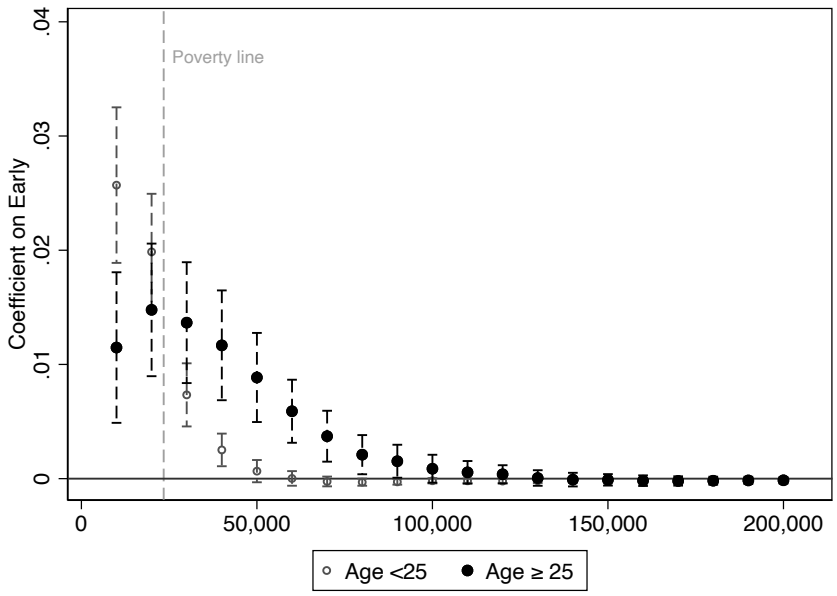


(c) Household earnings



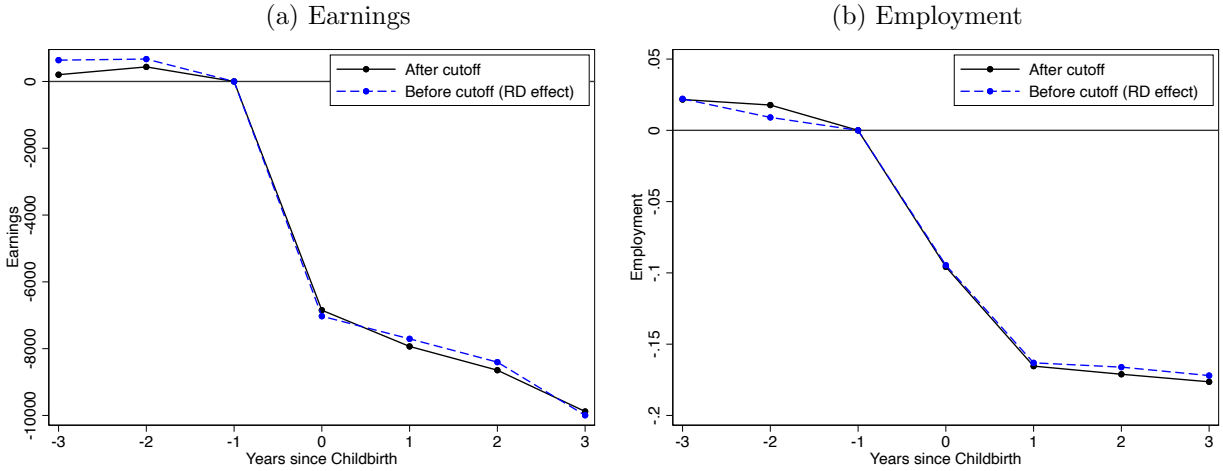
Notes: This figure presents the estimated effect (along with 95% confidence intervals) of being born just before the school entry cutoff on average earnings (Panel A), the likelihood of working (Panel B), and household earnings (Panel C). Sample includes women born between 1983–1987, and we estimate effects at each age between 20–34 for household earnings or at each age between 22-34 for other outcomes, which ensures that the panel is balanced across ages. 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: Linked IRS-Census administrative earnings records. See text for details.

Figure A.6: Discontinuity in Distribution of Earnings in Early- and Mid-Career (Earnings Sample)



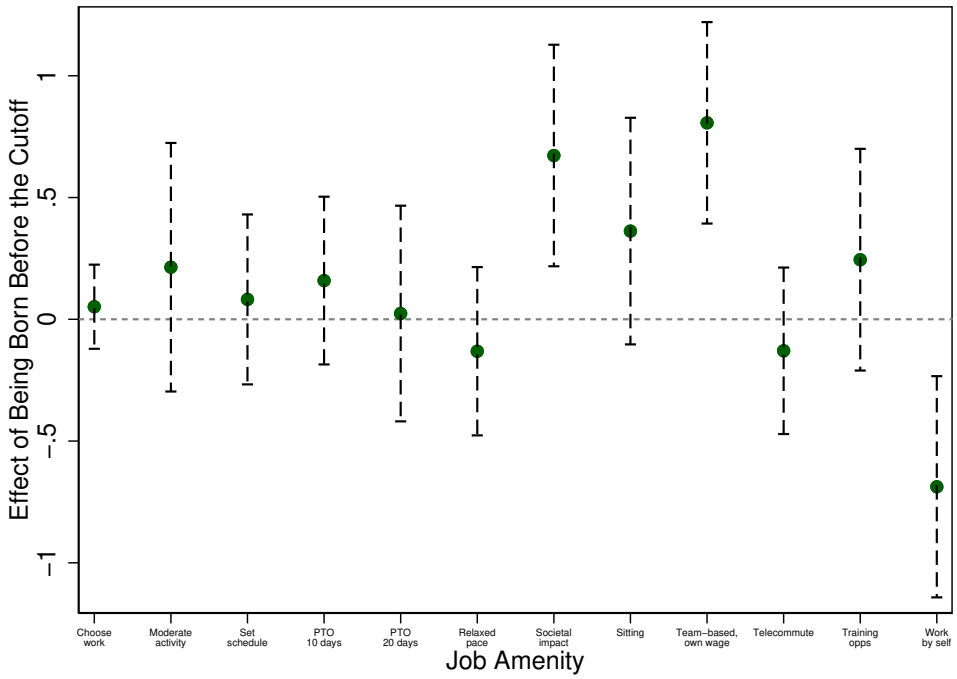
Notes: This figure presents the estimated effect (and 95% confidence interval) of being born just before the school entry cutoff on the likelihood of having earnings greater than X, where X is a dollar amount shown on the x-axis. The empty gray markers show the estimates for early-career, and the filled black markers show the estimates in mid-career. The average poverty line at mid-career (calculated based on family size shares and 2016 poverty thresholds) is included for reference. See the notes of Figure 5 for details about the earnings sample and estimation.

Figure A.7: Child Penalty Among Women Born Around the Cutoff (Earnings Panel)



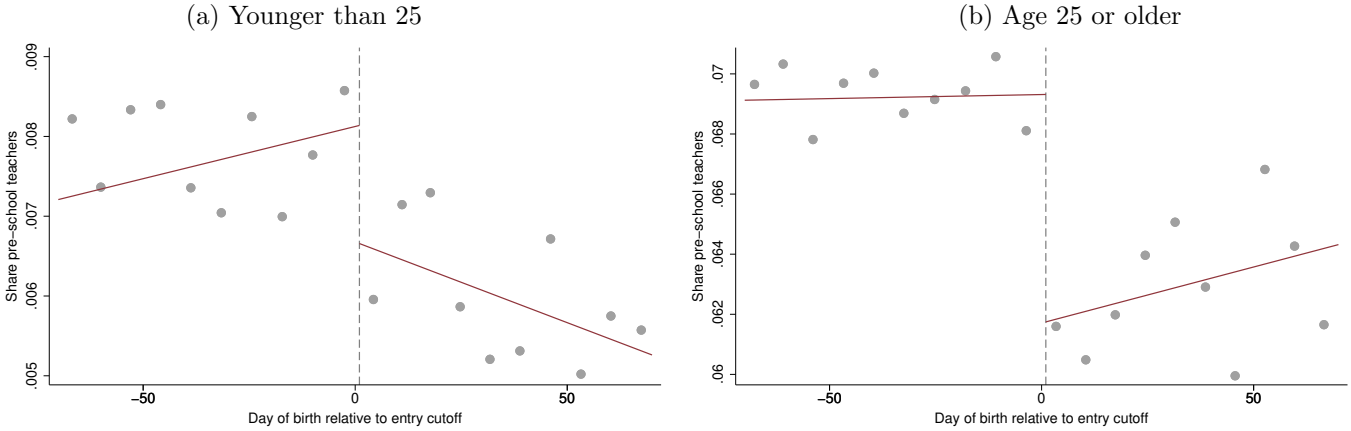
Notes: The solid black line in each figure presents the change in women's earnings (Panel A) or employment rates (Panel B) in each year around a first birth, relative to the year before birth, among women born after the cutoff. The dashed blue line shows the counterfactual change for women born after 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.8: Mid-Career Impact of the Cutoff on Working Conditions (Birth Records Sample)



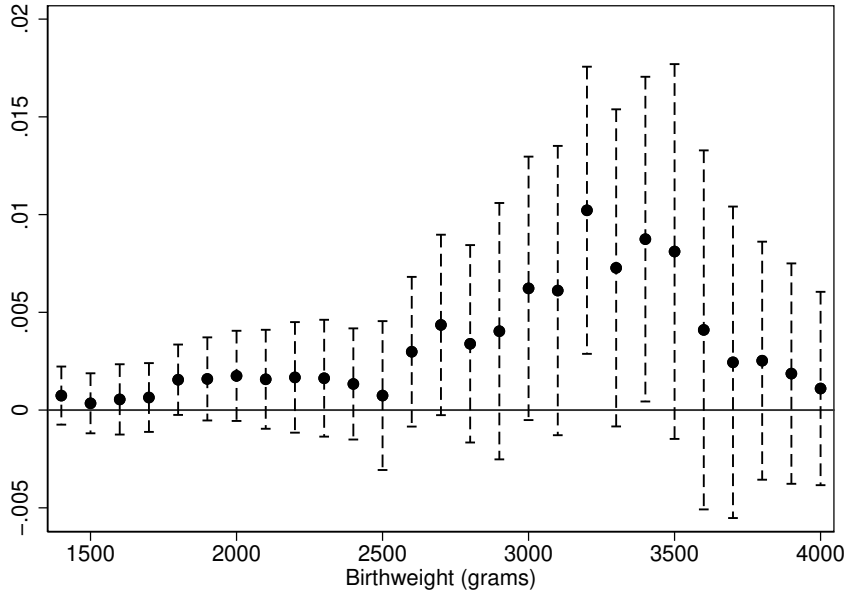
Notes: This figure presents the estimated effect (and 90% confidence interval) of being born just before the school entry cutoff at mid-career on job amenities. The outcomes are occupation-level average working conditions calculated from the American Working Conditions Survey (Maestas et al., 2017), described in Appendix Section B2. See the notes of Figure 3 for further details about the birth records sample and estimation.

Figure A.9: Discontinuity in Being a Pre-School Teacher (Birth Records Sample)



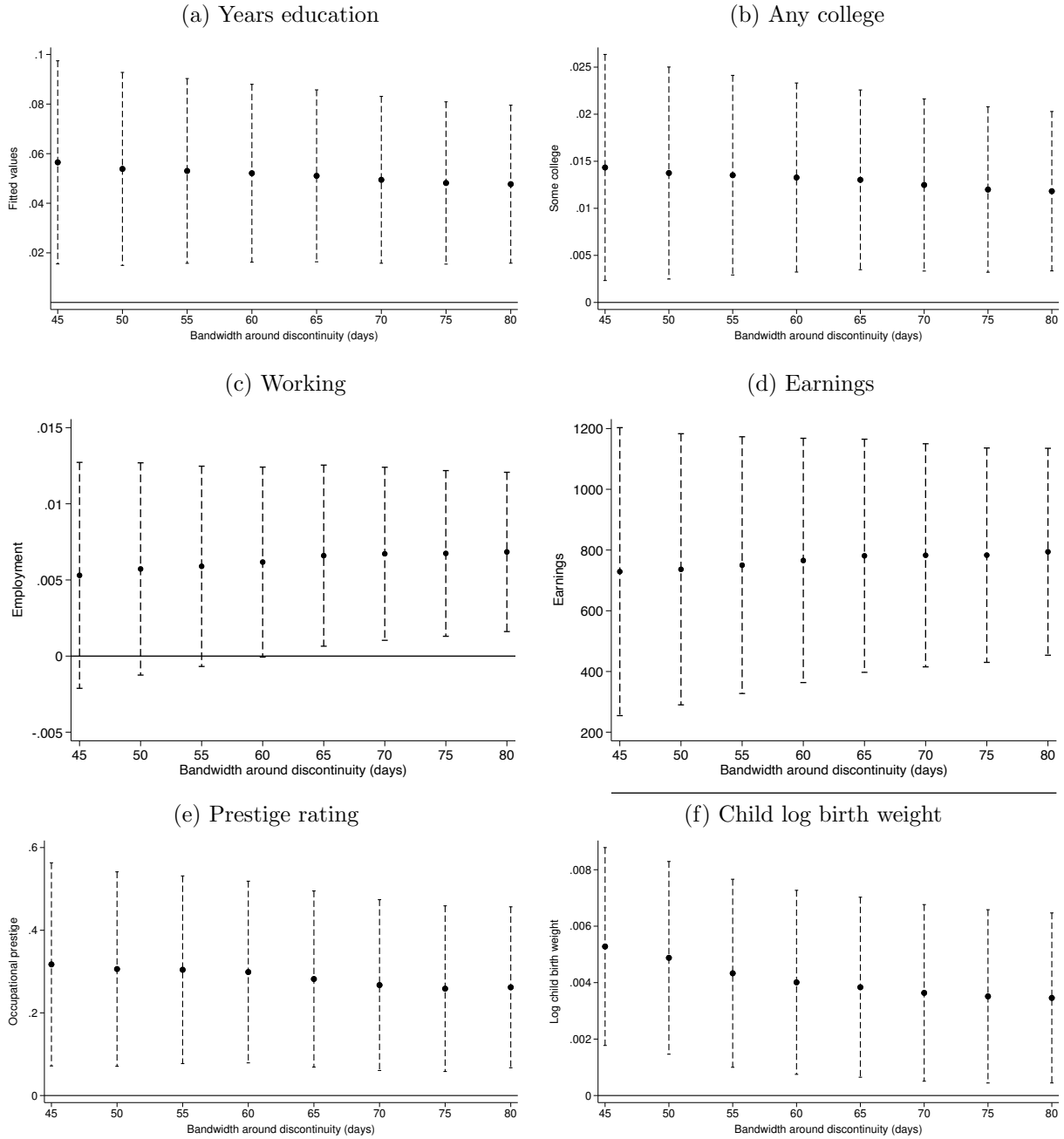
Notes: This figure presents the likelihood of being a pre-school teacher for women born around the school entry cutoff. Subpanel (a) shows outcomes in early-career and subpanel (b) shows outcomes in mid-career. Outcomes are residualized with recentered year of birth fixed effects. Average outcomes (shown 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 A.10: Discontinuity in Distribution of Birth Weight at Mid-Career (Birth Records Sample)



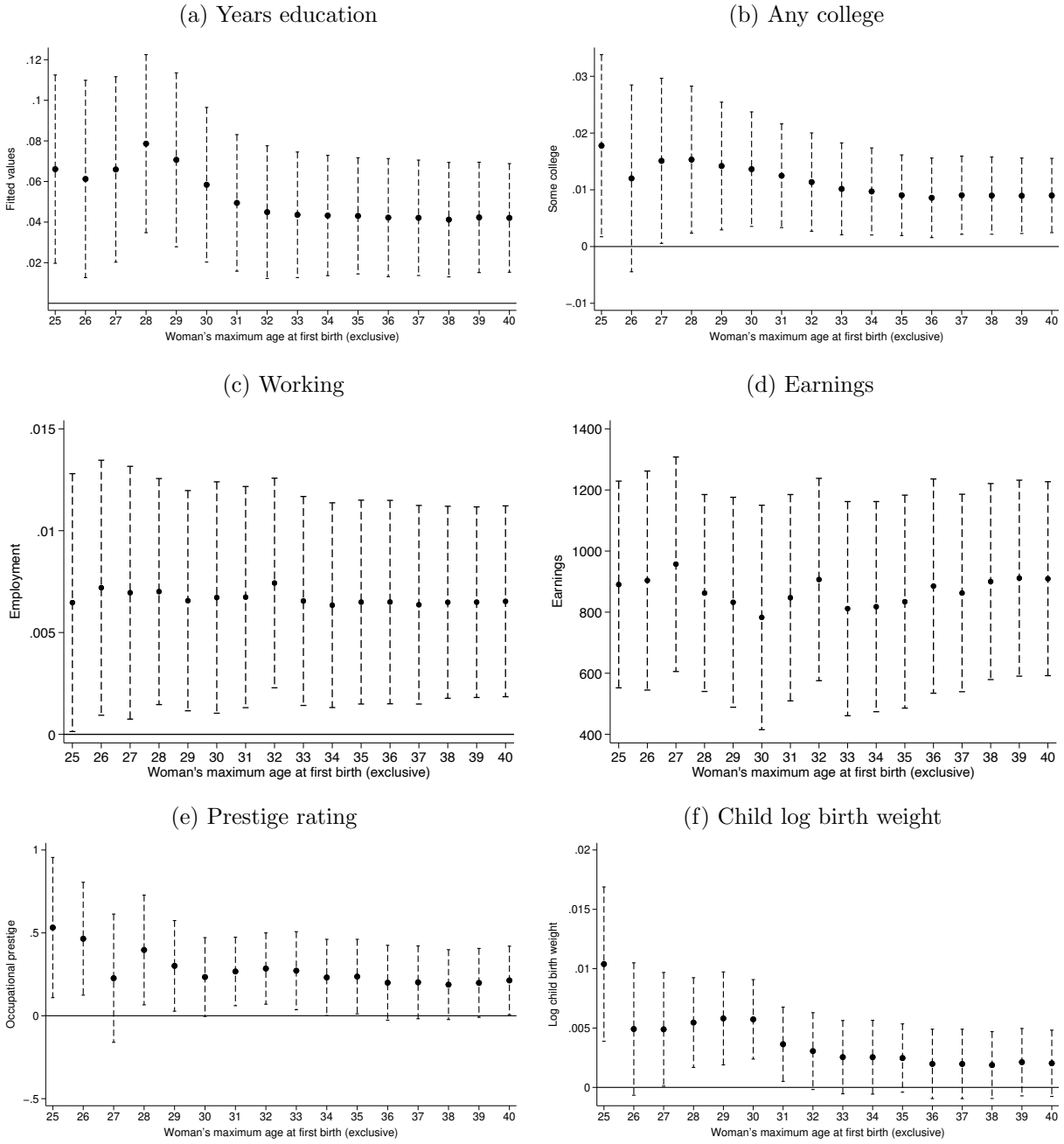
Notes: This figure presents the estimated effect (and 95% confidence interval) of being born just before the school entry cutoff on the likelihood of having a child with a birth weight greater than X, where X is shown on the x-axis. See the notes of Figure 3 for details about the birth records sample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

Figure A.11: Effects at Mid-Career using Alternative Bandwidths



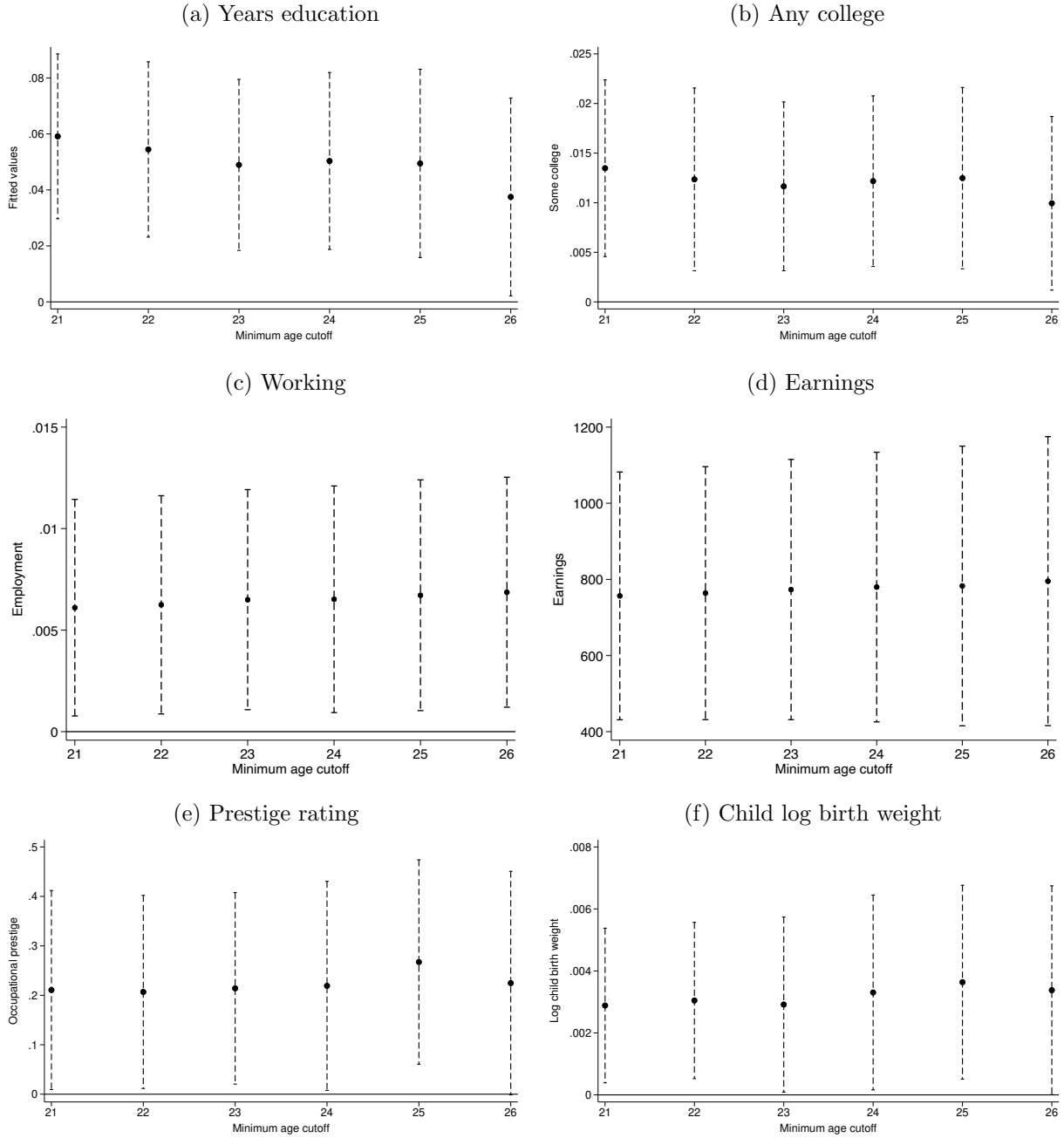
Notes: This figure presents the estimated effect of being born just before the school entry cutoff at mid-career across bandwidths, along with 95% confidence intervals. Each subfigure presents a different outcome: imputed years of education (Panel A), completion of some college (Panel B), working (Panel C), total earnings (Panel D), average occupational prestige (Panel E), child log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation, and the notes of Figure 5 for details about the baseline earnings sample.

Figure A.12: Effects at Mid-Career using Alternative Age at First Birth Cutoffs



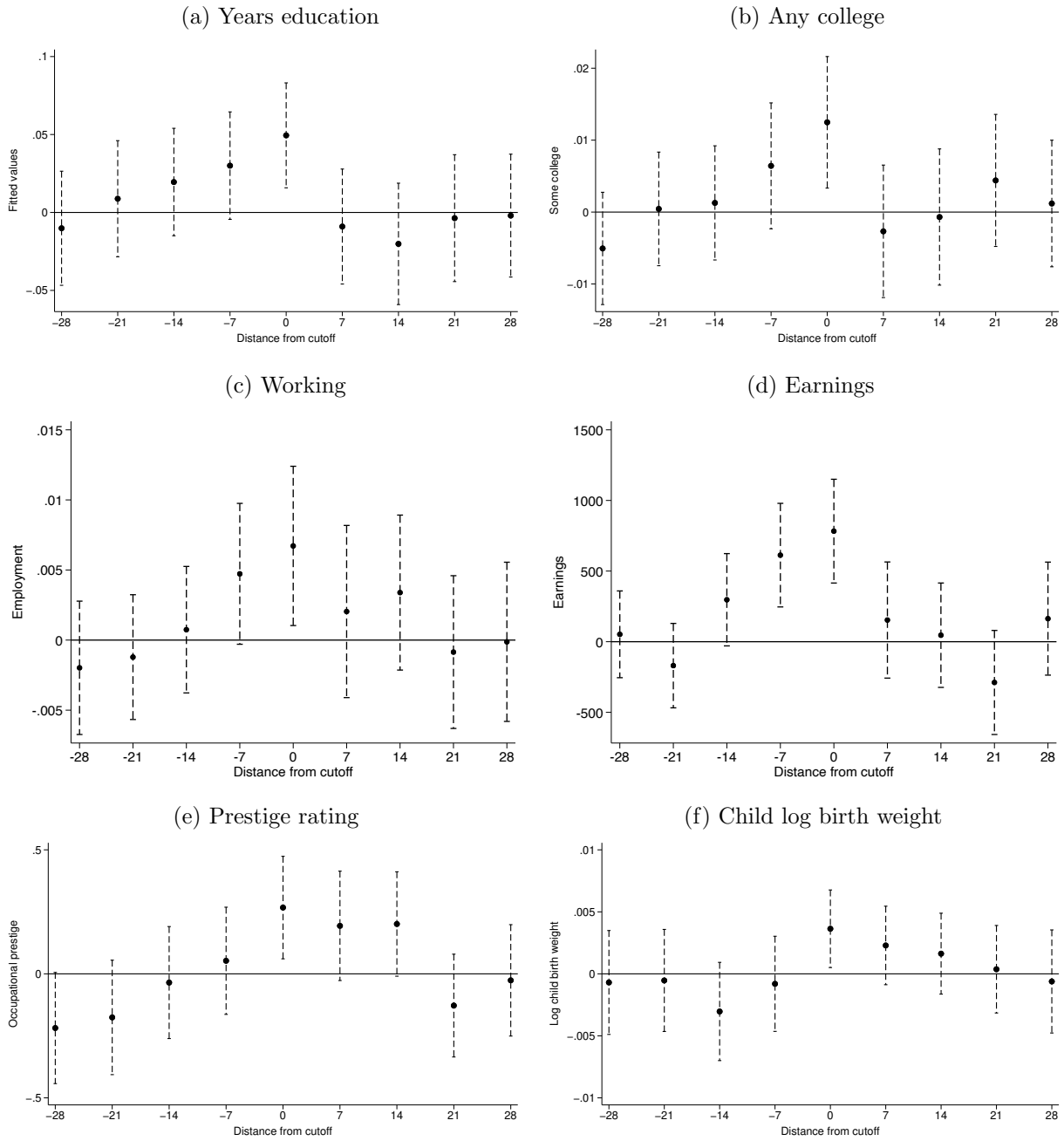
Notes: This figure presents the estimated effect of being born just before the school entry cutoff at mid-career using different sample cutoffs for age at first birth, along with 95% confidence intervals. Each subfigure presents a different outcome: imputed years of education (Panel A), completion of some college (Panel B), working (Panel C), total earnings (Panel D), average occupational prestige (Panel E), child log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation, and the notes of Figure 5 for details about the baseline earnings sample.

Figure A.13: 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 (Panel C), total earnings (Panel D), average occupational prestige (Panel E), child log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation, and the notes of Figure 5 for details about the baseline earnings sample.

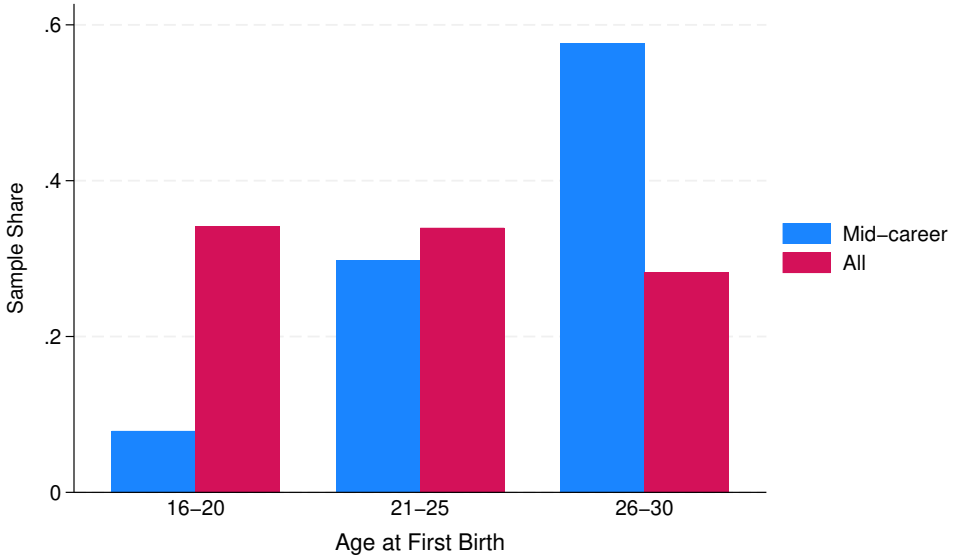
Figure A.14: Effects at Mid-Career using Placebo Cutoffs



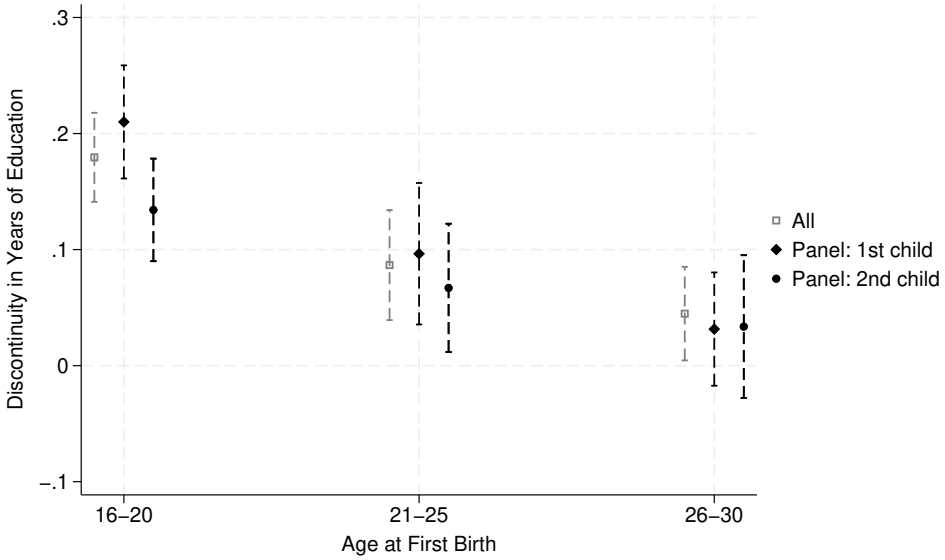
Notes: This figure presents the estimated effect of being born just before the school entry cutoff at mid-career for placebo school entry cutoffs, along with 95% confidence intervals. The placebo cutoffs are chosen within an eight-week window around the cutoff and spaced apart by two-weeks. Each subfigure presents a different outcome: imputed years of education (Panel A), completion of some college (Panel B), having positive earnings (Panel C), total earnings (Panel D), average occupational prestige (Panel E), child log birth weight (Panel F). See the notes of Figure 3 for further details about the birth records sample and estimation, and the notes of Figure 5 for details about the baseline earnings sample.

Figure A.15: Sample Shares and Impacts on Education by Age at First Birth (Birth Records Sample)

(a) Sample shares



(b) Impacts on Education



Notes: Panel A of this figure shows sample shares by bins of age at first birth (16-20, 21-25, 26-30) for women in the recreated earnings panel overall and at mid-career. Panel B shows the estimated effect (along with 95% confidence intervals) of being born just before the school entry cutoff on completed years of education by bins of age at first birth (16-20, 21-25, 26-30) for women in the recreated earnings panel. The grey unfilled markers show estimates for the whole sample. The black filled markers show estimates when we limit to a balanced panel of women: the diamond markers show estimates for a first birth; the circle markers show estimates for a second birth. See the notes of Figure 3 for further details about the birth records sample and estimation.

Table A.1: Descriptive Statistics Across Estimation Samples

	All				Drop Black mothers			
	Birth Records	Birth Replication	Earnings Panel	Panel Replication	Birth Records	Birth Replication	Earnings Panel	Panel Replication
Black	0.09	0.09	0.10	0.10	0.00	0.00	0.00	0.00
White, non-Hispanic	0.32	0.31	0.37	0.37	0.35	0.34	0.41	0.41
Hispanic	0.54	0.55	0.49	0.47	0.59	0.60	0.54	0.53
Year of birth	1987	1988	1982	1982	1987	1988	1982	1982
Age at first childbirth	22.93	23.04	22.99	23.01	23.02	23.09	23.09	22.87
Observations	388,070	409,000	320,000	349,716	347,541	374,000	290,000	312,239

Notes: This table presents summary statistics for our four main estimation samples: mothers in the 2007-2017 birth records (columns 1 and 5); women in the recreated birth records sample in the linked IRS-Census records (columns 2 and 6); women in the earnings panel (columns 3 and 7); and mothers in the recreated earnings panel in the 1989-2017 birth records (columns 4 and 8). Each observation is a unique woman, and we limit to a 70-day bandwidth around the entry cutoff, as in our baseline estimation. Observation counts for the birth records replication and earnings panel samples are rounded, consistent with Census disclosure standards. See the notes of Figures 3 and 5 for details about the birth records and earnings panel samples.

Table A.2: 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	0.099
	(0.228)	(0.273)	(0.286)	(0.285)	(0.299)
White, non-Hispanic	0.316	0.376	0.333	0.326	0.372
	(0.465)	(0.484)	(0.471)	(0.469)	(0.483)
Hispanic	0.454	0.480	0.526	0.532	0.478
	(0.498)	(0.500)	(0.499)	(0.499)	(0.500)
Asian	0.132	0.034	0.024	0.024	0.022
	(0.338)	(0.181)	(0.152)	(0.153)	(0.147)
High school graduate	0.845	0.874	0.854	0.850	0.806
	(0.362)	(0.332)	(0.353)	(0.357)	(0.396)
Some college or more	0.594	0.581	0.515	0.513	0.461
	(0.491)	(0.493)	(0.500)	(0.500)	(0.498)
College graduate	0.334	0.259	0.172	0.169	0.164
	(0.472)	(0.438)	(0.378)	(0.375)	(0.370)
Age	27.765	26.234	24.285	24.286	23.706
	(6.239)	(6.034)	(4.603)	(4.621)	(4.568)
Year of birth	1,983.603	1,985.292	1,987.139	1,986.943	1,982.269
	(6.681)	(6.468)	(5.231)	(5.090)	(3.145)
Worked last year	0.776	0.805	0.775	0.769	-
	(0.417)	(0.396)	(0.418)	(0.422)	-
Infant birth weight (g)	3,286.217	3,297.666	3,299.212	3,300.700	3,318.371
	(556.8)	(567.0)	(557.2)	(556.1)	(564.2)
Observations	3,931,688	2,006,194	1,562,347	516,266	583,927

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 in the recreated earnings panel, who are born between 1977-1987. Each observation is a childbirth event. Source: California birth records, 2007–2017 (birth records sample) or 1989-2017 (earnings sample).

Table A.3: 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) for the birth records sample. 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.4: 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) for the recreated earnings sample. 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.5: 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.6: 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 2 for details about the earnings panel and estimation.

Table A.7: 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 2 for details about the birth records sample and estimation.

Table A.8: 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 2 for details about the earnings panel and estimation.

Table A.9: 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	778.0***	730.0***	695.0***	525.6***	804.3***	855.4***
	(145.0)	(197.9)	(81.9)	(52.0)	(172.7)	(305.0)
Mean above cutoff	25,500	29,470	11,040	7,624	28,750	42,150
Observations	3,454,000	6,348,000	637,000	2,331,000	2,818,000	4,017,000

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). Values are rounded, consistent with Census disclosure standards. See the notes of Figure 5 for details about the earnings panel and estimation.

Table A.10: Discontinuity in Employment and Earnings in the Year of Childbirth

	Working last year		Earnings	
	Earnings sample	Birth records sample	Earnings sample	Birth records sample
A. All				
Born before cutoff	0.010**	0.014***	614.3***	541.7***
	(0.004)	(0.002)	(168.9)	(103.4)
Mean above cutoff	0.803	0.719	18,540	13,670
Observations	292,000	492,000	292,000	492,000
B. Younger than 25				
Born before cutoff	0.009*	0.018***	498.5***	504.8***
	(0.005)	(0.004)	(111.6)	(68.7)
Mean above cutoff	0.797	0.649	8,900	6,642
Observations	94,000	275,000	94,000	275,000
C. Age 25 or older				
Born before cutoff	0.011***	0.009**	684.6**	603.1***
	(0.004)	(0.004)	(238.5.2)	(223.5)
Mean above cutoff	0.806	0.806	23,130	22,480
Observations	198,000	217,000	198,000	217,000

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). Values are rounded, consistent with Census disclosure standards. See the notes of Figure 5 for details about the data and estimation.

Table A.11: 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 (or 0 for non-workers). Work inflexibility from Cubas et al. (2019), and is defined as 1 minus the typical share of hours worked during between 8 a.m. and 5 p.m. for each occupation. See the notes of Figure 3 for details about the birth records sample and estimation, and Appendix B for descriptions and sources for the outcomes.

Table A.12: Robustness of Mid-Career Effects to Estimation Parameters, Inference, and 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)
Earnings	782.8***	668.4***	782.8***	782.8***	630.8***	727.8***
	(187.5)	(85.7)	(55.5)	(187.5)	(97.2)	(52.2)
Employment	0.007**	0.005***	0.007***	0.007**	0.004***	0.006***
	(0.003)	(0.001)	(0.001)	(0.003)	(0.001)	(0.001)
Conditional Earnings	804.3***	751.9***	804.3***	804.3***	723.2***	741.7***
	(172.7)	(88.9)	(63.8)	(172.7)	(104.3)	(59.9)

Notes: This table presents the estimated effect at mid-career 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. Estimates for earnings, employment, and conditional earnings are rounded, consistent with Census disclosure standards. See the notes of Figure 3 for details about the baseline birth records sample and estimation, and the notes of Figure 5 for details about the baseline earnings sample.

Table A.13: Discontinuity in Infant Outcomes and the Pregnancy Environment Including Third Births (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	1.336	0.258	-0.000	0.040	0.005	-0.004
	(0.002)	(4.584)	(0.518)	(0.002)	(0.039)	(0.004)	(0.004)
Mean above cutoff	8.076	3274.128	272.353	0.051	11.407	0.784	0.707
Individuals	243,410	243,410	243,390	240,369	237,048	240,927	242,904
B. Age 25 or older							
Born before cutoff	0.003***	8.275**	0.504**	-0.004***	0.023	-0.008***	-0.012***
	(0.001)	(3.581)	(0.243)	(0.001)	(0.031)	(0.003)	(0.003)
Mean above cutoff	8.102	3362.824	272.802	0.042	12.239	0.392	0.335
Individuals	324,962	324,962	324,912	322,546	319,690	322,020	324,671

Notes: This table presents the estimated effect of being born prior to the school entry cutoff on the birth outcomes of children and the pregnancy environment when we include third births. 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. Estimates are weighted using a triangular kernel and use a bandwidth of 70 days around the cutoff. Standard errors are clustered by day of birth relative to the school entry cutoff. Source: California birth records 2007–2017.

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 1.3% of outcomes of non-college-educated women born before the cutoff. Column 3 presents the upper bound, dropping the bottom 1.3% 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.

B Additional Data Description

B1 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 B1 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.[29] 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, 59% are in the labor force. For the 36% who are not in the labor force, the majority report being stay-at-home mothers.

Table B1: 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.

B2 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.

[29] 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., 2024). 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 (rather than having a schedule set by a manager); telecommute; perform moderate physical activity (rather than perform heavy physical activity); sit (rather than perform heavy activity); choose how to do work (rather than well-defined tasks); 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 (rather than on the team's performance); work alone (rather than in a team, and being evaluated as a team); 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.

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,for continuous measures, or the maximum across jobs, for binary measures.