Multigenerational Benefits of Women's Education

Na'ama Shenhav*

January 31, 2025

Preliminary and Incomplete

Abstract

Women's steady rise in college-going – attaining and then surpassing parity with men – has been arguably the largest shift in the landscape of post-secondary education in recent decades, yet little is known about how this has affected women's careers and families. This paper uses a regression discontinuity design based on an individual's date of birth and school entry laws to evaluate the early- and mid-career impacts of greater schooling on women's work and spillovers to children's well-being. I find that women who obtain greater education are more likely to be employed, more likely to hold professional, higher-prestige jobs that also have better job amenities, and have higher earnings. Further, a majority of these additional jobs require *exactly* some college, suggestive of a link between improvements in careers and the rise in post-secondary education. The effects on women's job amenities are larger in mid-career than early-career, and when monetized, correspond to a substantive return to education for women. Finally, I find positive effects on children's health, but only once job amenities improve, suggesting a connection between these outcomes. The results indicate that investments in women's college attendance are among the most effective policies to improve maternal careers, and has spillovers to the next generation that grow over time.

*UC Berkeley and the National Bureau of Economic Research. Mary Yilma and Jacklyn Pi provided excellent research assistance. I am grateful to Patricia Cortes, Yana Gallen, Matt Notowidigdo, Chris Walters, Melanie Wasserman, and to seminar audiences at the University of Nebraska, BU Questrom, San Diego State University, UC Davis, the SOLE Annual Meeting and the Nebraska Labor Summit for helpful comments. The Census Bureau has ensured appropriate access and use of confidential data and has reviewed these results for disclosure avoidance protection (CBDRB-FY25-CES004-006)

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

At the same time, recent work suggests that traditional measures of labor market success, such as earnings, may not fully capture the value of education for women's careers. For example, women value many job amenities, such as social meaning, as highly as a moderate increase in wages (Maestas et al., 2023; Kesternich and De Schouwer, 2023). Education may also help buffer against costly shocks to women's productivity, such as those that occur around childbirth, which are an important contributor to the gender gap in earnings (Cortés and Pan, 2020). Yet, to date, there are no estimates of the return to education that incorporate these potential benefits.

This paper studies the early- and mid-career impacts of greater schooling on women's work, job amenities, and spillovers to child well-being. Building on the classic work of Angrist and Krueger (1991) and refinements in McCrary and Royer (2011), I use a regression discontinuity design based on individual's date of birth and school entry laws to evaluate the effects of completing more education. I draw on employment surveys, administrative earnings records, and confidential health information of mothers from California. These data allow me to measure intent-to-treat effects of being born before the entry threshold on multigenerational outcomes, including impacts on women's education, propensity to work, earnings, and job conditions, and on the health of their children. I also newly map the *trajectory* of these effects over women's careers, which allows me to illustrate

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

how multigenerational benefits evolve over the life cycle.

To begin, I show that the entry cutoff affects completed education, but not fertility, at each stage of women's careers. Women born just-before versus just-after the entry cutoff have, on average, 0.10 more years of education, and are 2.2 percentage points (p.p.) more likely to graduate from high school, 2.0 p.p. more likely to complete some college, and 0.5 p.p. more likely to have a college diploma. The improvement in completed education is persistent over the life-cycle, although by mid-career (ages 25 to 40) it is concentrated in a higher completion of some college. At the same time, women's pre-determined characteristics, propensity to have a child, and fertility timing are all well-balanced across career stages. This provides reassuring evidence that birth dates are not manipulated around the cutoff.

Following women over time, I find that the rise in education manifests in substantial improvements in outcomes in later-life and across generations. First, women born just-before rather than just-after the entry cutoff are 0.5 to 1.1 p.p more likely to have worked in the last year and have $600 higher earnings. The effects on employment and earnings grow over women's careers, consistent with a steeper earnings trajectory with greater education. The rise in employment is accompanied by a strengthening of women's career identities, based on an increase in reporting an occupation in the labor force (rather than common alternatives, like "homemaker.")

Second, women born just-before the cutoff are more likely to hold jobs that are associated with greater social status and are more selective, particularly in mid-career. This includes increases in the shares of women who hold a professional job, and whose job requires a college credential. The rise in college-educated positions accounts for at least 60% of the additional jobs at the cutoff, suggesting that marginal students may deliberately target jobs that make use of their education credentials. Further, the impacts on job selectivity grow over a career, consistent with women born before the cutoff having greater job mobility.

Third, jobs improve along other dimensions of quality. In particular, women born just-before the cutoff have jobs that are rated as more prestigious, have higher-educated co-workers, and are more socially impactful – among gains in other working conditions. In part this reflects a greater propensity to be educators, which are jobs that feature many of these amenities. In line with the

dynamic effects on professional positions, the impacts on job amenities also increase in mid-career. These work upgrades do not appear to be offset by greater *effort*, in terms of more work hours per week or work weeks per year.

Fourth, I find that children's health at birth improves at the cutoff, but only once women advance into jobs with better amenities. In particular, I find that birth weight increases by 0.4 percent (9 grams) and gestational age lengthens by 0.5 days for children who are born during mid-career. In contrast, there are no effects on children who are born in early-career. Prenatal *inputs* improve in tandem with careers, including a mid-career reduction in smoking, lower use of public insurance, and increase in expected earnings. Thus, early-career effects on next-generation health appear to be a lower bound of human capital spillovers to children (McCrary and Royer, 2011).

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

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

Assuming that education is the *only* mechanism at the cutoff, I find that the implied return to education investments for women's careers is substantive. Completing one additional year of education raises employment by nearly 11 percentage points. This can explain most, if not all, of the considerable, 45 point gap in employment across women who drop out of high school and who attend some college. Thus, education appears to play a central role in women's decisions to pursue a career, even in the face of the challenges of motherhood.

Further, I show that education-induced improvements in job amenities contribute substantially to worker welfare. In particular, I monetize the value of the estimated impacts on working conditions using previous existing willingness-to-pay estimates (Maestas et al., 2023), and find that these amenity upgrades are equivalent to a 3% increase in annual earnings per year of education. This amenity-based return is roughly equal to one-third of the wage effects of two-year college attendance for women, based on earnings effects in prior work and to six percent of the earnings return for mothers in this context (Jacobson et al., 2005; Mountjoy, 2022). Thus, incorporating these impacts on working conditions would imply that the value of education based on wages alone provides a modest lower bound for the total labor market value of education.

Finally, I use the long span of infant outcomes in the birth records to study heterogeneity in the intergenerational return to maternal education over time. Applying the same research design to a moving window of birth records, I find that the impact of the cutoff on child birth weight has increased substantially over the last three decades. This entirely reflects a growing *transmission* of education, as the "first stage" on completed education has been stable over time. Further, since gains in birth weight are associated with greater career success in adulthood, this could also suggest that maternal education may have a larger role in child mobility going forward (Black et al., 2007; Royer, 2009; Bharadwaj et al., 2017).

The results rely on the identification assumption that assignment to either side of the entry cutoff is as good as random. The balance tests around the cutoff, described earlier in the introduction, provide compelling, direct evidence in support of this assumption. As additional support, I show that the discontinuities in outcomes are visibly apparent and occur precisely at the entry cutoff. This mitigates potential concerns related to unobserved, seasonal differences in the composition

of births, which would be expected to produce more gradual changes in outcomes (Buckles and Hungerman, 2013). Additionally, I show that placebo entry cutoffs do not reproduce the main results, which confirms that average outcomes are typically similar within narrow windows of birth dates. Further, I show that the conclusions are not sensitive to using a balanced panel of mothers. This rules out the possibility that the age heterogeneity in the results could reflect variation in the unobserved types of mothers by age of childbirth. Finally, in favor of the external validity of the results, I show that the outcomes for mothers in the sample match those for women more broadly.

This paper makes four primary contributions. First, I show that post-secondary education increases professional work and improves job amenities, and that these upgrades contribute meaningfully to the return to education. This adds to a sparse literature on the effects of education on job titles, which finds that obtaining a college diploma in France raises men's propensity to be in elite occupation (Maurin and McNally, 2008) and that being admitted to a selective major in college increases the likelihood of working in a related industry after graduation (Bleemer and Mehta, 2022; Grosz, 2020). Relative to these studies, I examine effects on a broad set of job amenities to capture how schooling affects job mobility and one's work environment. Moreover, I use novel estimates of workers' valuations of working conditions (Maestas et al., 2023) to monetize the effects of schooling on amenities for the first time. The estimates imply that schooling-induced improvements in job amenities lead to a substantial increase worker well-being, separate from effects on earnings.

Second, I show that post-secondary education plays a central role in women's labor force participation decisions. This complements recent work that largely uses distance to college instruments and individual fixed effects shows that access to two-year colleges improves women's wages (e.g Jacobson et al., 2005; Mountjoy, 2022), and that select two-year credentials raise employment rates (Stevens et al., 2019; Grosz, 2020; Minaya and Scott-Clayton, 2022).[2] My results reinforce and extend these studies by using a transparent and credible empirical strategy; estimating the employment effects of *attending* college, which is more common than completing a credential (PPIC Higher

[2]In contrast, completing earlier grades has been shown to have little effect on employment (Oreopoulos, 2006; Pischke and von Wachter, 2008; Devereux and Hart, 2010; Fredriksson and Öckert, 2013). Dobkin and Ferreira (2010) study the effect of the school starting age on women's outcomes, and find no impact on employment *conditional* on being in the labor force. This is consistent with the interpretation that our effects are driven by increases in labor force participation.

Education Center, 2019); and showing that the impacts on work are driven by entry into high-skilled occupations. My findings imply a large causal role for education in explaining employment gaps among women.

Third, I provide new evidence on the multigenerational impact of maternal education over the life cycle. This builds on earlier work that studies (i) the average effect of maternal education on child health (Currie and Moretti, 2003; Lindeboom et al., 2009; McCrary and Royer, 2011) or (ii) the life-cycle effects of schooling for men (e.g., Bhuller et al., 2017; Nybom, 2017) – but has not studied life-cycle effects for women. I find that education has modest effects on labor market earnings and amenities in early career that grow and stabilize at substantially higher levels by mid-career. This pattern suggests that the unique role of mothers' human capital in children's long run outcomes (e.g., Black et al., 2005) may in part reflect improvements in the in-utero environment, and growth in maternal investments in children over the life cycle. I also find suggestive evidence that the impacts of schooling on women's professional success and child investments may be linked, as a potential mechanism for impacts on long-run outcomes.

Fourth, I contribute to the work on the long-run effects of the school entry threshold on labor market outcomes (Dobkin and Ferreira, 2010; Black et al., 2011; Fredriksson and Öckert, 2013; Oosterbeek et al., 2021). Relative to the one other evaluation of this policy in the U.S., which finds null effects on careers, I examine a more extensive and precise set of measures labor market success, including labor force participation, administrative earnings, occupations, and workplace amenities, which provides a more complete assessment of policy impacts (Dobkin and Ferreira, 2010). This leads to a qualitatively and quantitatively different conclusion about the impacts of early school entry for women: that it has substantial positive effects for professional development. I also develop a new approach to distinguish between the effects of younger age for grade and greater years of education, based on the skill level of occupations. This could be a useful diagnostic for future work on this policy to understand the role of these mechanisms.

1 School Entry Policies and Conceptual Framework

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

1.1 Background on School Entry and Leaving Laws

Two discrete, age-based policies around school attendance contribute to the discontinuity in education at the school entry age cutoff in California. First, children are only legally permitted to enroll in kindergarten if they turn five by the prescribed cutoff date during the academic year. For the women in this study, the relevant cutoff date was either December 1 or December 2 (Gregory, 1976, 1987; Dobkin and Ferreira, 2010).[3] This entry rule leads to sharp change in a child's expected and actual school entry age based on one's date of birth. In particular, two girls who are born on opposite sides of the entry cutoff in 1980 are expected to begin school one year apart – at ages 4-years-and-9-months and 5-years-and-9 months, respectively. In the data, the average gap in entry ages at the cutoff is slightly below this this benchmark (0.75 years), consistent with strong, but imperfect compliance (Elder and Lubotsky, 2009). Notably, girls are more likely to comply with these policies, which suggests that the discontinuity in school entry ages for girls may be even larger than this estimate for all children (Schanzenbach and Larson, 2017).

Second, despite this variation in age of school entry, all California students are legally required to remain in school until age 18. This implies that a student who enters school one year earlier will be able to complete more grades if the compulsory schooling age binds. For example, under normal grade progression, early entrants will complete 12^{th} grade before turning 18, while late entrants will only complete 11^{th} grade. In line with this, prior work shows that early entrants are more likely to

[3]The cutoff date was changed from December 1 to December 2 in 1987 (Gregory, 1987). I assume that the new policy went into effect the following year, in 1988, and thus applies to children born in 1983 onward, who turned five in that year.

finish high school and attend some college (McCrary and Royer, 2011; Dobkin and Ferreira, 2010).

1.2 Expected Effects of Early School Entry on Careers

While the goal of the empirical analysis is to identify the effect of education, early school entry may affect later-life outcomes through other channels. To guide the interpretation of the results, I outline the various mechanisms through which the entry cutoff could affect careers. I also develop empirical tests to help distinguish the effects of education from possible alternative mechanisms.

Greater years of schooling First, early entry may impact labor market outcomes by increasing completed years of education – particularly through higher rates of college entry. This could set off a cascade of career changes by leading to an improvement in labor market signals, credentials, and skills, among other possibilities. The clearest evidence of this is in the substantial wage gains of women who attend two-year colleges (Jacobson et al., 2005; Stevens et al., 2019; Grosz, 2020; Mountjoy, 2022). These wage gains could reflect the productive skills that are taught in college courses (Arteaga, 2018) or greater eligibility for high-wage jobs that require a professional license or credential (Kleiner and Soltas, 2019). Along similar lines, greater schooling may also lead to movement up the job ladder, by fulfilling requirements for more selective work.

Younger age-for-grade Second, early entry reduces a child's relative age compared to her classroom peers. Unlike years of schooling, the direction of this channel on career outcomes is much less clear. On the one hand, children who are relatively younger in the classroom 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 such impacts (positive or negative) appear to be concentrated among men (Black et al., 2011; Bedard and Dhuey, 2006; Cascio and Schanzenbach, 2016). To the extent that younger grade-for-age improves careers, these effects appear to be temporary, and linked to a rise in early work experience from finishing school sooner (Black et al., 2011; Fredriksson and Öckert, 2013).

Partner improvements Finally, early school entry may affect one's choice of partner through changes in assortative mating (for women whose education is increased by the cutoff) or through exposure to older peers (Lefgren and McIntyre, 2006; McCrary and Royer, 2011; Geruso and Royer, 2018). In that sense, this possibility is best viewed as mediator of the first two channels, rather than an independent mechanism. The career implications of changes in partner selection are also ambiguous; improvements in partners may amplify or detract from one's own career ambitions.

Empirical tests To distinguish the role of greater education from other channels, I develop two sets of empirical tests. First, to distinguish between the effects of schooling and younger age-for-grade, I study the impacts of the cutoff on jobs' credential requirements, and how closely these align with the effects on education. To understand the idea behind this test, suppose, for example, that the cutoff only increases the likelihood of having some college. If greater schooling is a primary channel, this should lead to parallel effects on job credentials: women should be more likely to have jobs that require exactly some college, and should not be more likely to hold jobs that require a college diploma. In contrast, if younger age-for-grade is the main channel, the impacts on jobs are expected to be more uniform across job skills. More generally, this suggests that the closer the alignment between effects on job credentials and schooling, the more likely that education is to be the dominant force. Concretely, I test for impacts on jobs whose education requirements are the *same* or *different* than the effects on completed education at the cutoff (e.g., require some college or require a college degree, in the previous scenario). I then use the ratio of these effects as a measure of the relative contribution of schooling versus age-for-grade.

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

2 Data

I obtain information on the multigenerational outcomes of women and their children from the restricted-access microdata of birth certificates in California, administrative earnings records, and information on job characteristics from nationally-representative surveys.[4] This section describes the main variables and data sources used in the analysis, followed by descriptive statistics on the estimation sample.

Vital Statistics records The complete California birth certificate data span from 1960 to 2017 and include detailed information from forms filled out at the time of childbirth. Based on the availability of labor market information, the primary analysis focuses on the years 2007 to 2017; however, the data construction and extensions make use of information from earlier years.[5]

An important advantage of these data is that they contain detailed information on both parents and children at a point in time, including parents' background and employment, and measures of child health and prenatal investments. First, the data include each parent's name, date of birth, state of birth, race, and completed education. Educational attainment is reported in fine categories, including high school graduate, some college, associate's degree, bachelor's degree. I use these education groups as outcomes and also as inputs to predict completed years of education.[6] I use personal identifiers to construct a mother's fertility history and determine her age at first birth.

Second, the data include a parent's typical occupation and industry of work.[7] Relative to typical labor market surveys, the job titles provided by parents are unstructured (write-in), and can include information on activities inside and outside of the labor force. For example, responses include specific positions like "third grade teacher" and at-home roles like "homemaker" – the most-commonly

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

[5]I use records from 1989 to 2017 to construct a mother's fertility history and predicted years of education, and to measure intergenerational impacts over time (see Section 6.3). I use the full history of records to obtain the grandparental traits for each birth.

[6]To predict years of education, I use earlier records, from 1989 to 2005, to regress completed years of education on indicators for the four education categories; fixed effects for race and age fixed; and a linear term in maternal year of birth using records. I then use the estimated coefficients to generate fitted values for my sample.

[7]These fields are collected in order to enable tracking of job-related risks for infant birth defects, which has been the primary use of these data (Brender et al., 2008).

reported occupation for women. To have a consistent measure of jobs, I harmonize these occupations to conform with the Census Bureau's 2010 classification system, as described in Appendix A.1. I refer to women who are assigned a standardized occupation code as "identifying as a worker." I also infer school attendance from this field, based on reports of being a student.

Third, each parent is asked to report the date (month and year) he or she last worked, which I use to determine recent employment. Parents are permitted to report any date; however, in practice the dates are very recent: 95% are within the last year and 77% are in the last month. The main employment outcome is an indicator for having worked in the last year and not being a student.[8] Note that because of the backwards-looking nature of this measure, reports by first-time mothers will capture work *before* having children, while reports by second-time or higher-parity mothers will capture work *after* having children.

A possible concern is that parents may report their employment information inaccurately, either due to lapses in memory or purposeful misrepresentation. However, validation exercises suggest that this is not the case. I show that the employment trends in the sample match those in other labor market surveys – see Section 2.1 – and that the results for employment are very similar when I use self-reported and administratively-reported outcomes.

Finally, the birth records contain extensive measures of infant health and prenatal care. For infant outcomes, I focus on birth weight (measured in grams or log-transformed) and gestational age (in days), due to the strong correlations with child cognition and long-run well-being (Black et al., 2007; Figlio et al., 2014). For prenatal care, I focus on whether a woman ever smoked during her pregnancy or the preceding quarter, her total number of prenatal doctor visits (since nearly all women attend at least one visit), and indicators for whether she participated in public health insurance (Medi-Cal) or nutritional assistance (WIC).

Census and IRS records I measure individual annual earnings from the income information reported to the IRS from 1998 and 2022. This includes income that is reported by individuals on Form 1040 filings as well as by employers on W-2 forms. To recreate the sample of mothers in the

[8] I set employment as missing for the 1% of individuals who report a date last worked more than a year prior to childbirth, since many of these appear to be implausible.

Vital Statistics, I merge these records to the Census Household Composition Key and 2000 Census, which allows me to identify children for women in our sample, and also with the 2010 Censuses and 2005-2020 ACS surveys, which contain individual race. Finally, I use the Numident file to obtain exact date of birth and location of birth for each parent and child.

Job characteristics To characterize the status, selectivity and working conditions of parents' occupations, I assemble a wide range of occupation-level job characteristics from population surveys and previous work. See Appendix A.2 for details on the data sources used to construct these variables. The measures of status include an indicator for being a professional, which is a group of high-skilled, white-collar positions (from Autor and Dorn, 2013), and a percentile ranking of occupational prestige (from Hughes et al., 2022; Smith and Son, 2014). Job selectivity is proxied by the minimum years of education required to obtain a license or credential and the share of workers in the occupation who attended some college. Working conditions include the degree of scheduling inflexibility (from Cubas et al., 2019); the average hours worked per week; the average weeks worked per year; the average hourly wage and average annual earnings; and twelve other job amenities, including social impact (used in Maestas et al., 2023). When possible, job characteristics are calculated specifically for California workers.

Sample definition I construct the estimation sample based on two main criteria. First, I focus on mothers who were born in California to simplify the analysis of school entry policies. Second, I limit to mothers who have a first birth by age 31 because they are more likely to be "compliers" of school entry policies (see McCrary and Royer, 2011, and evidence in Section 5). This leaves 92% of mothers who do not have a college degree.[9,10] Further, for the analysis of childbirth outcomes, I focus on each woman's first two births (the complete fertility history for 80% of mothers), and trim outlier ages (below 16 or beyond 40). I apply these criteria separately in the Vital Statistics and the linked IRS-Census records, and reassuringly obtain a very similar sample across both sources.[11]

[9]The higher compliance rates may reflect the more-disadvantaged backgrounds of these women, who have younger parents and were born in counties with lower average education (see Appendix Figure A.1).

[10]Note that this criteria excludes a small share of mothers who have a first birth outside of California, since I can not determine their age at first birth.

[11]This avoids the need to link these data together, and avoids the potential risks associated with a large-scale data transfer.

I show that the results are robust to alternative sample criteria in Section 5.

Table 1 provides summary statistics for all first- and second-time mothers (column 1); and the subset of those who are California natives (column 2), in the estimation sample (column 3), and near the school entry cutoff (column 4). Relative to all mothers and California natives, the estimation sample has a similar share of high school graduates (85%) and share employed in the previous year (77%); but a lower share with a college degree and a lower average age. The traits for the estimation sample remain the same when I limit to women close to the entry cutoff and who meet the sample age criteria (column 4).

2.1 Descriptive Evidence

To motivate the analysis, Figure 1 presents descriptive evidence on how careers and child health vary based on different levels of women's education. While past work has documented education-based disparities in women's employment, I also document a strong divergence in women's professions, and show how differences in career paths correspond to disparities in infant health. At baseline, I focus on women in the birth records analysis sample. However, as a benchmark, I also show career outcomes for two other groups: (i) all mothers in the birth records and (ii) women of childbearing age in the American Community Survey (ACS) (Ruggles et al., 2010).[12]

Panel A of Figure 1 presents the share of women who worked in the last year across four levels of education; no high school diploma, high school diploma/GED, some college, and college or more. Rates of work rise steeply across these groups – from 37 percent to 72 percent, 81 percent, and 93 percent, in order of increasing education. Reassuringly, these trends are similar across sources, indicating that the work decisions in the analysis sample reflect broader patterns among women.[13] Moreover, these education disparities manifest both before *and* after having children (see Appendix Figure A.2). Hence, this gradient does not solely arise from differential responses to motherhood.

As a measure of job *quality*, Panel B shows how the share of professional work varies by edu-

[12] For ease of comparison, I apply the same age and nativity criteria as in the main analysis.

[13] Prior-year employment is consistently lower in the ACS, which could reflect subtle differences in incentives for reporting or in the survey instruments across sources. As one difference in the survey design, the ACS only collects information on whether an individual received wages or salary in the last year, not whether she was employed. This may lead to a lower employment-to-population ratio if some women under-report wages or only receive business income.

cation, conditional on working. While just 16% of non-high-school-graduates hold professional jobs (and a similar share of high school graduates), this jumps to 31% and 75% of those with some college and college or more, respectively. Combining this with the differences in employment in Panel A produces even larger gaps in career circumstances. Inclusive of non-workers, professional jobs are four times as common among women with some college than high school dropouts (25% vs. 6%).

Panel C shows that child health is positively related to both maternal education and employment. The thick, upward-sloping line shows a near-100 gram difference in average child birth weight between the least- and most-educated mothers. These health gains appear linked to maternal employment, as shown in the 50-gram surplus between working and non-working mothers in the dashed lines. This suggests that the children of more-educated mothers may have better early life health due to a dual advantage in the prenatal environment: higher rates of maternal work and resources (vertical shift in the intercept) coupled with other benefits of human capital (positive slope).

Panel D shows that infant weight is also strongly related to a mother's job title, indicating that some professions may be more conducive to early child development. Each marker in the figure shows the child outcomes of mothers in a distinct occupation, and the markers are ordered by rates of college attendance within the profession. The positive slope of the best-fit line echoes the patterns by education shown in Panel C. The vertical spread of the markers suggest that job-specific amenities may also influence child outcomes, *within* education groups. For example, teachers appear to have particularly good child outcomes (are above the best-fit line), while nurses do not (are below the best-fit line). This indicates that the *type* of work that women are engaged in may be a mediator for the effects of maternal education on child health.

3 Empirical Analysis and Identification

To study the role of early entry in first- and second-generation outcomes, I use the sharp change in school entry age among individuals who are born around the cutoff. Because school entry age may affect later-life outcomes through multiple channels, I estimate the intent-to-treat effect of being born just-before versus just-after the cutoff using a regression discontinuity design (RD).

Specifically, I estimate:

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

where Y_{ia} is either an outcome for woman i or for a child born to woman i when she is age a. The running variable is DOB_i, the difference between a woman's date of birth and the school entry cutoff for her birth cohort (either December 1 or December 2). BeforeCutoff_i is an indicator for whether a woman was born prior to the cutoff. The key coefficient of interest is δ, the change in outcomes of having a birthday prior to the cutoff. ρ_b is a fixed effect for year of birth b, which ensures that comparisons are made within mothers who grew up in the same policy environment. X_{ia} is a vector of additional individual covariates, which includes fixed effects for a woman's age and race (white non-Hispanic, Hispanic, Black, Asian, or other) as well as for child parity and having multiple births, for child outcomes. I recenter year of birth and age around the entry cutoff, such that these take on the same value for individuals born close to the same entry threshold who are observed in the same year.[14] At baseline, I apply triangular kernel weights, use a bandwidth of 70 days around the cutoff (from the range of optimal bandwidths suggested by Calonico et al. (2014)) and cluster standard errors on the running variable. I show that the results are robust to alternative weighting, bandwidths, and inference in Section 5.

For transparency into the RD estimates, I also present binned scatter plots of the mean outcomes around the cutoff. This allows for visual inspection of whether any discontinuity in outcomes occurs precisely at the cutoff. I residualize outcomes of year of birth fixed effects, as in the main specification, to ensure that changes in the volume or composition of births across years do not influence the interpretation of the patterns.

To examine the heterogeneous effects of early entry over women's careers, I split the sample into two groups: (i) early-career women, in their late-teens and early-twenties (ages 16 to 24) and (ii) mid-career women, in their mid-twenties or thirties (age 25 to 40). This allows me to trace how the impacts of early education experiences evolve as women progress further in their careers. To

[14] I also similarly recenter calendar year, and use these recentered measures of age and time for any analyses that stratify the sample along these lines.

ensure that these heterogeneous treatment effects reflect women's career progression and not other factors, I show that women's observable characteristics are balanced around the cutoff in each of these samples (below), and that the results are unchanged when I use a balanced panel of women (Section 5).

The key identification assumption for this RD design is that assignment to either side of the entry cutoff is as good as random. In a sample of California mothers, this could potentially be violated in two ways. First, if compulsory schooling policies affect either fertility or cross-state migration, there may be differential selection into the sample around the cutoff (Black et al., 2008). Second, the composition of women who are *born* just around the cutoff may differ (Buckles and Hungerman, 2013). Reassuringly, these concerns have not been born out in empirical tests using older cohorts of mothers (McCrary and Royer, 2011). I conduct similar tests for more recent cohorts of mothers.

I study potential manipulation around the cutoff with a series of balance tests. The first set of tests examine potential shifts in fertility around the cutoff – akin to testing for a break in the density of the running variable (McCrary, 2008). To measure effects on fertility *rates*, I divide the unique number of mothers who were born on date DOB_i by the number of females born in California on DOB_i. The second set of tests examine covariate balance. Because fertility responses may vary across demographic groups, I estimate effects separately by maternal race/ethnicity (Black et al., 2008), as well as for the pooled sample.

Columns 1–3 of Table 2 show that there is no significant change in the likelihood of having any birth, a first birth, or a second birth for any group. The narrow confidence intervals around the estimates for the larger subgroups allow me to rule out even moderate effects, such as a 3% change in any birth for Hispanics. I find similar null results when I test the joint significance of the estimates, as shown by the large p-values in the bottom of Table 2.

Columns 4–6 of Table 2 show that maternal age is also unaffected for the majority of groups, including White mothers, Hispanic mothers, and Other mothers (who account for over 90% of mothers). Nevertheless, there is a slight imbalance in age at first birth in the pooled sample ($p = 0.054$),

which is driven by a small decline among Black women.[15] This leads me to exclude this group at baseline, in order to restore balance and avoid possible contamination of the results. The remaining sample is balanced in demographic characteristics and proxies for childhood circumstances, including when I stratify by early- or mid-career mothers (see Appendix Table A.2).

I also test for more subtle forms of manipulation. I visually confirm that there is no sorting around the cutoff in histograms of the running variable, as well as binned scatters of fertility rates (see Appendix Figures A.4 and A.3). I also show that fertility timing remains balanced when I study distributional effects on age at childbirth (see Appendix Figure A.5). The collection of these results provide strong evidence ruling out selection around the cutoff.

4 Results

4.1 Effects on Completed Education

Figure 2 shows a binned scatterplot of average years of education for women born within 100 days of the cutoff. Panels A–C shows the averages for all women, for early-career women (younger than 25) and for mid-career women (age 25 or older). Women's completed education decreases sharply just after the cutoff in each of the panels, with gaps ranging from 0.05 years for mid-career women to 0.16 years for early-career women (see Column 1 of Table 3). The clear break in outcomes verifies that this educational advantage develops from adherence to school entry policies. Moreover, the persistence of these effects into mid-career indicates that school entry laws shape *permanent* levels of education. Outside of the break, the trend in mean education on either side of the cutoff is linear in age, consistent with the model, and often flat.

Decomposing these effects on years of schooling, I find that women born just-before the cutoff persist more in secondary and post-secondary education. Columns 2–4 of Table 3 show that, in the whole sample, these women are 2.2 p.p. more likely to graduate from high school, 2.1 p.p. more likely to complete some college, and 0.5 p.p. more likely to graduate from college (Panel A). These effects attenuate over a career, as with completed education (Panels B–C). However, the effects

[15] This could reflect the "incarceration" effect of schooling, since mothers who are born before the cutoff complete schooling earlier on average.

on high school graduation and some college remain prominent at mid-career. In particular, women born just before the cutoff are 0.3 p.p. less likely to be a high school dropout (7 percent) and 1.2 p.p. more likely to have attended college (1.7 percent).[16]

Importantly, the persistent effects on education do not reflect ongoing differences in school attendance. Column 5 of Table 3 shows that women born just before and after the cutoff are equally likely to report being a student at mid-career. In turn, this suggests that any later-life effects on work decisions can not be explained by transitory effects of school enrollment (i.e., catch-up among students born after the cutoff). It also implies that the remaining gaps in education are likely to be permanent.

4.2 Effects on Careers

Job Profiles at Childbirth To begin the analysis of careers, I study mothers' work at the time of childbirth, which we posit is likely to have an outsize influence on child outcomes in early life. Figure 3 studies the average impact of the cutoff on two self-reported measures of labor force attachment. The first, employment in the last year (shown in Panel A), captures the demonstrated propensity to work in the recent period. The second, having a career identity (shown in Panel B), is not tied to an individual's work history, and thus may capture up a more persistent form of attachment or intention to work. These measures are correlated, but there is significant variation in reported work status and career identity: 40% of women who *did not work* in the last year still identify as a worker, and 20% of women who *did work* in the last year instead identify as a stay-at-home mother.

The two panels of the figure show a remarkably consistent pattern of effects. The likelihood of working last year and the share with a career identity increases by 1.1 to 1.2 p.p. for women born before relative to after the cutoff, compared to baseline means around 74 percent and 57 percent for each of these outcomes, respectively (see Columns 1–2 of Table 4). I also find similar effects on employment when I use the administrative records, as I discuss below. These reduced-form effects are similar across early- and mid-career women (Columns 3–6 of Table 4), despite the larger

[16] Appendix Figure A.6 confirms that the changes in these education outcomes occur precisely at the cutoff.

impacts on education in early career. This suggests that the effects of education may become more important once women begin to pave permanent working identities.

Next, I turn to measures of women's career intensity and success, including the selectivity and social impact of jobs. Because of potential concerns about selection into work, I first examine effects on a range of unconditional, binary measures of job traits. These impacts will capture potential changes in the quality of positions among new entrants as well as job transitions among inframarginal workers. Throughout, I estimate effects separately for women in early- and mid-career.

Panel A of Figure 4 presents effects on the propensity to hold a professional job. In contrast to the effects on working, the cutoff has no significant effect on being a professional in early career. However, a clear positive impact emerges in mid-career. The point estimates indicate that women born just before rather than just after the cutoff are 1.3 p.p. more likely to hold jobs in this top tier of positions, relative to a baseline rate of 35.7 percent (see Column 1 of Table 5). The magnitude of this effect implies that the increase in professional jobs can account for the entirety of the overall rise in work.

As additional evidence on job selectivity, Column 2 of Table 5 shows effects on having a job that requires some post-secondary education. Based on the impacts on college attendance, these are the highest-skilled jobs that would be relevant for marginal students, particularly at mid-career. Similar to the impacts on professional jobs, these effects grow substantially across career stages. While women born just before the cutoff are 0.4 p.p. more likely to have a job that requires some college in early-career, this effect doubles to 0.8 p.p. in mid-career. In turn, this suggests that greater access to licensed work can explain 61 percent of the rise in professional careers (since all of the jobs that require a post-secondary credential are included in that class of jobs).

The following columns of Table 5 study effects on social impact, which is among the most valued job attributes for women (Kesternich and De Schouwer, 2023; Burbano et al., 2020). At baseline, I proxy for this with an indicator for being in a "high impact" profession; specifically, a preschool teacher – which is the job with the highest rating of social impact, among jobs that do not require

a college degree.[17,18] Columns 3 and 4 of Table 5 show that effects on these high impact jobs also increase over a career. The range of mid-career effects spans from 0.2 p.p., when I use the most narrow definition of preschool teachers, to 0.8 p.p., when I use a broader definition.

The next set of results shifts to consider how the cutoff affects upward mobility among *workers*, for whom a broader set of job quality measures are available. As mentioned above, the interpretation of these effects could potentially be complicated by changes in worker composition. For example, one might worry that the women who are induced to work at the cutoff may be less motivated and choose unusually low-quality jobs, which could lead to a downward bias in the effects. Contrary to this concern, selection-correcting the estimates has no effect on the conclusions (see Section 5).

The main finding is that job status among workers improves in mid-career, both in terms of occupational prestige and peer quality. Panel B of Figure 4 and Column 5 of Table 5 show that occupational prestige of women born just before the cutoff increases by around 0.27 points, or roughly 1 percent. Examining distributional effects, this appears driven by a shift into jobs in the top half of the prestige distribution (Column 6). Colleagues in one's profession are also higher-educated on average. Column 7 shows that women born before the cutoff have a greater share of workplace peers who have attended college. Exposure to better peer effects could also lead to a host of other, indirect benefits, such as greater productivity or career aspirations (Nix, 2020; Clark et al., 2009).

Are these improvements in job status accompanied by changes in working conditions? Appendix Figure A.7 presents mid-career effects on a range of other job amenities that have been shown to be valued by workers (Maestas et al., 2023). Women born just before the cutoff work in jobs that involve greater social impact, in line with the educator effects above; more team-based activities; and fewer self-driven tasks. To summarize the total value of these impacts on amenities, in Section 6.2, I monetize these effects using recent willingness-to-pay estimates. I do not find changes in work effort, as measured by weekly hours of work or weeks of work, but there is a small reduction in

[17] Based on the American Working Conditions Survey (Maestas et al., 2017), over 90% of women in the Census occupation "Preschool and Kindergarten Teachers" believe they have a positive impact most or all of the time. By comparison, 26% of licensed practical nurses and 81% of registered nurses believe this to be true.

[18] Preschool teachers are required to have a child development permit, which requires college-level course work in early childhood education combined with (i) extensive work experience or (ii) completion of an Associate's degree and field work. See `https://www.ctc.ca.gov/credentials/leaflets/child-development-permits-(cl-797)`.

scheduling flexibility declines at mid-career (see Appendix Table A.3).

Earnings over the Life Cycle Building on this evidence of upward mobility in women's jobs, I next study how being born before the cutoff affects earnings over the life cycle. Importantly, because I have a panel of earnings for each woman, I am able to estimate these impacts on income over the life cycle. This allows me to test whether the steeper career trajectories that I observe for women born before the cutoff generalizes to the years beyond childbirth. To ensure that the sample is balanced across ages, I focus on women born between 1977 and 1987, for whom I can fully observe earnings between ages 21–35.[19]

Figure 5 presents the annual impact of being born before the cutoff on women's average earnings (Panel A) and employment (Panel B) in early and mid-career. Each marker shows the estimated discontinuity from Equation 1 for a single age group (shown on the x-axis), with the 95-percent confidence interval indicated by the vertical bar. The results show strong gains in earnings and employment that increase steadily over women's careers. Women born before the cutoff earn roughly $400 more per year in early career and $800 more per year in mid career. They are also 0.5 p.p. more likely to be working in mid-career, with smaller and less-precisely-estimated impacts in early career. This suggests that increases in earnings reflect improvements along the intensive margin (e.g., jobs) and greater attachment to work.

Panel C of Figure 5 studies impacts on household earnings. These impacts could be smaller than the gains in individual earnings if maternal income substitutes for father's earnings, or larger if they are complements. The patterns show that household earnings rise by roughly as much as individual earnings, suggesting that there is little impact on spousal earnings.

To connect these life-cycle effects on earnings to the career gains at childbirth, Figure A.8 presents the discontinuity in average earnings and employment in the year prior to having a child. Both of these outcomes increase sharply for women born before the cutoff, with magnitudes in line with the mid-career effects in Figure 5 ($\approx$ 800). Hence, the occupational gains that I document at childbirth appear to translate into meaningful gains in earnings. Further, the fact that I find

[19] Appendix Table A.4 shows that the impacts of the cutoff on education and occupations for these cohorts is very similar to the whole sample, with nearly identical estimates for mid-career women.

similar effects on earnings over time provides suggestive evidence that the job and amenity gains that I find at childbirth may also persist throughout women's careers.

How much of this rise in earnings at the cutoff can be explained by improvements in professions? Using the average wages of occupations and the jobs reported at childbirth, I estimate that women born before the cutoff would be expected to earn $531 more per year in mid-career (see Appendix Table A.3). The rise in occupational earnings reflects the combined effects of greater employment and movement into jobs with higher hourly wages. These estimates suggest that better occupations can explain around two-thirds of the overall increase in earnings, with the remaining one-third explained by higher earnings *within* occupations.

4.3 Effects on Infant and Maternal Health

Turning to the outcomes of the next generation, Figure 6 presents a binned scatter of the log birth weight of children, ordered by their mother's birth date relative to the cutoff. The patterns in child outcomes mirror the effects on mothers' job status. When mothers are in early career, children on opposite sides of the cutoff have very similar birth weight (Panel A). Instead, when mothers are in mid-career, their children's outcomes diverge around the cutoff (Panel B). In particular, children just to the left of the cutoff experience a 0.4 percent improvement in child birth weight (column 1 of Table 6). This produces a clear drop in the otherwise positive trajectory of child weight.

Infant health improves similarly based on other markers in mid-career. Columns 2 and 3 of Table 6 show that the children to the left of the cutoff are 9 grams heavier ($p < 0.05$), and 0.5 days older ($p < 0.10$) at birth. These impacts do not appear in early career, as before. This growth in child benefits could potentially reflect greater resources or amenities from maternal career advancements or greater adoption of healthy behaviors as mothers mature.

Appendix Figure A.9 presents distributional effects on birth weight around the cutoff. It shows that improvements are present throughout the distribution, including a reduction in the likelihood of being in the lower tail, between 1500 and 2000 grams, and an increase in the likelihood of being around median, between 3,000 and 3,500 grams. There is no effect on being very high birth weight, above 4000 grams, consistent with the interpretation that these are health gains.

The remaining columns of Table 6 show that prenatal *inputs* also expand in mid-career. Mothers born before the cutoff are 0.5 p.p. less likely to smoke, a large reduction relative to the 4 percent at baseline. They up to 1.1 p.p. less likely to participate in means-tested programs, including public health insurance (Medi-Cal) and nutritional assistance (WIC), consistent with a rise in income. I do not find any effect on the reported number of prenatal visits, although this may reflect the high mean at baseline (12.3 visits). The fact that benefits accrue across multiple domains – maternal health capital, financial resources, and insurance – is indicative of a large shift in the quality of the prenatal environment.

Putting the effects in perspective, the gains in birth weight are sizable, but appear to be proportional to the effects on inputs. For example, greater earnings in the year of birth alone can explain 5.1 grams (57%) of the increase in birth weight, based on a 6.39 gram effect per $1,000 of income (Hoynes et al., 2015). Moreover, these benefits for child health could be compounded if earnings growth is sustained over several years, which could enhance these effects through the accumulation of financial resources or investment in complements to health, such as better housing. As another benchmark, reductions in smoking can explain around 0.9 grams (9.8%) of the improvement in health, based on the effect of smoking on birth weight (15 grams per cigarette per day) and the average number of cigarettes per day among smokers in the quarter prior to pregnancy (12) (Permutt and Hebel, 1989). As with the effects on earnings, the impact of declines in smoking could also be magnified if reductions in smoking persist over a longer period, or there are declines in other risky behaviors around pregnancy.

5 Robustness

In this section, I address potential threats to the interpretation of the results and verify the robustness of estimates. The exercises focus on effects estimated at mid-career in the birth records, based on the stronger patterns that emerge at that phase of women's lives. The results for earnings outcomes are in progress.

First, a potential concern with the comparison of treatment effects between early- and mid-career is that the *types* of women that I observe at these stages may be different. Thus, the difference

in treatment effects across career stages could reflect distinct LATE's across women, rather than changes in the treatment effect over a career. To rule this out, I rerun the analysis using a balanced panel of women, for whom I observe outcomes exactly twice.[20] If the larger treatment effects at mid-career are driven by selection, then restricting to this balanced sample should lead me to find constant impacts across different ages (births) of the same mother. Contrary to this, Appendix Table A.7 shows that treatment effects increase when the *same* mother is older. This suggests that the growth in impacts across career stages is most likely explained by age effects, rather than selection.

Note that this exercise following mothers across births in Appendix Table A.7 is also informative about the effects of the cutoff on the child penalty, since the outcomes reported at a first and second birth refer to periods before and after having children, respectively. In particular, the change between the impacts on work for a second and first child can be interpreted as the difference in the child penalty between women born just-before and just-after the cutoff. The difference in these effects is positive, which is consistent with schooling attenuating the child penalty.

Second, I test the sensitivity of the results to using alternative estimation windows around the cutoff, recent advances in inference methods, or bounding approaches to selection. The effects are similar when I use the mean-squared-error optimal bandwidth for each outcome, which effectively balances the bias-variance trade-off for the RD estimator (Calonico et al., 2014), or apply any bandwidth within the range of selected bandwidths (see Appendix Table A.8 and Figure A.10). The findings are also unaffected when I estimate Eicker-Huber-White robust standard errors to address possible under-coverage of clustered standard errors (Kolesár and Rothe, 2018), bootstrap standard errors for the two-step estimation of years of education, or bias-correct the estimates and confidence intervals to account for potentially "large" bandwidth selection (Calonico et al., 2014). Trimming the sample to account for potential selection into work similarly does not change the conclusions. See Appendix Tables A.8 and A.9 for details.

Third, I relax the sample selection criteria related to race and age at first birth. Including

[20] This includes mothers that have both a first and second birth between 2007-2017, for working and schooling outcomes, or between 1989-2017, for infant outcomes. I limit mothers with at least a two-year spacing between births in order to create a gap in the timing of outcomes.

Black women slightly reduces the "first stage" on education, but the effects on other outcomes are qualitatively similar (see Appendix Table A.8). Appendix Figure A.11 presents the effects using alternative cutoffs for age at first birth in the sample. Impacts on all outcomes tend to be larger for younger first-time mothers (consistent with these women being more marginal to school policies), but the findings are not materially changed when I include older mothers.

Finally, I test whether the results could have arisen by chance by re-running the results using placebo cutoffs. The placebo cutoffs are chosen within an eight-week window around the cutoff, to capture possible seasonality around this period, and spaced apart by two-weeks. Across 48 placebo estimates, only two are statistically significant, and both of these go in the opposite direction of the main estimates (Appendix Figure A.12). Outside of these exceptions, the effects are typically small, inconsistently-signed, and uncorrelated across outcomes. This makes it is highly improbable that the systematic improvements in outcomes in the main results could reflect statistical noise.

6 Discussion

The results thus far show substantial reduced-form effects of the entry cutoff across generations. As context for these findings, in this section I provide evidence on the role of education in generating labor market gains, and the implied return to schooling. I also study the evolution of the intergenerational effects of education over time and discuss implications for future child mobility.

6.1 Evidence on Mechanisms

As discussed in Section 1.2, the main potential mechanisms for career improvements are *more* education (the schooling channel) and *earlier* education (the age-for-grade channel). To distinguish between these possible channels, I examine how closely the impacts of the cutoff on required job skills match up with the effects on education. In particular, I focus on outcomes at mid-career, and compare the impacts on jobs that require *exactly* some college – the margin of education for which I find the largest effects – to the effects on jobs that require lower or higher levels of education. As explained earlier, if greater education is the primary channel, one should expect a large impact on jobs that require exactly some college, and little impact on jobs that require other credentials.

In strong support of the education channel, I only find increases in jobs that require exactly some college. Appendix Table A.5 presents the effects on jobs that require other credentials. I find a precisely estimated zero effect on jobs that require a college degree. This includes a null effect on teaching roles within these college jobs, unlike the strong effects on preschool educators shown earlier. I similarly do not detect any effects on less-selective jobs, which only require an exam (no minimum education). It is challenging to reconcile this pattern with other mechanisms, such as younger age-for-grade, which would be expected to deliver broader-based improvements.

In line with this, I do not find evidence that the effects on careers are mediated by changes in partner selection. Appendix Table A.6 shows that women born before the cutoff are neither more likely to have any partner nor more likely to have a successful partner, as measured by completed education, employment, or professional status. This is consistent with evidence that women with some college tend to choose partners from the same pool as lower-educated women (Lundberg et al., 2016). The one responsive trait is partner age, consistent with matching taking place within a school cohort. This alone is very unlikely to explain the results for other outcomes, however.

6.2 Implied Labor Market Return to Education

Assuming that education is the *only* source of improvements at the cutoff, a useful exercise is to calculate the implied causal return to an additional year of schooling. This allows me to compare the effects of education across settings and to quantify the extent to which the raw correlations between education and careers reflect these causal effects. To calculate this return, I divide the reduced form impacts of the cutoff on career outcomes by the estimated effect of the cutoff on years of education.[21] I focus on the impacts at mid-career to capture the later-life benefits of schooling.

Employment The results indicate that one additional year of education raises the likelihood of working by between between 10 p.p. and 16 p.p. in mid-career (depending on whether I rely on the estimates from the birth or Census records). Comparing to other employment policies for young mothers, this suggests that greater education is a highly effective intervention (Olivetti and Petrongolo, 2017). For example, this return is 2 to 3 times as large as the much-noted effect of

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

the mid-1990's expansion of the Earned Income Tax Credit on young, single mothers (Kuka and Shenhav, 2020; Michelmore and Pilkauskas, forthcoming). At the same time, these effects resemble the career-altering impacts of high-demand vocational degrees. For example, Grosz (2020) finds that winning a lottery for a nursing associate's degree program (roughly one year of additional education) increases employment in the health industry by 19.5 p.p., although these effects are not statistically significant, and Stevens et al. (2019) show that vocational degrees raise employment by 2 to 15 p.p. This suggests that the employment return for mothers is likely similar, or perhaps slightly larger, than the impacts of the most influential AA degrees.

As another benchmark, the estimated return to schooling can explain the large majority, if not all, of the disparities in work across education groups (shown in Figure 1). In particular, the estimated 10 to 16 p.p. increase per year can explain at least 70% of the gap in work between women with some college and dropouts, based on a three-year difference in education. However, the effects on employment are imprecisely estimated, so I also can not rule out a more moderate role for education (e.g., that the causal effects explain 40% of the gaps).

Job amenities For job amenities, I perform an additional step to convert the calculated return (i.e., the change in working conditions per year of education) into a money metric. This allows me to easily aggregate impacts across amenities into the total benefits for workers, and to compare this to the value of improvements in wages. This conversion is possible because of the recent elicitation of women's willingness-to-pay (WTP) for twelve job amenities in Maestas et al. (2023), combined with new survey data with measures of the same amenities across occupations.[22] The WTP estimates are expressed as a percent of hourly wages. To monetize the return for each amenity, I multiply the change in the amenity per year of education (calculated using the RD estimates shown in Appendix Figure A.7) by the corresponding WTP estimate.

Appendix Figure A.14 presents the return to an additional year of education – as a percent of wages – across these amenities. In total, the improvements across amenities are equivalent to a 3.3% annual increase in wages ($p < 0.06$). This reflects an increase in value to workers from

[22] Job-level amenities are calculated using the American Working Conditions Survey (Maestas et al., 2017), which is the basis for the experimental survey in Maestas et al. (2023). See Appendix A.2 for details.

greater societal impact, less strenuous physical demands, and team-based work, net of the costs of less self-directed work. The 3.3% return is roughly equivalent to one-third of the direct wage return to community college, based on past estimates that hover around 10% (Jacobson et al., 2005; Mountjoy, 2022), and for mothers is equivalent to 6 percent of the rise in individual total earnings.[23] However, it is worth noting that this does not include improvements in valuable working conditions outside the WTP set, such as co-worker education, which could further raise the amenity-inclusive return (Jencks et al., 1988).

6.3 Intergenerational Transmission Over Time

Finally, as context for the effects on infant health, I examine whether these gains for children are *new* benefits of maternal education, or a continuation of well-established patterns. This is relevant because a growing literature establishes that gains in infant health are likely to improve long-run outcomes, and thus it is relevant to understand how these gains in birth weight are likely to affect the long-run gains from maternal education (Black et al., 2007; Royer, 2009; Bharadwaj et al., 2017). I study this question by estimating the RD separately for moving windows of nearly thirty cohorts of children (beginning in 1989, when maternal education was added as a field).

Appendix Figure A.15 shows a clear increase in the birth weight RD over time. While cohorts from the mid-2000's onward experience increases of 8-9 grams in birth weight, estimates that include the 1990's cohorts are small and statistically insignificant. In contrast, the estimated "first stage" on completed education is relatively constant over time. This suggests that the *transmission* of education to children is increasing over time. This could, in turn, imply a tighter link between education investments and upward mobility going forward.

7 Conclusion

This paper documents the multigenerational effects of increasing women's education. The results show that attending college substantially increases women's employment and earnings, elevates

[23] Specifically, rescaling the $800 increase in earnings at the cutoff by the mean of $31,590 and the increase in years of schooling implies a 52% earnings return per year of education.

women along the job ladder, and transmits to a greater well-being of the next generation. These effects are stronger at mid-career than early-career, consistent with a growing role for education over the life cycle. Back-of-the-envelope estimates suggest that the improvement in job amenities from one additional year of education is equivalent to a 3.3% increase in annual earnings. Thus, these benefits which have not been previously quantified meaningfully raise the labor market benefits of education. Overall, the findings suggest that investments in women's college attendance may be one of the most effective policies to improve maternal careers, with important spillovers to the health and mobility of the next generation.

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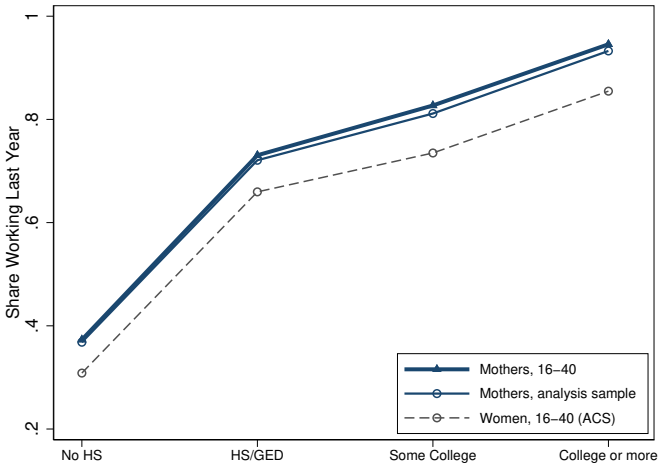
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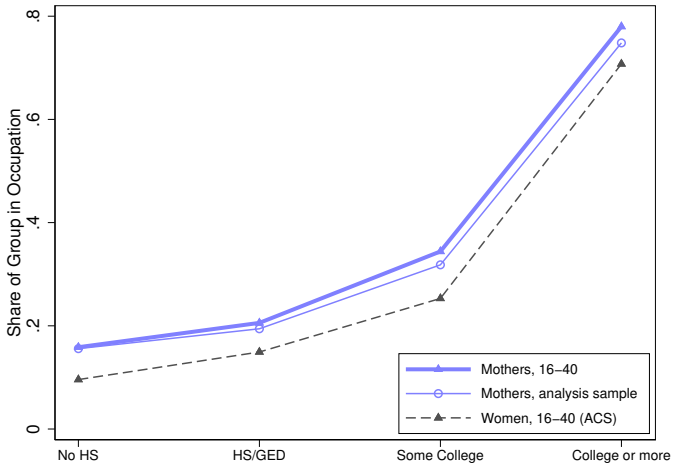
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Figure 1: Descriptive Links between Women's Education, Careers, and Child Health

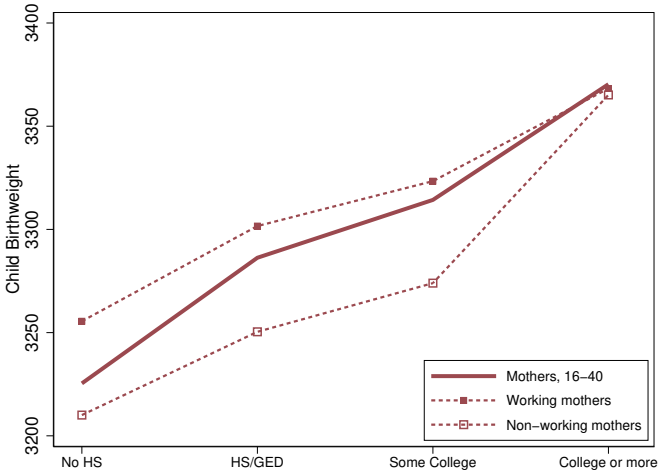
(a) Worked last year



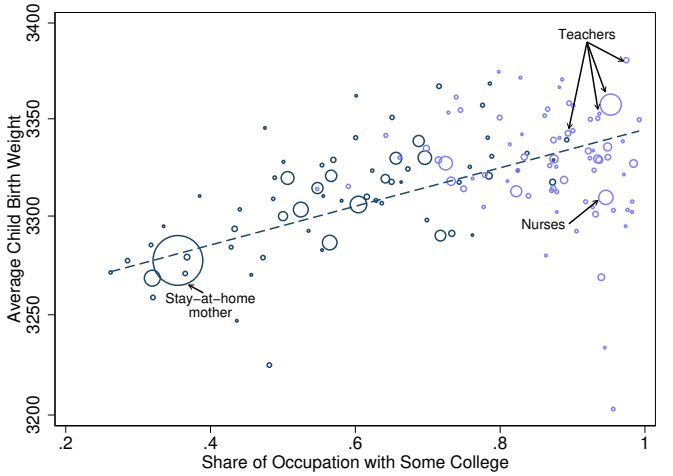
(b) Professional jobs



(c) Child birth weight by maternal employment



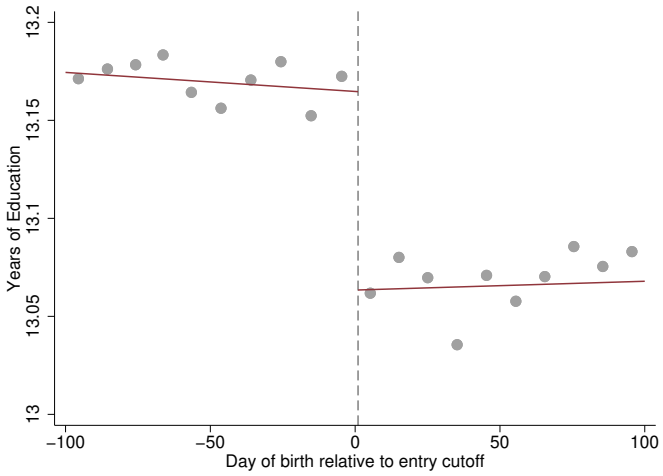
(d) Child birth weight by maternal occupation



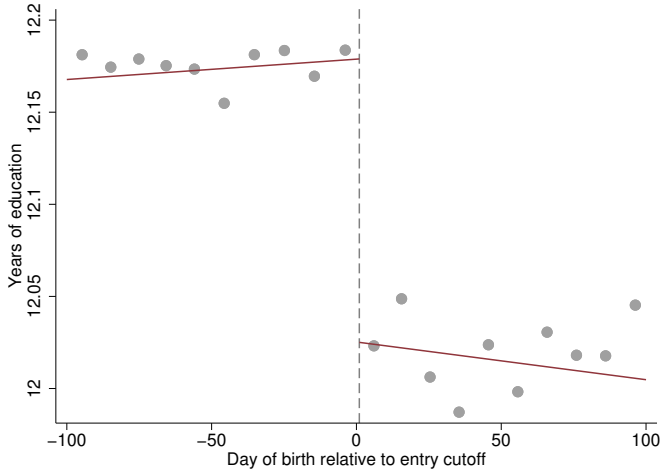
Notes: This figure presents the average labor market and child outcomes for women grouped by completed level of education: no high school diploma (No HS), high school diploma or GED only (HS/GED), some college attendance only, college degree or more. Panels A and B show the share of women who worked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

Figure 2: Increase in Completed Education at School Entry Cutoff

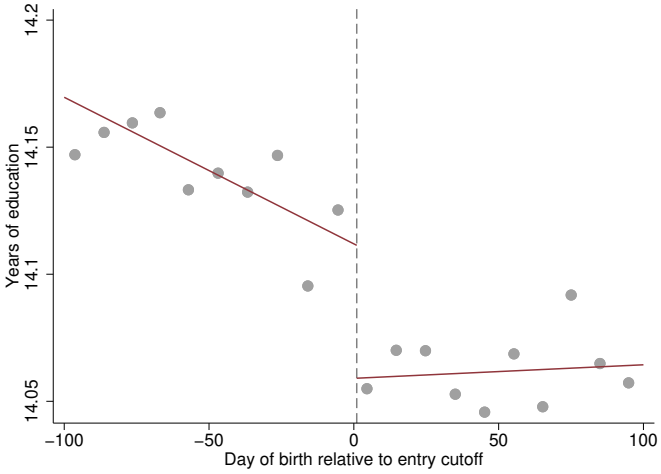
(a) All



(b) Younger than 25

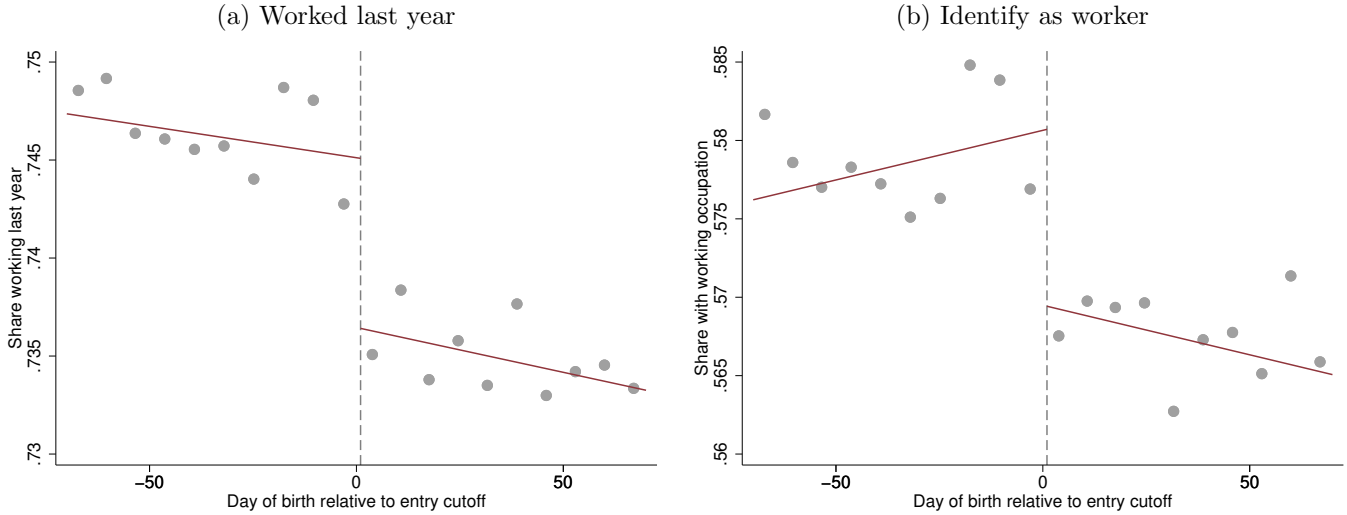


(c) Age 25 or older



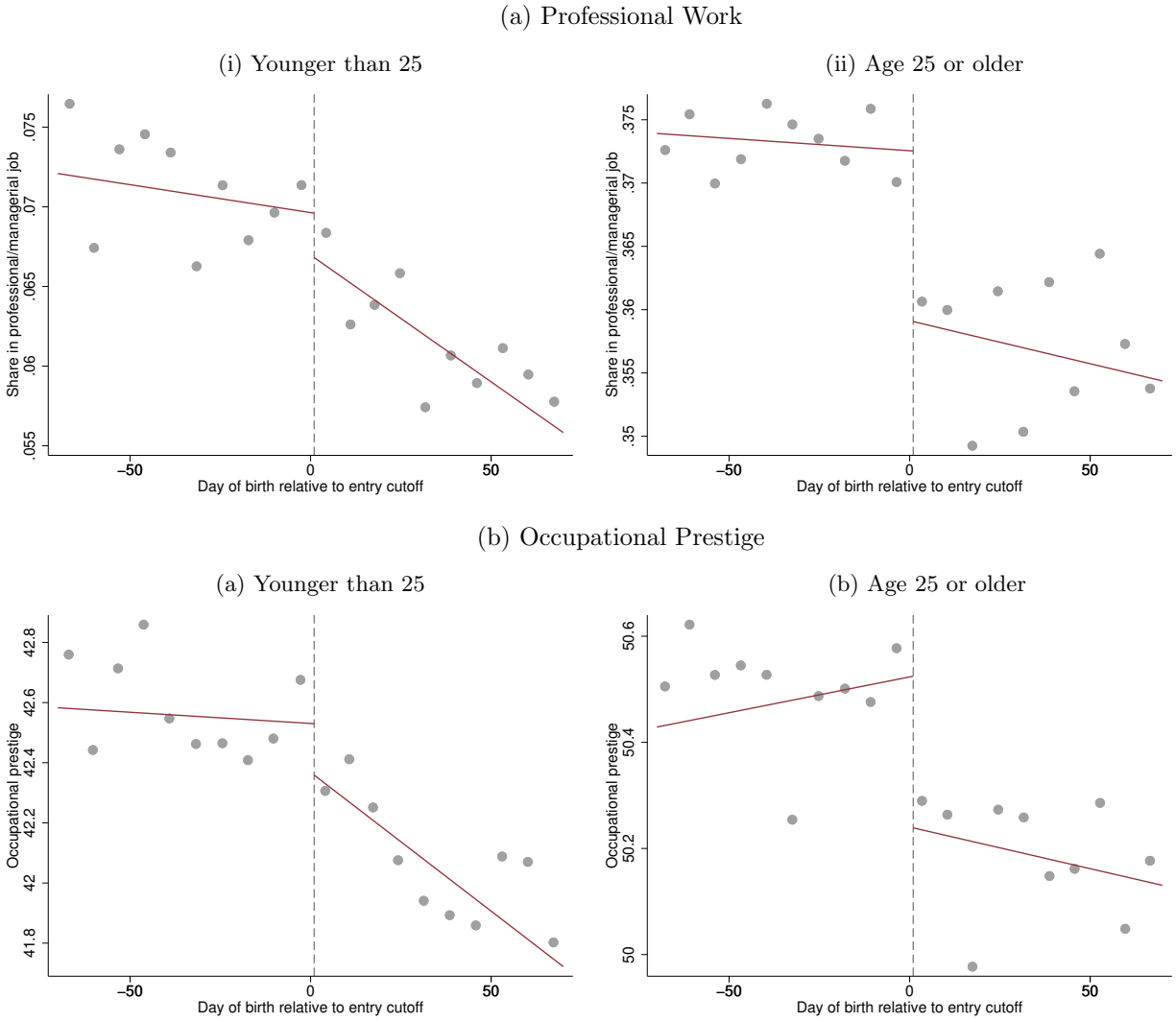
Notes: This figure presents the average years of education overall (panel a); among early-career women (panel b) or among mid-career women (panel c) for women born within 100 days of the school entry cutoff. Outcomes are residualized of (recentered) year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Source: individual birth records from California for births between 2007–2017.

Figure 3: Rise in Propensity to Work and Career Identity (Birth records)



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

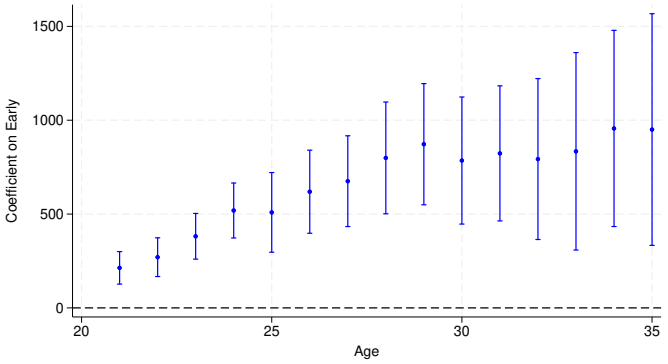
Figure 4: Greater Professional Work and Occupational Prestige at Mid-Career (Birth records)



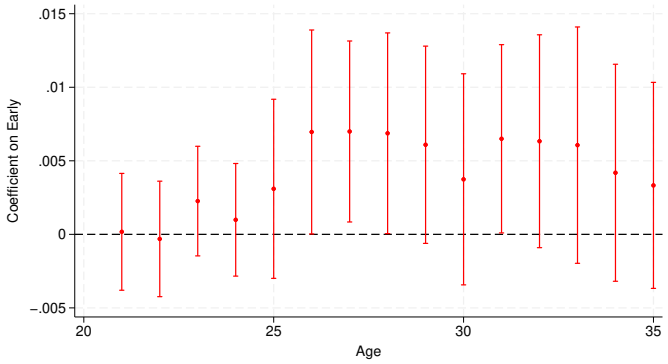
Notes: This figure presents the likelihood of having a professional occupation (panel a) and the average occupational prestige (panel b) for women born around the school entry cutoff. Subpanel (i) shows outcomes in early-career and subpanel (ii) shows outcomes in mid-career. Outcomes are residualized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

Figure 5: Annual Improvements in Individual and Household Earnings (Census records)

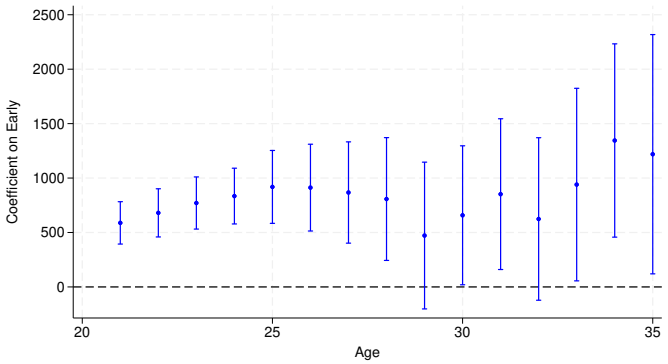
(a) Earnings



(b) Worked last year



(c) Household earnings

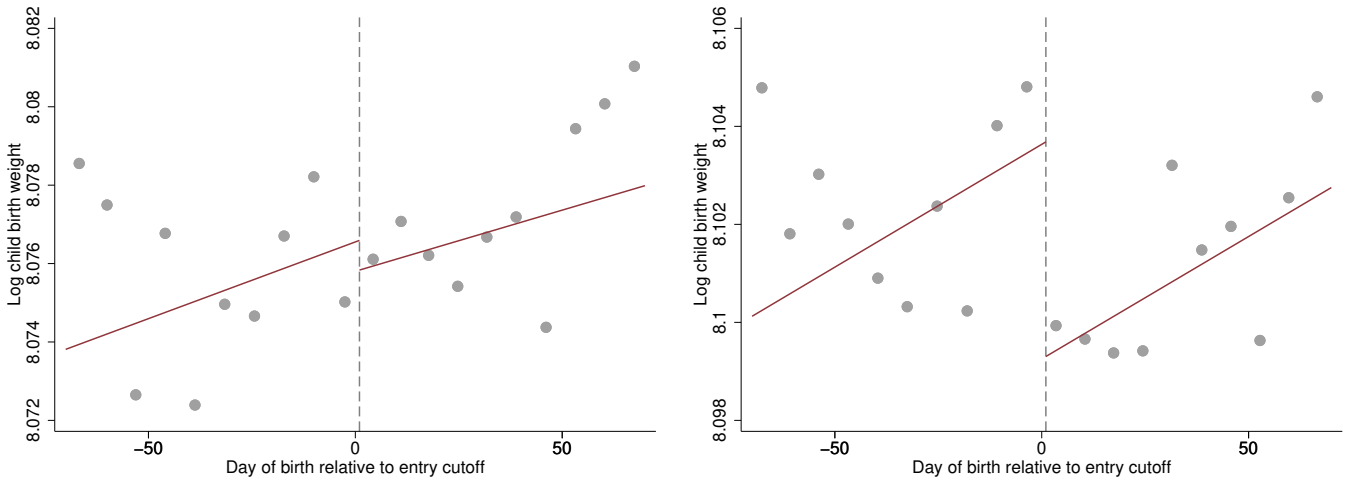


Notes: This figure presents the estimated effect of being born just prior to the school entry cutoff on average earnings (Panel a) and the likelihood of working (Panel b) at every age between 25–35. Sample is restricted to women born between 1977–1987 so the panel is balanced across ages. Source: Census administrative earnings records. See text for details.

Figure 6: Intergenerational Effects on Infant Health Grow over Time

(a) Younger than 25

(b) Age 25 or older



Notes: This figure presents the average birth weight for children born in a mother's early-career (left panel) or mid-career (right panel) for mothers born around the school entry cutoff. Outcomes are residualized of (recentered) year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff.

Table 1: Descriptive Statistics

	Mothers, 1^{st} or 2^{nd} child		Estimation sample	
	All	CA natives	First child by 31	70 day window, ages 16–40
Black	0.055	0.081	0.090	0.089
	(0.228)	(0.273)	(0.286)	(0.285)
White, non-Hispanic	0.316	0.376	0.332	0.326
	(0.465)	(0.484)	(0.471)	(0.469)
Hispanic	0.454	0.480	0.527	0.532
	(0.498)	(0.500)	(0.499)	(0.499)
Asian	0.132	0.034	0.023	0.024
	(0.338)	(0.181)	(0.151)	(0.153)
High school graduate	0.845	0.874	0.850	0.850
	(0.362)	(0.332)	(0.357)	(0.357)
College graduate	0.334	0.259	0.171	0.169
	(0.472)	(0.438)	(0.377)	(0.375)
Age	27.765	26.234	24.258	24.286
	(6.239)	(6.034)	(4.679)	(4.621)
Year of birth	1,983.603	1,985.292	1,987.159	1,986.943
	(6.681)	(6.468)	(5.285)	(5.090)
Employed last year	0.776	0.805	0.773	0.769
	(0.417)	(0.396)	(0.419)	(0.422)
In school	0.056	0.077	0.093	0.094
	(0.230)	(0.267)	(0.291)	(0.292)
Infant birth weight (g)	3,286.217	3,297.666	3,298.374	3,300.700
	(556.8)	(567.0)	(557.5)	(556.1)
First childbirth	0.554	0.583	0.596	0.571
	(0.497)	(0.493)	(0.491)	(0.495)
Second childbirth	0.446	0.417	0.404	0.429
	(0.497)	(0.493)	(0.491)	(0.495)
Observations	3,931,690	2,006,194	1,571,924	516,266

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 the sample to women born in California; Column 3 further limits to women with a first birth by age 31; and Column 4 focuses on women born within seventy days of the school entry cutoff and who are between the ages of 16 and 40. The sample includes women with a birth in California between the years 2007–2017.

Table 2: No Selection into Motherhood Around Cutoff

	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 prior to the school entry cutoff on the likelihood of being observed having any children (column 1), having a first child (column 2), or having 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 date-relative-to-the-cutoff, while the outcomes in columns 4–6 are maternal ages at the individual level. Estimation is performed separately for women who are White, Hispanic, Black and Other (Panels A through D, respectively). The sample includes California native women who have a first birth by age 31 between the years 2007–2017. 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.

Table 3: Discontinuity in Educational Attainment and School Attendance

	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 prior to the school entry cutoff on education outcomes on average (panel a), in early-career (panel b) or in mid-career (panel c). Each column provides results for a different outcome: imputed years of education (column 1), the likelihood of completing high school (column 2); the likelihood of completing some college (column 3); the likelihood of graduating college (column 4); and current school attendance (column 5). The sample includes non-Black California native women who have a first birth by age 31 between the years 2007–2017. 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.

Table 4: Discontinuity in Employment and Career Identity over a Career (Birth records)

	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 prior to the school entry cutoff on the likelihood of working in the previous year and of identifying as a worker (having a "career identity"). Columns 1–2 present average impacts; columns 3–4 present impacts in early career; and columns 4–5 present impacts in mid-career. See Table 3 for details about the sample and estimation.

Table 5: Discontinuity in Job Ranking, Social Impact, and Prestige Over a Career (Birth records)

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

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

Table 6: Discontinuity in Infant Outcomes and the Pregnancy Environment

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

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

ONLINE APPENDIX:

Multigenerational Benefits of Women's Education

Na'ama Shenhav

January 31, 2025

A Data appendix

A.1 Assignment of Occupation Codes in the Birth Records

This section described the process of converting parents' occupation strings to 2010 Census occupation codes. Appendix Table A.1 summarizes the output for maternal occupations from this process. Match rates to occupation codes are similar for fathers.

I assign 2010 occupation codes in three steps. First, I parse strings with multiple jobs into the component occupations (0.5% of women). This produces a list of 19,310 unique maternal occupations, 13,967 of which contained meaningful information about occupations.

Second, I together with a team of research assistants attempted to map each occupation string to an occupation code or as "not in the labor force" (NILF). This is the "cleaned occupation" for each woman. To be as consistent as possible with Census classifications, we relied on the "Alphabetical Indexes of Occupations", which includes a comprehensive list of specific occupations and the corresponding occupation code (U.S. Census Bureau, 2019). For strings that encompassed multiple more-granular occupations (e.g. "Teacher") we assigned multiple occupation codes corresponding to the occupations in group.[24] This yields a cleaned occupation for 88% of women.

Third, we use information on parental industry combined with occupation to disambiguate additional strings. For example, individuals who report their occupation to be "Civil" and industry to be "Engineering" are assigned the occupation "Civil Engineer" (OCC 1360) in this step. This yields an occupation code for an additional 7% of women.

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

Table A.1: Assignment of Census Occupation Codes

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

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

[24] 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).

A.2 Data Sources for Job Characteristics

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

Occupational prestige: To measure occupational prestige, I take the simple average of ratings from two large-scale surveys. The first rating comes from a survey of 3,113 workers on Amazon's Mechanical Turk in 2016 (Hughes et al., 2022). Respondents were asked to rate 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 a job description that describes the job 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 were asked to rank jobs (covering all occupations in the 2010 Census) on a scale of 1 to 9 along an "occupational prestige ladder," with "9" representing an occupation with the highest possible social standing. I convert the GSS rating to a 100 point scale to make the ratings comparable across surveys.

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

Hourly wages: I 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 the weekly earnings divided by the usual hours per week. Wages are updated to 2017 dollars using the CPI from the Bureau of Labor Statistics. The sample consists of women between the ages of 25 and 49 in California (Flood et al., 2020).

Annual earnings and weeks of work: I 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 2017 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: I 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 of the paper for estimates). These include whether a job allows a worker to set her own schedule; telecommute; perform moderate physical activity (rather than heavy physical activity); sit (rather than perform heavy activity); choose how to do work; have 10 days paid time off (instead of none); have 20 days paid time off (instead of none); be evaluated on your own work within a team; work alone; have training opportunities; and have frequent opportunities for social impact (instead of occasional opportunities). The working conditions that were manipulated in the experimented 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, I use the AWCS to calculate analogous measures of these amenities for each Census occupation. Note that only 1,914 respondents in the AWCS report an occupation, so occupation-level characteristics (for the over 216 reported occupations) are likely measured with some noise.

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

Table A.2: No Selection into Motherhood within Early- and Mid-career Women

	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 prior to the school entry cutoff on the likelihood of being observed having any children, maternal traits, and grandparental traits, estimated separately by whether a woman is in early-career (panel a) or mid-career (panel b). The outcome in columns 1 is a fertility rate for cells defined by year-of-maternal-birth by date-relative-to-the-cutoff. The outcomes in the remaining columns are at the individual level, and include maternal age (column 2), an indicator for being White, non-Hispanic (column 3), an indicator for being Hispanic (column 4), an indicator for being Asian (column 5), the age of the maternal grandmother at the time of the mother's birth (column 6) and the age of the maternal grandfather at the time of the mother's birth (column 7). The sample includes non-Black California native women who have a first birth by age 31 between the years 2007–2017. 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.

Table A.3: Discontinuity in Occupation-level Earnings and Work Schedule

	Wage and salary earnings			Work schedule		
	Real annual wage (incl 0's)	Log real hourly wage	Log real annual wage	Usual weekly hours	Inflexibility	Weeks last year
A. Younger than 25						
Born before cutoff	400.974**	0.010*	0.010	-0.094*	0.001	0.071
	(165.169)	(0.005)	(0.007)	(0.049)	(0.002)	(0.073)
Mean above cutoff	11626.394	2.740	10.068	34.640	0.732	44.179
Individuals	205,815	80,703	81,216	80,835	82,197	81,303
B. Age 25 or older						
Born before cutoff	544.679**	0.007**	0.003	-0.031	0.002**	0.022
	(264.397)	(0.003)	(0.005)	(0.057)	(0.001)	(0.043)
Includes non-workers	Yes	No	No	No	No	No
Mean above cutoff	33877.764	3.059	10.438	35.089	0.772	45.854
Individuals	207,329	153,786	154,180	154,001	156,175	154,389

Notes: This table presents the estimated effect of being born just prior to the school entry cutoff on women's expected annual earnings and work schedule, in early-career (panel a) and mid-career (panel b). Each column provides results for a different outcome: real annual earnings, inclusive of 0's (column 1); log real hourly wage (column 2); log real annual earnings (column 3); usual weekly hours (column 4); work inflexibility (column 5); and weeks worked last year (column 6). The labor market outcomes in columns 1–5 and 6 are occupation-specific averages for workers in California calculated from the 2007–2017 Current Population Survey. Work inflexibility is taken from Cubas et al. (2019), and is defined as 1 minus the typical share of hours worked during between 8 am and 5 pm for each occupation. See Table 3 for details about the sample and estimation.

Table A.4: Discontinuity in Education and Careers for Cohorts in Earnings Panel Analysis

	Younger than 25		Age 25 or older	
	All	Born 1977-87	All	Born 1977-87
Years of education	0.158***	0.087***	0.049***	0.047***
	(0.013)	(0.027)	(0.017)	(0.017)
Share with any college	0.029***	0.017*	0.012***	0.012***
	(0.004)	(0.009)	(0.005)	(0.004)
Share with working occupation	0.012***	0.001	0.013***	0.014***
	(0.004)	(0.011)	(0.004)	(0.003)
Share working last year	0.014***	-0.007	0.008**	0.008***
	(0.004)	(0.010)	(0.003)	(0.003)
Professional	0.003	-0.007	0.013***	0.013***
	(0.002)	(0.008)	(0.004)	(0.004)
Occupational prestige	0.254	-0.415	0.267**	0.197*
	(0.169)	(0.380)	(0.105)	(0.106)
Obs	80889	23222	150704	125034

Notes: This table presents the estimated effect of being born just prior to the school entry cutoff on education and work outcomes for the baseline sample and for women included in earnings panel analysis (born between 1977-1987). Columns (1) and (2) show the early career estimates for the whole sample and the earnings analysis sample, respectively; while Columns (3) and (4) show the mid career estimates for the whole sample and the earnings analysis sample, respectively. Each row presents results for a separate regression for a different outcome. See Table 3 for details about the sample and estimation.

Table A.5: Testing for the Role of Non-Education-Based Channels

	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 prior to the school entry cutoff on the likelihood of having an occupation that requires a college degree. Each column provides results for a different outcome: having a job that requires a college degree (column 1); being a teacher for a kindergarten class or higher grade (column 2); being a kindergarten teacher (column 3); being an elementary or secondary teacher (column 4); and being a post-secondary teacher (column 5); having a job that requires an exam only (column 6). See Table 3 for details about the sample and estimation.

Table A.6: No Discontinuity in Partner Characteristics at Mid-Career

	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 prior to the school entry cutoff on the characteristics of women's partners in mid-career. Partner characteristics include the likelihood of being present 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 Table 3 for details about the sample and estimation.

Table A.7: 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 prior to the school entry cutoff using a balanced panel: women for whom I observe both a first and second birth. The sample is limited to mothers with at least a two-year spacing between births in order to create a meaningful gap between the timing of outcomes. Each column provides results for a different outcome: imputed years of education (column 1), the likelihood of working in the previous year (column 2), the likelihood of 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). For maternal outcomes, the sample includes the years 2007–2017; while for child outcomes, the sample includes the years 1989-2017. See Table 3 for further details about the sample and estimation.

Table A.8: Robustness 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)

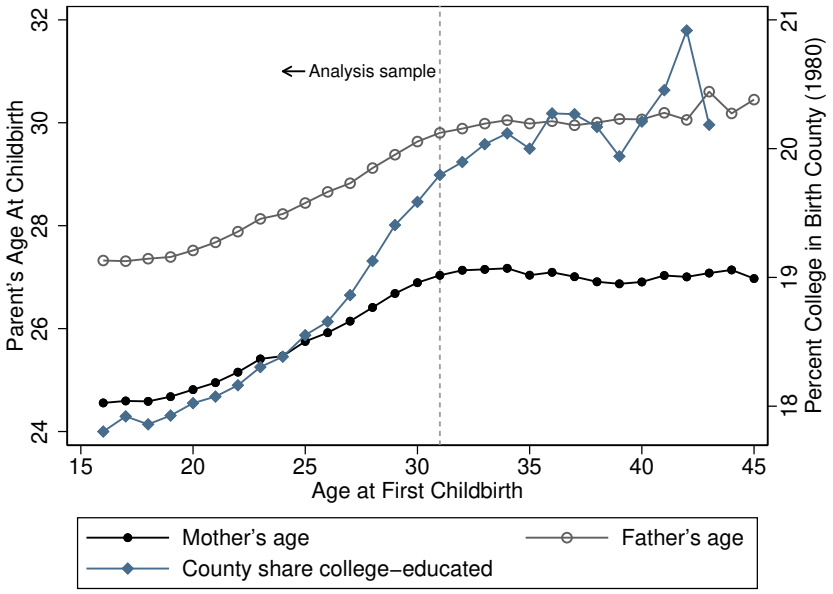
Notes: This table presents the estimated effect of being born just prior to the school entry cutoff using alternative estimation methods and samples. Each column presents results for a different modification to the baseline specification: using the mean-squared-error optimal bandwidth for each outcome,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

Table A.9: Bounds on Mid-Career Estimates, Taking into Account Potential Selection into Work

	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 the estimated bounds on the effect of being born just prior to the school entry cutoff on working outcomes, taking into account selection (Lee, 2009) bounds. Column 1 presents the baseline estimates. Column 2 presents the lower bound, in which I drop the top 1.9% of outcomes of non-college-educated women born before the cutoff. Column 3 presents the upper bound, in which I drop the bottom 1.9% of outcomes of non-college-educated women born before the cutoff. Each row presents outcomes for a different outcome that conditions on having an occupation. See Table 3 for details about the sample and estimation.

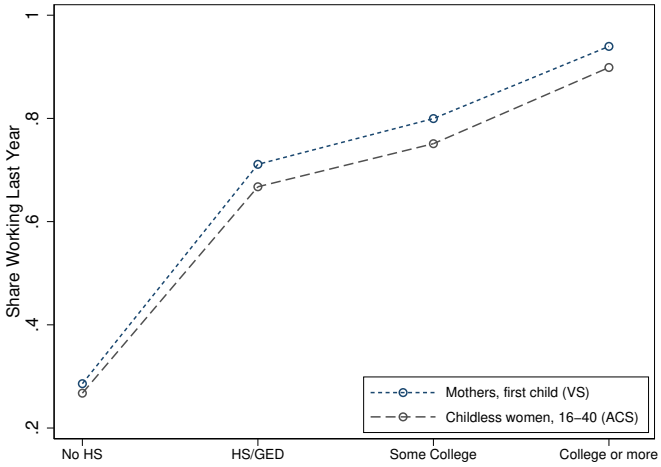
Figure A.1: Background Characteristics by Age at First Childbirth



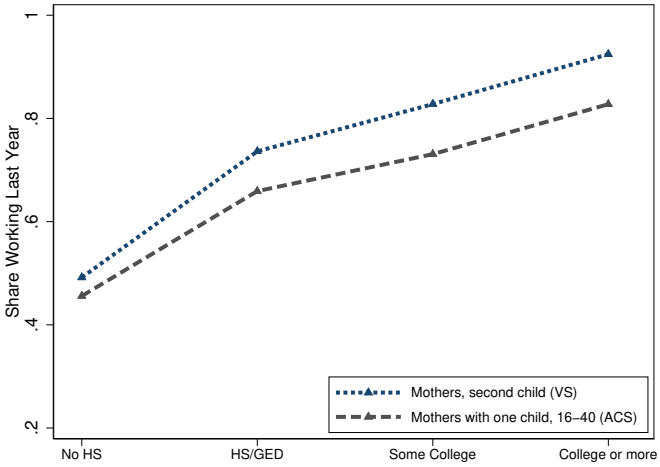
Notes: This figure presents the average background characteristics of women by age of first childbirth. Background characteristics include mother's age at childbirth (left y-axis), father's age at childbirth (left y-axis), and the share college-educated in one's county of birth. Share college-educated calculated from counts based on the 1980 Census, accessed through the IPUMS National Historical Geographic Information System (Manson et al., 2022). Mothers sample includes all women who have a first birth in California between the years 2007–2017.

Figure A.2: Likelihood of Working by Education Among Mothers and Non-mothers

(a) No children



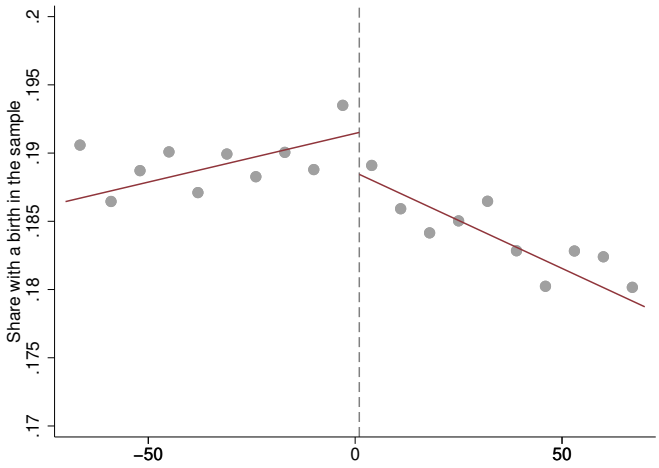
(b) Young children



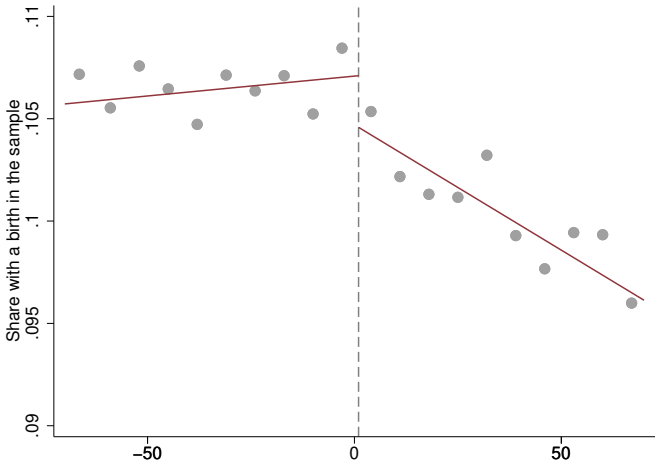
Notes: This figure presents the share of women working in the previous year by bins of completed level of education: no high school diploma (No HS), high school diploma or GED only (HS/GED), some college attendance only, college degree or more. Panel (a) presents patterns for first-time mothers in the Vital Statistics birth records and for childless women in the ACS. Panel (b) presents patterns for second-time mothers in the Vital Statistics birth records and for mothers with one child in the ACS. Vital Statistics sample includes all women who have a first or second birth in California between the years 2007–2017; ACS estimates use survey years 2007 to 2017.

Figure A.3: No Discontinuity in the Likelihood of Appearing in the Sample Across the Cutoff

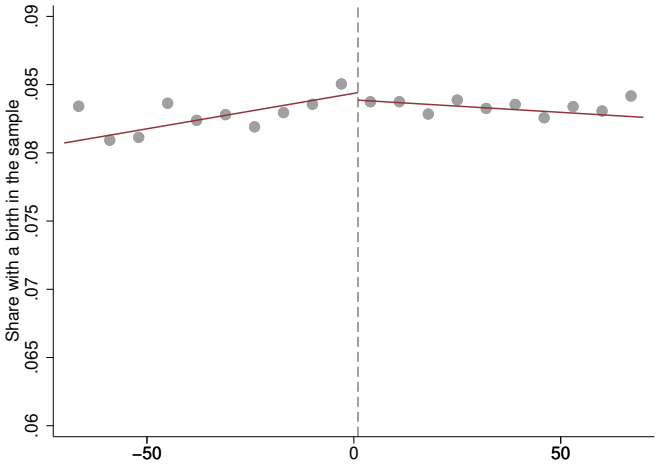
(a) All



(b) Younger than 25



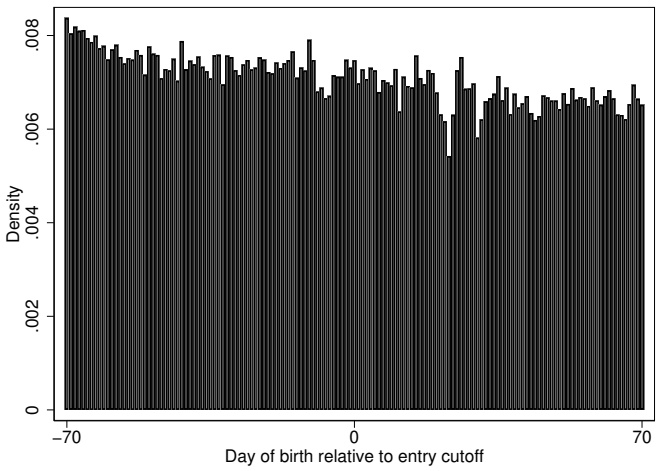
(c) Age 25 or older



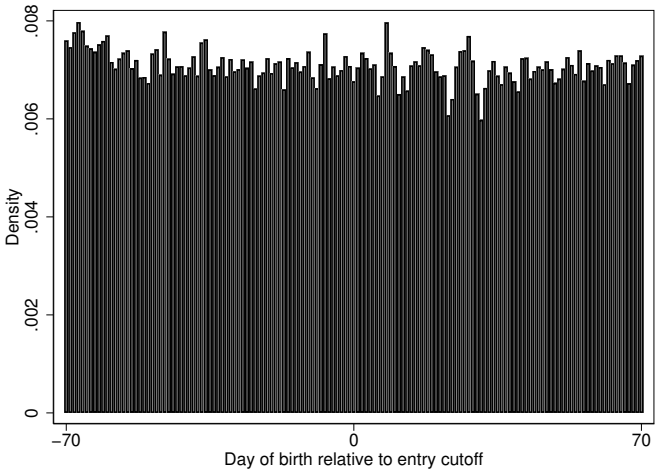
Notes: This figure presents the share of women who appear in the sample for women born around the school entry cutoff. Outcomes are residualized of (recentered) year of birth fixed effects. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Panel (a) presents shares for the overall sample; Panel (b) presents shares for the early-career sample; and Panel (c) presents shares for the mid-career sample. The denominator for this share is the number of girls born on each day and year, which is obtained from the 1960-2017 birth records. The numerator for this share is the number of unique women that appear in the sample. The numerator and the denominator exclude Black women. See Table 3 for additional details about the sample.

Figure A.4: Density of Birth Dates Around the Cutoff in Early- and Mid-Career

(a) Younger than 25

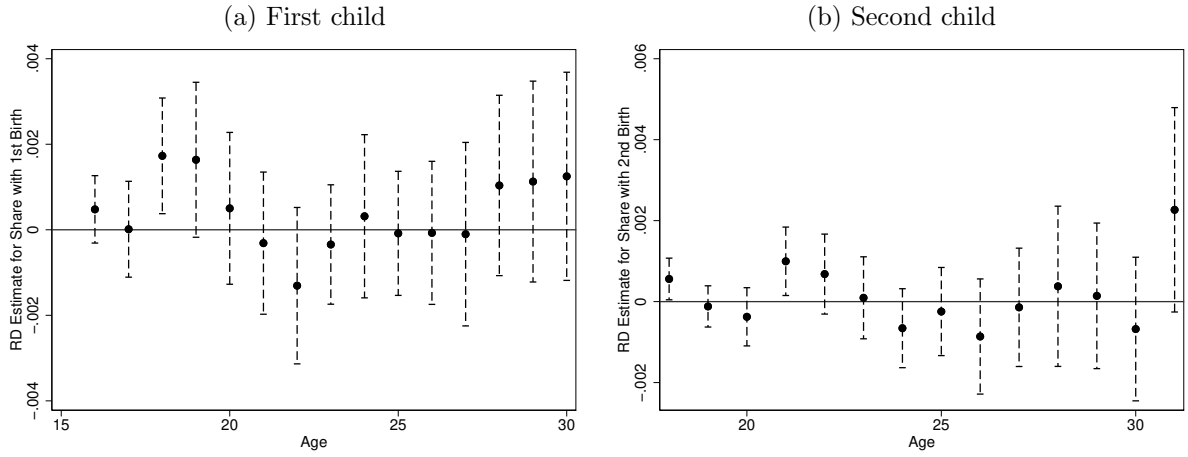


(b) Age 25 or older



Notes: This figure presents the histogram of dates of birth around the cutoff, for the early-career sample (panel a) and the mid-career sample (panel b). See Table 3 for additional details about the sample.

Figure A.5: No Discontinuity in Likelihood of Giving Birth Across Age Distribution



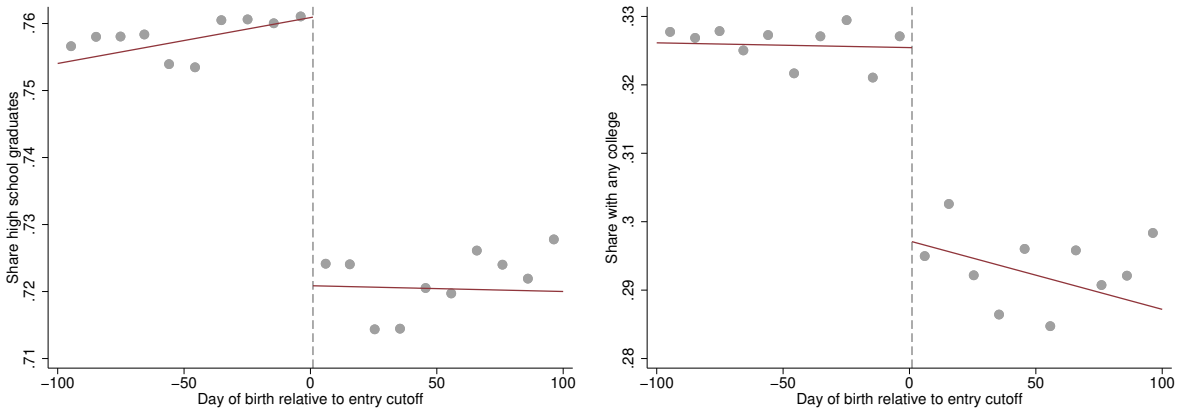
Notes: This figure presents the estimated effect (and 95% confidence intervals) of being born just prior to the school entry cutoff on the likelihood of having a first child (panel a) or second child (panel b) at each age. See Table 3 for details about the sample and estimation.

Figure A.6: Discontinuity in Secondary and Post-Secondary Education at School Entry Cutoff

(a) Younger than 25

(i) High school or more

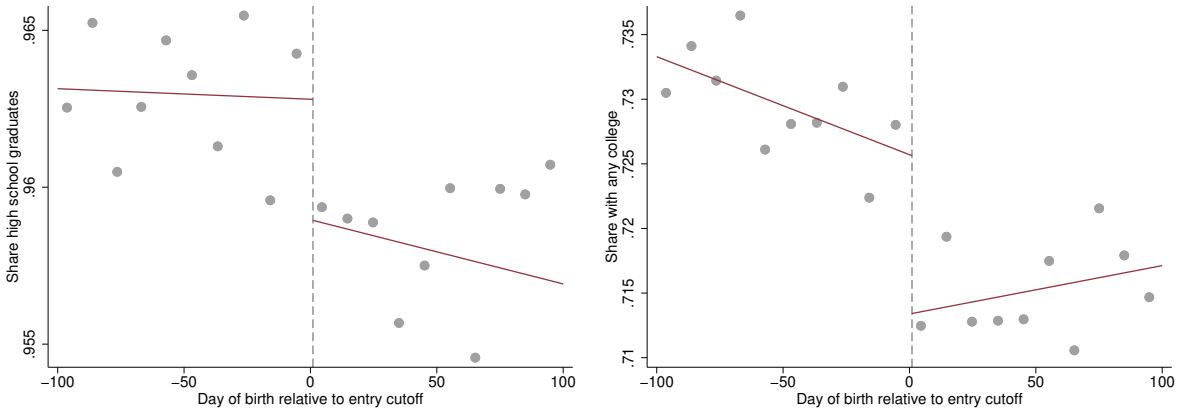
(ii) Some college or more



(b) Age 25 or older

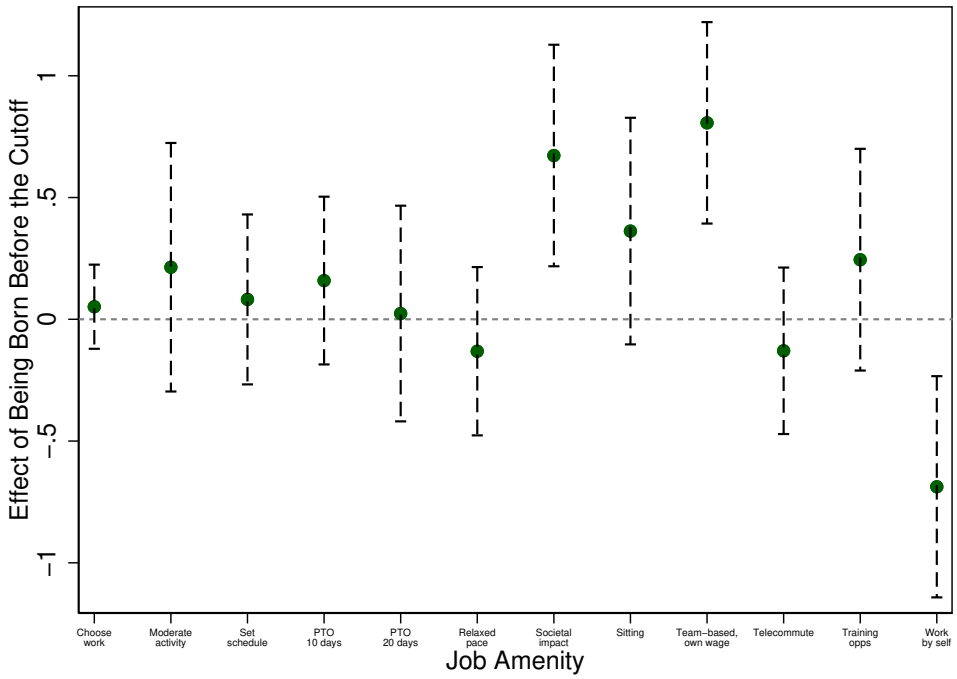
(i) High school or more

(ii) Some college or more



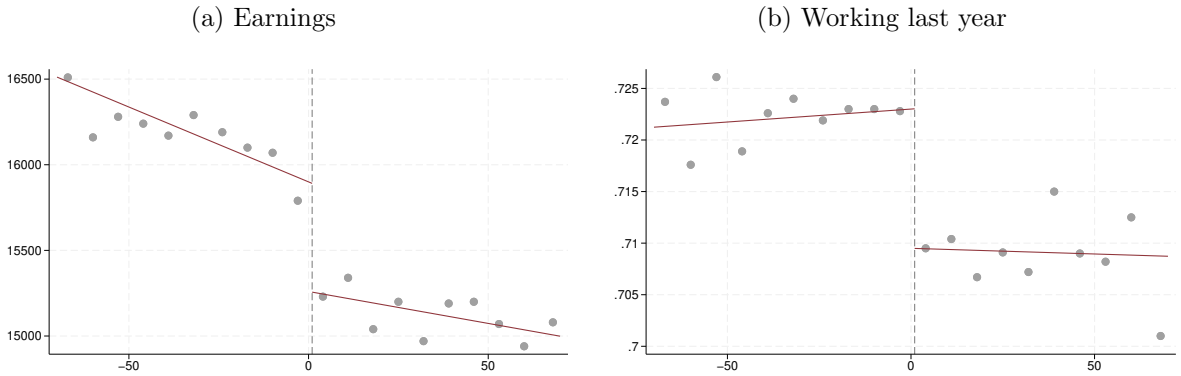
Notes: This figure presents the average secondary and post-secondary education outcomes for women born around the school entry cutoff. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Panel A presents outcomes in early-career and Panel B presents outcomes in mid-career. Outcomes include the share of women who have graduated from high school (subfigure i) and the share of women who have attended any college (subfigure ii).

Figure A.7: Impact of the Cutoff on Working Conditions



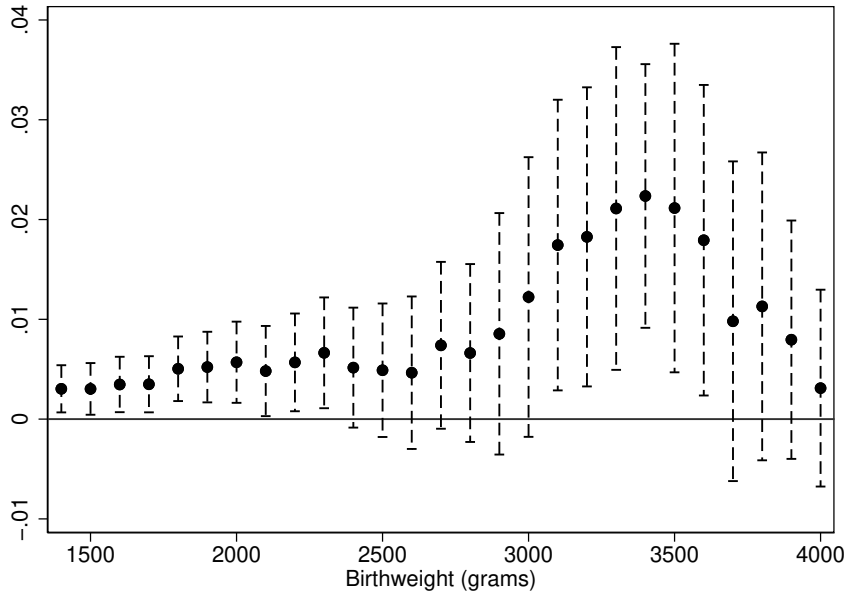
Notes: This figure presents the estimated effect (and 90% confidence interval) of being born just prior to the school entry cutoff at mid-career on a range of job amenities. The effect of the discontinuity on each job amenity is estimated using occupation-level averages calculated from the American Working Conditions Survey (Maestas et al., 2017), which is described in Appendix Section A.2. See Table 3 for further details about the sample and estimation.

Figure A.8: Discontinuity in Earnings and Employment at Childbirth (Census records)



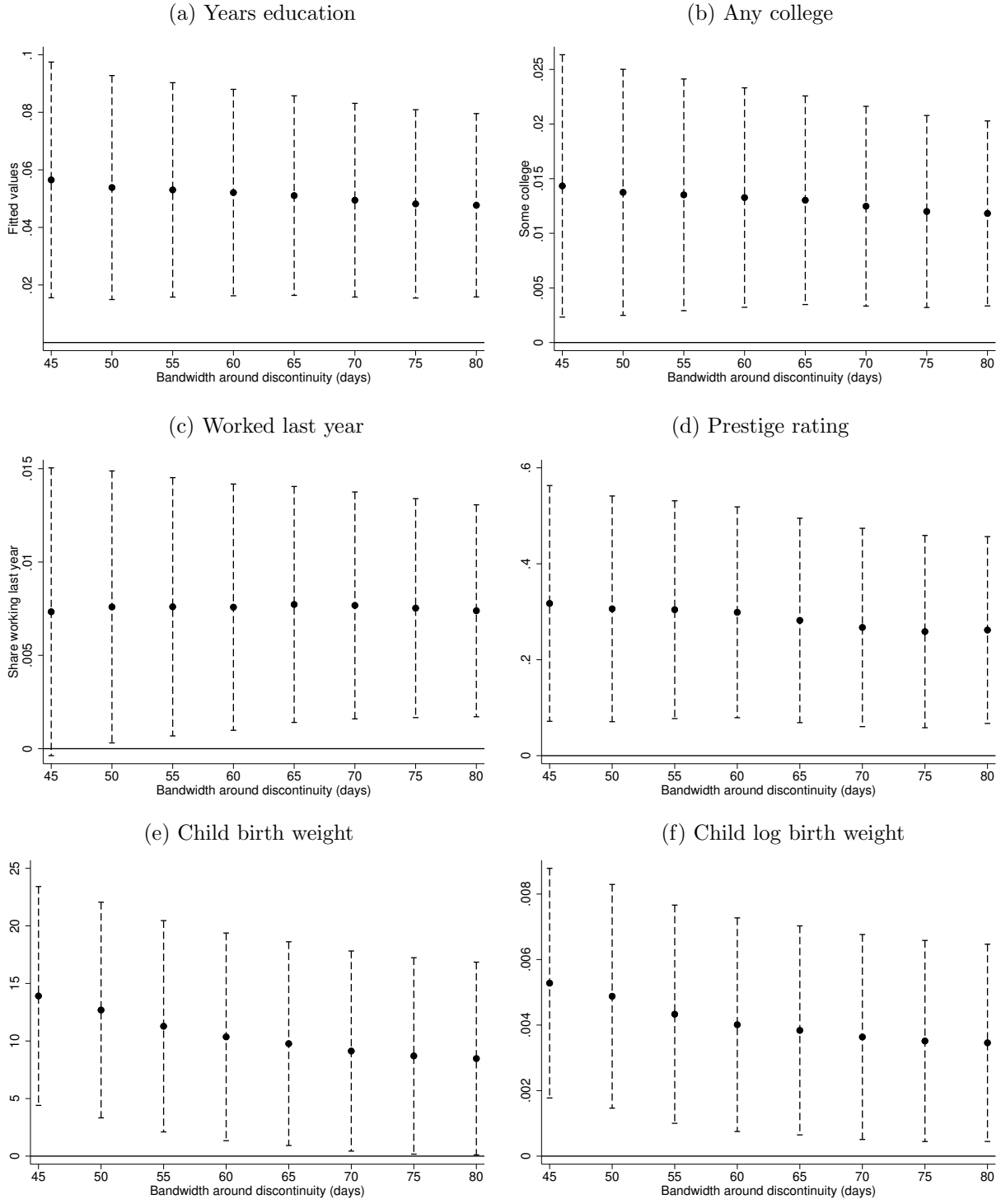
Notes: This figure presents the average earnings and share working last year for women born around the school entry cutoff. Average outcomes (shown on the y-axis) are reported for 10 equally-sized bins of birth dates on each side of the cutoff. Source: Census administrative earnings records. See text for details.

Figure A.9: Discontinuity in Distribution of Birth Weight at Mid-Career



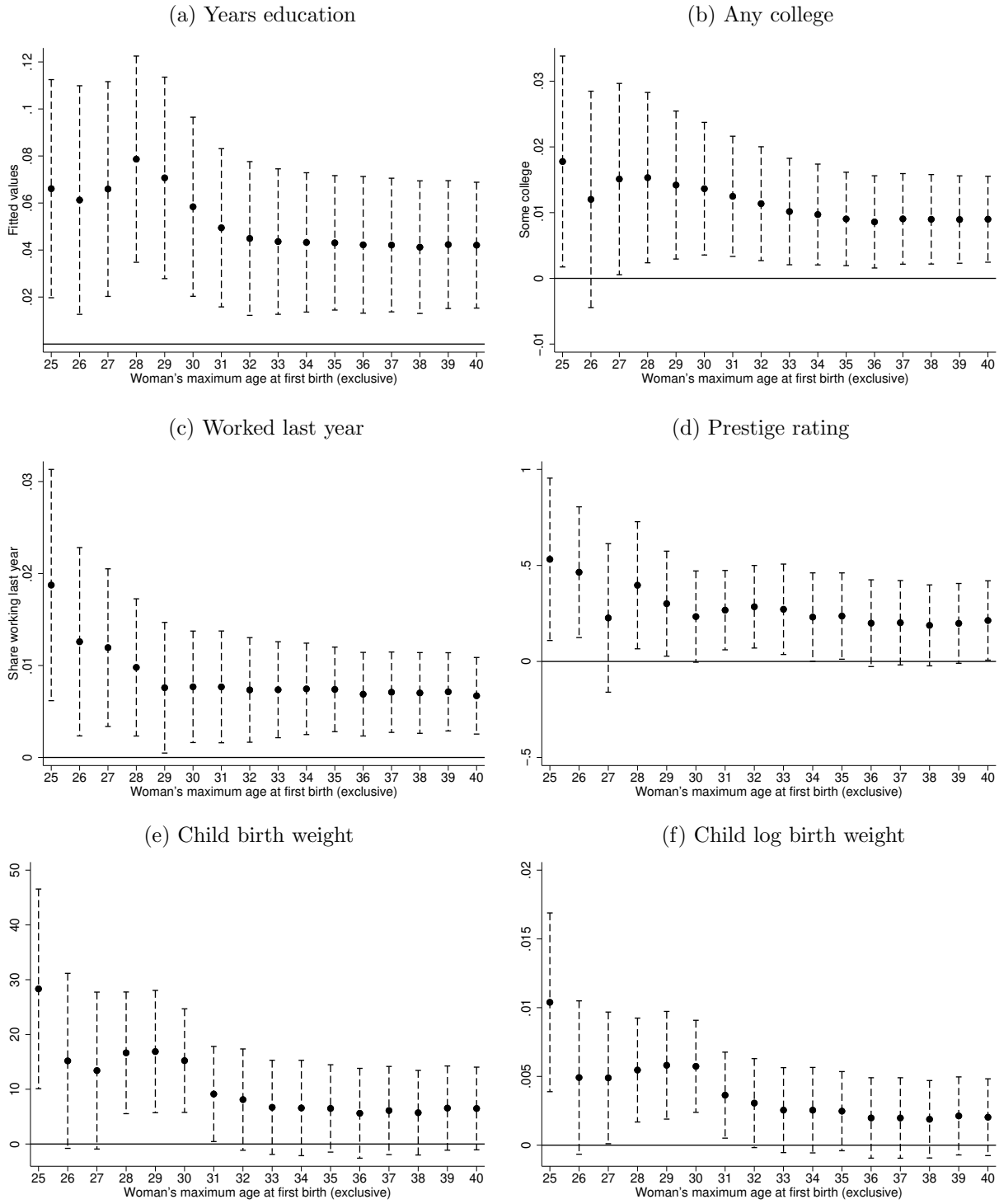
Notes: This figure presents the estimated effect (and 95% confidence interval) of being born just prior to the school entry cutoff on the likelihood of having a child with a birth weight greater than X, where X is shown on the x-axis. See Table 3 for details about the sample and estimation.

Figure A.10: Effects at Mid-Career using Alternative Bandwidths



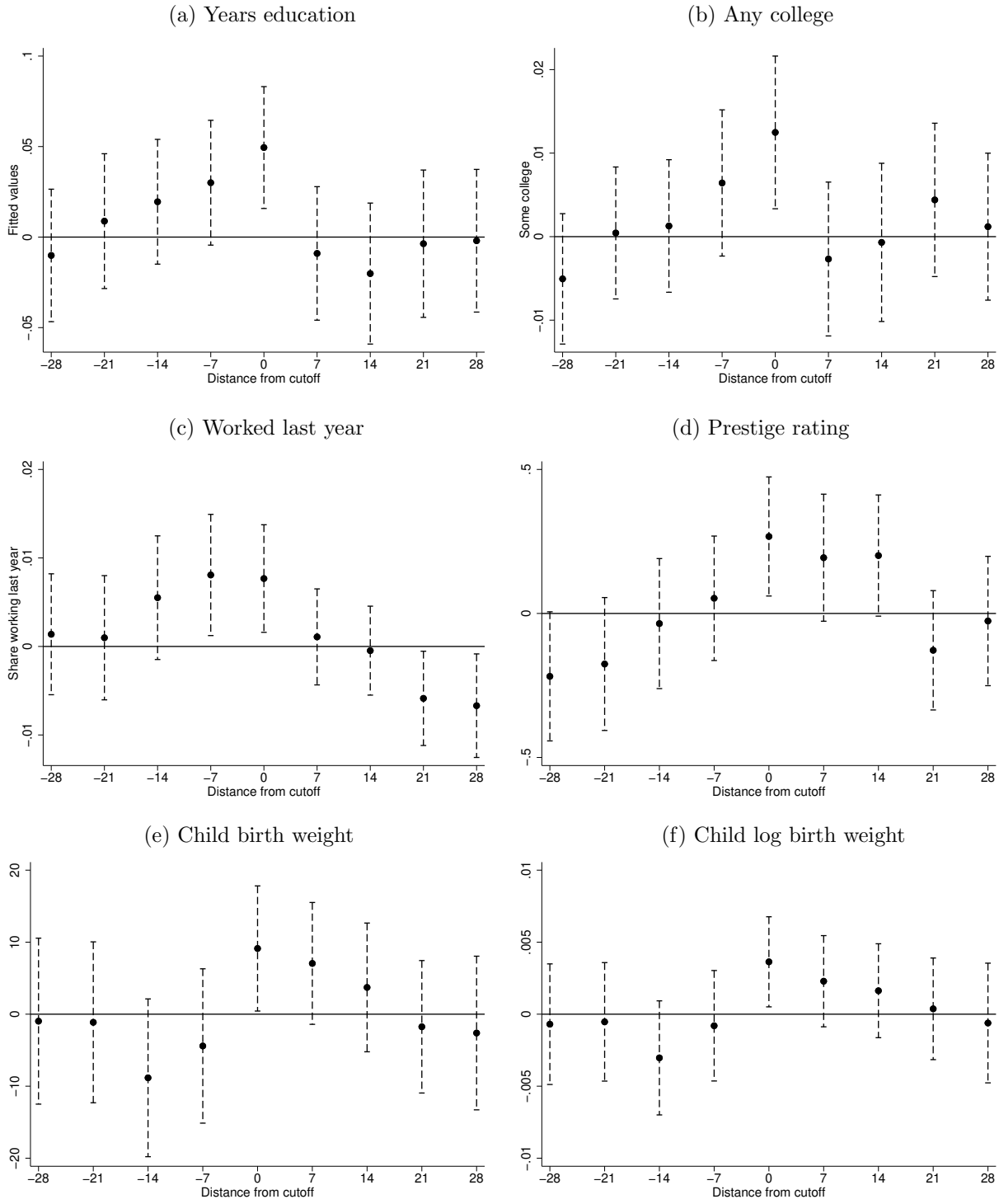
Notes: This figure presents the estimated effect of being born just prior to the school entry cutoff at mid-career for a variety of alternative bandwidths, along with 95% confidence intervals. Each subfigure presents a different outcome: imputed years of education (panel a), completing any college (panel b), the likelihood of working in the previous year (panel c), average occupational prestige (panel d), child birth weight (panel e); log birth weight (panel f). See Table 3 for further details about the sample and estimation.

Figure A.11: Effects at Mid-Career using Alternative Age at First Birth Cutoffs



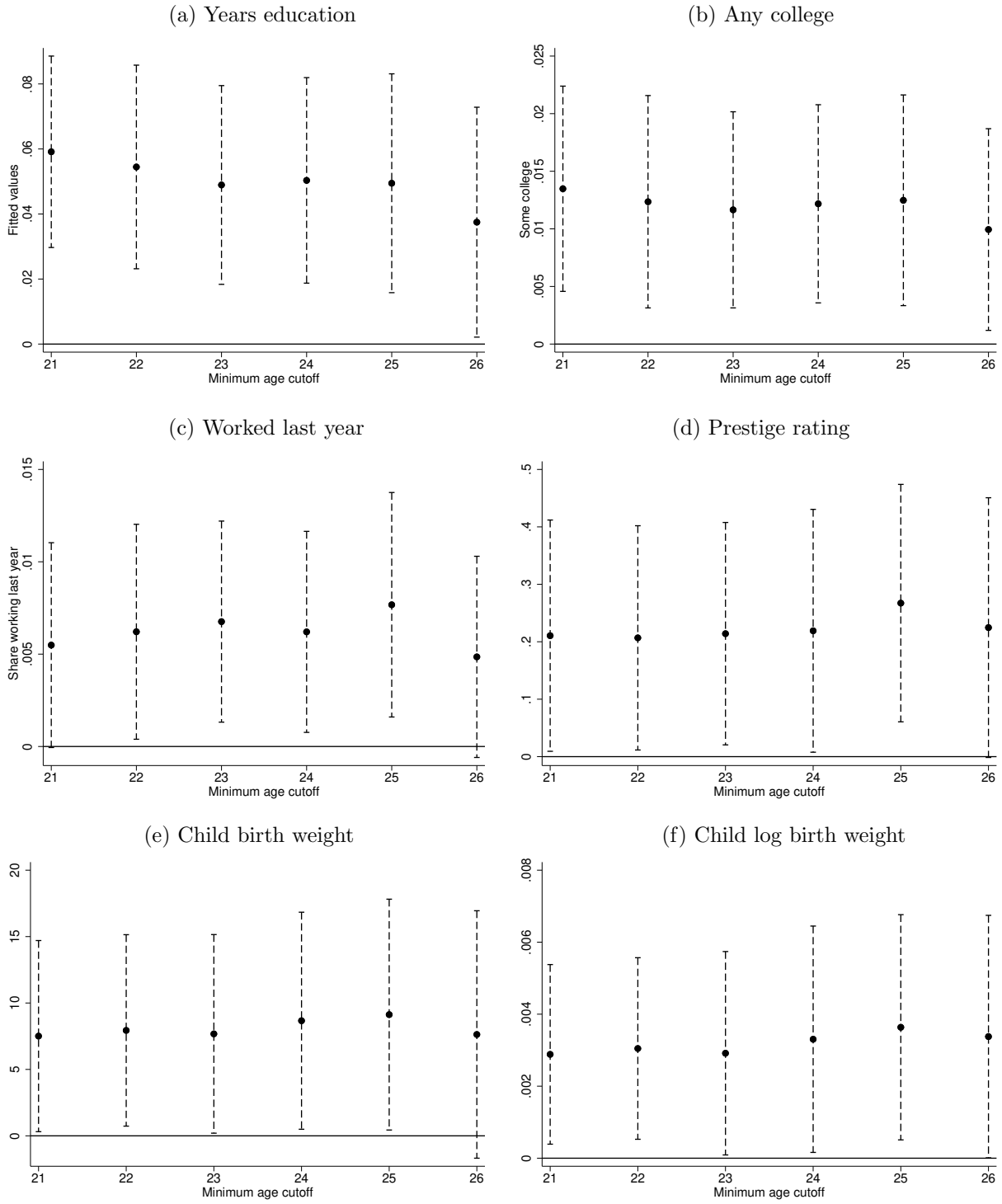
Notes: This figure presents the estimated effect of being born just prior to the school entry cutoff at mid-career using alternative sample cutoffs for age at first birth, along with 95% confidence intervals. Each subfigure presents a different outcome: imputed years of education (panel a), completing any college (panel b), the likelihood of working in the previous year (panel c), average occupational prestige (panel d), child birth weight (panel e); log birth weight (panel f). See Table 3 for further details about the baseline sample and estimation.

Figure A.12: Effects at Mid-Career using Placebo Cutoffs



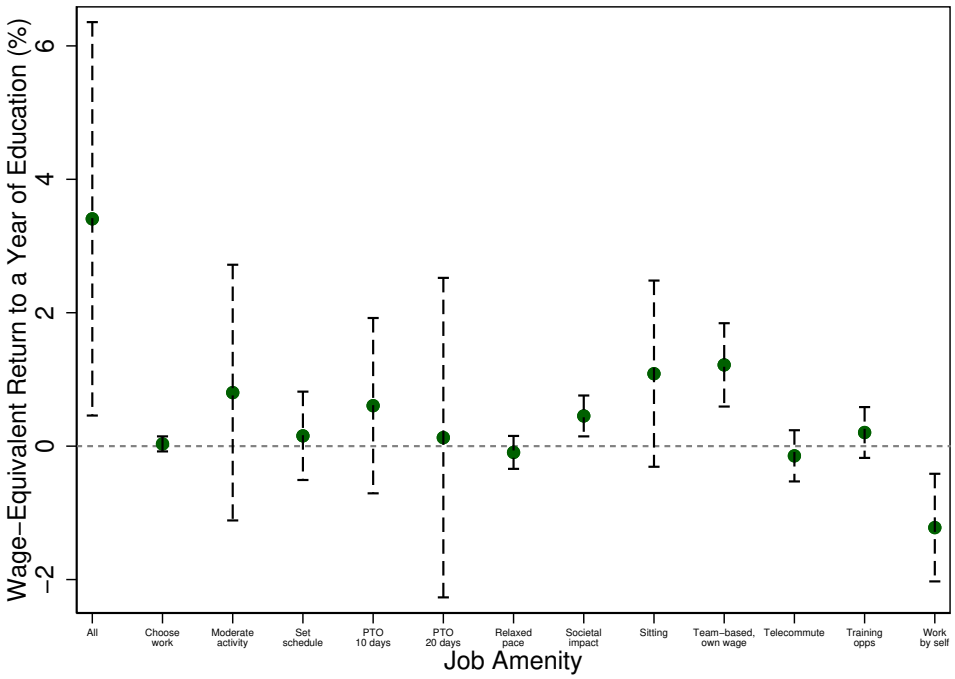
Notes: This figure presents the estimated effect of being born just prior to the school entry cutoff at mid-career for placebo school entry cutoffs, along with 95% confidence intervals. The placebo cutoffs are chosen within an eight-week window around the cutoff and spaced apart by two-weeks. Each subfigure presents a different outcome: imputed years of education (panel a), completing any college (panel b), the likelihood of working in the previous year (panel c), average occupational prestige (panel d), child birth weight (panel e); log birth weight (panel f). See Table 3 for further details about the baseline sample and estimation.

Figure A.13: Effects Using Alternative Cutoffs for Mid-Career



Notes: This figure presents the estimated effect (and 95% confidence intervals) of being born just prior to 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), completing any college (panel b), the likelihood of working in the previous year (panel c), average occupational prestige (panel d), child birth weight (panel e); log birth weight (panel f). See Table 3 for further details about the baseline sample and estimation.

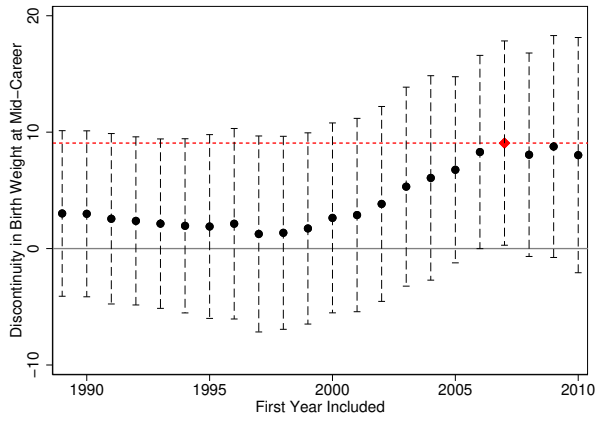
Figure A.14: Wage-Equivalent Return to Education from Job Amenities



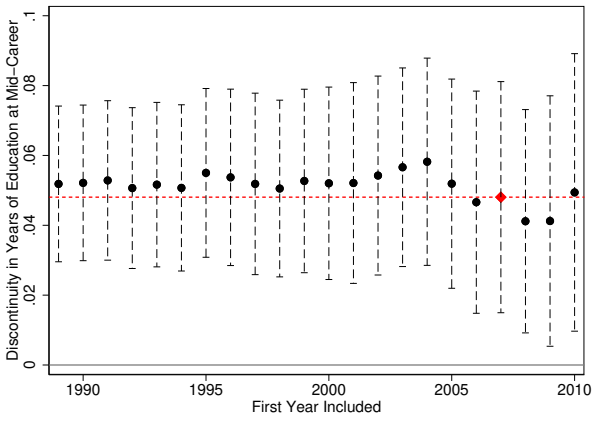
Notes: This figure presents the estimated effect (and 90% confidence interval) of being born just prior to the school entry cutoff at mid-career on a range of job amenities, rescaled to give the willingness-to-pay for the amenity value of one additional year of education. The effect of the discontinuity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

Figure A.15: Intergenerational Transmission Increases over Time

(a) Child birth weight



(b) Years of Education



Notes: This figure presents the estimated effect (and 95% confidence interval) of being born just prior to the school entry cutoff at mid-career, when the sample is expanded to include earlier years of birth records. Each marker shows the estimate from a regression that includes the years from X until 2017, where X is shown on the x-axis. The baseline estimate is shown in the red diamond. The panels present different outcomes: birth weight (panel a) and predicted years of education (panel b). See Table 3 for further details about the baseline sample and estimation.