Birth Timing and Spacing: Implications for Parental Leave Dynamics and Child Penalties*

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Abstract

We develop new facts on relationships between the timing and spacing of births, parental leave take-up, and labor market outcomes using Danish administrative data. We document substantial heterogeneity in age at first birth across maternal skill levels. Average spacing of pregnancies is also tighter for highly skilled mothers, resulting in higher fertility levels and time on parental leave soon after first birth. We estimate event studies by skill level and find that much of child penalties in earnings and participation after first birth can be explained by incapacitation effects from parental leave around subsequent births, especially for the highly educated.

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1 Introduction

Consider Morgan. Morgan is a new parent who takes one year of job-protected parental leave following the birth of their child. At the end of the leave period, they return to the same pre-birth job. During parental leave, Morgan is entitled to 6 months of full pay paid by the firm, plus 6 months at 50% of usual pay, paid by the Government. When estimating the impact of childbirth on labor market outcomes, should Morgan be treated as employed or non-employed during the leave period? How should Morgan's earnings be measured? Zero percent of their usual level? Or would one of 50%, 75% or 100% be more appropriate?

The answers to these questions should depend on the particular research question at hand. If one is concerned with the value of productive work supplied to the formal labor market, then it is appropriate to treat Morgan as non-employed and earning 0% of their usual pay. Conversely, if the aim is to analyze labor market attachment and labor supply adjustments post-childbirth, Morgan should be counted as employed. Indeed, the ILO guidelines suggest that job protected parental leave should be assimilated to employment.

However, contributions to the growing literature on career costs of children are often unclear or inconsistent in their measurement of labor market outcomes over spells of parental leave.[1] The institutional design of parental leave benefits can complicate the consistent classification of outcomes during leave, as parental benefits can be paid by employers (and thus assimilated into labor earnings) or by governments through social security. If one uses positive labor earnings to define employment, a person on parental leave will be counted as employed in periods when they receive transfers from an employer, but nonemployed when those transfers come from the government.

In this paper, we show that the treatment of labor market outcomes over spells of parental leave has a significant impact on estimates of child penalties, even in the medium run. Using rich Danish administrative data, we explore relationships between parental leave dynamics, labor market outcomes, and the timing and spacing of births. As monthly information on earnings and hours worked is available for the universe of Danish employees starting in 2008, we focus on cohorts of first-born children born from 2012 to 2014, including more than 74,000 children and their parents, to observe a detailed set of outcomes for a balanced panel of parents during a 10-year window centered around first birth.

We first show that mothers spend a substantial fraction of the five years following their first birth on parental leave. While this is largely mechanical in the year a woman first becomes a mother,[2] in the medium run this is driven by higher-order births. We document

[1] We summarize approaches adopted by some of the most cited papers in the literature in Table E.

[2] Some contributions omit outcomes in the year of birth when documenting child penalties because of this, see Albrecht et al. (2024).

that 75% of mothers have at least one additional child in the five years following their first birth, with substantial heterogeneity in the number of higher-order births by mothers' skill levels. More than 80% of the highest educated women have at least one additional child in the five years following first birth, compared to just 60% in the least educated group. This variation partly arises from more educated mothers having their first child at older ages, often close to graduation: the average woman in the least educated group is 25.4 years old at first birth compared to 30.8 years for the average woman with a university degree.

Heterogeneity in timing and spacing of births create large differences in the likelihood that a mother is on parental leave in the five years following first birth by education group. We find that the most educated mothers spend 25% of that time on leave on average, compared to 11% for the least educated group. As take-up of parental leave by Danish fathers is universally low, the share of men on leave is negligible, with little variation by education group in the first years into parenthood.

In the second part of the paper, we show that these dynamics are relevant for harnessing event study designs to estimate the "child penalty" in labor market outcomes, as popularized by Kleven et al. (2019b). The penalty measures the degree to which female outcomes fall behind men's due to parenthood. The primary specification used in this literature includes gender-specific time-from-first-birth effects, alongside age and calendar year fixed effects. In so doing, the approach assumes that age at first birth is independent of earnings potential, that labor market outcomes are smooth in the absence of childbirth, and that measurement error in labor market outcomes is independent of the timing of first birth.

Our findings in the first part of the paper advocate for two sets of simple modifications to the main child penalty specification to recover the impact of childbirth on labor market outcomes. Systematic variation in age at first birth by skill level and discontinuous arrival rates of first birth around graduation require a fully interacted model with skill effects and inclusion of controls for years since graduation. We show that this modification reduces the estimated child penalty in earnings five years following first birth by 13% on average, but this reduction ranges from 24% among college graduates to nil among the least educated group, relative to that implied by the standard specification. This difference reflects that age-earnings profiles are relatively steep upon graduation for all groups except the least educated, and correlations between age, graduation, and time of first birth differ substantially between women and men.

Our finding that women spend a substantial fraction of time on parental leave for higher-order births implies that estimates of the child penalty are clearly affected by how one treats parental leave when measuring labor market outcomes. Conceptually, parental leave and other work interruptions shape earnings through several channels. First, there is a contemporaneous incapacitation effect, as actual hours and earnings drop to zero during leave. Second, labor market experience does not accumulate, and human capital may depreciate, with time not actively working. Third, labor supply choices may adjust after childbirth to accommodate work-life balance.[3]

We provide plausible bounds on the impact of different approaches to treating parental leave on the child penalty. If one is interested in the value of productive work supplied to the labor market, outcomes should be set to zero while on leave. If researchers are rather interested in labor market attachment and the characteristics of jobs held by individuals, net of incapacitation effects of leave, one could set an individual's outcomes equal to those observed in the month before they start (job protected) leave for the whole leave duration, as these are the terms parents are entitled to come back to. This modification affects the child penalty directly through the measurement of the outcome variable, and indirectly via the impact on estimated age-earnings profiles. We show that treating parental leave spells in this way reduces child penalties in earnings five years after first birth by 45% on average, ranging from 50% among the highest educated to 12% among the least educated, relative to that implied by the standard specification. As birth spacing is lower, and leave take-up and pre-leave earnings are higher for the higher-educated, estimates of the child penalty for this group are especially sensitive to how one measures labor market outcomes during parental leave.

This paper makes two main contributions. First, we show that the measurement of economic outcomes over spells of parental leave has a first order impact on estimates of the child penalty. This is true even in the medium run because a significant portion of the estimated child penalty in Denmark arises from leave taking for higher-order births. This point is not consistently acknowledged in the growing literature estimating the impact of parenthood on labor market outcomes. While we follow Kleven et al. (2019b) and focus on Denmark in our empirical application, this measurement contribution is relevant in wider settings with job-protected parental leave. Our results are also important for interpreting changes in child penalties over time and across countries when parental leave policies vary. Institutional settings are vitally important to consider when estimating child penalties in labor market outcomes, to ensure a consistent approach to measuring outcomes of interest.

We find that a large portion of estimated child penalties can be attributed to incapacitation effects of parental leave taken for higher order births. This is consistent with findings from several existing evaluations of parental leave extensions, showing that longer entitlements mostly delay mothers' return to work, with negligible impacts in the longer

[3] In addition to these effects, which matter independently of job protection, career breaks that are not job-protected would require a new job search process to re-start work.

run (Schönberg & Ludsteck 2014; Lalive & Zweimüller 2009; Kleven et al. 2024b; Jakobsen et al. 2022). The distinction between incapacitation and long-run effects of career breaks is especially important to inform policy interventions aimed at reducing child penalties. This distinction is relevant not only in countries with relatively long parental-leave entitlements, but also in those where the private sector supplements (limited) public provision via company-level practices, as employer-provided leave is even more strongly correlated to worker skills than the take-up of public support.

Second, we highlight that systematic patterns in the timing of first and subsequent births have important implications for estimating child penalties on event study specifications. This finding complements existing work on the relationship between skill/income and fertility descriptively (e.g. Bailey & Hershbein, 2018) and causally (e.g. Monstad et al., 2008; Dalton et al., 2020).[4]

The documented skill heterogeneity in fertility dynamics can help reconcile the large child penalties often found with event study designs with the smaller penalties generated in instrumental variable approaches that compare the outcomes of women with successful and unsuccessful IVF treatments (see e.g. Lundborg et al., 2024; Besnes et al., 2023, and a recent discussion of this approach in Groes et al., 2024). The timing of fertility and the shape of age-earnings profiles differ systematically across genders and skill groups. Allowing age fixed effects and years-from-graduation fixed effects to vary by skill level provides an easy-to-implement solution for researchers using event-study designs to estimate child penalties across contexts and institutional settings where valid instruments for childbirth are not forthcoming.[5] Related to our approach, Melentyeva & Riedel (2023) propose a stacked difference-in-differences design to account for heterogeneity in the timing of first birth. While their approach avoids the "forbidden comparison" problem that arises when women who gave birth in the past serve as controls for women who give birth later, their estimates do not factor in time since graduation nor do they adjust outcomes during parental leave.

2 Data

We leverage Danish administrative data that include high-frequency information on earnings, benefits, hours worked, and parental leave for the universe of Danish-resident employees. Our

[4]See Doepke et al. (2023) for a review of the recent literature on the economics of fertility. Our work also complements research on the impact of delaying motherhood on labor market outcomes (Miller, 2011; Gallen et al., 2023).

[5]As the time from graduation is highly predictive of the timing of first birth for highly educated mothers, the inclusion of fixed-effects for the time from graduation can indirectly capture a woman's intention to conceive – something that the IV literature has measured based on the timing since the first IVF attempt.

sample consists of parents of first-born children born between January 2012 and December 2014. This sample period allows us to analyze parents' outcomes over the 10-year window centered on first birth using data at the monthly frequency, which are available from 2008.[6]

We use data from several population-level registers to build a 10-year balanced panel of 63,344 mothers and 59,644 fathers, containing information on demographics, education, fertility, and labor outcomes.[7] Table A.1 shows summary statistics on our sample.

Labor market outcomes Employment outcomes are obtained from the BFL-register, which contains monthly information on earnings and hours for each employer-employee match from January 2008 to December 2020. If a person is registered as self-employed in a given year (in the AKM-register), we drop this individual-year observation from our sample.

If an individual holds multiple jobs in a given month, we aggregate hours and earnings across jobs within each month. Our baseline definition of participation is working any strictly positive number of hours.

Parental leave Mothers in Denmark are entitled to 46 weeks of post-birth leave, with an additional 2 weeks reserved for co-parents.[8] We extract information on parental leave from the OF-register, which contains exact start and end dates of leave spells. Similarly, we observe spells on other benefits, including education, unemployment and disability benefits.

The first part of parental leave is covered by full salary replacement, for a duration determined by the applicable collective agreement. This hovers around 14 weeks in the private sector, and is typically longer in the public sector. After this initial period, parents are entitled to government benefits for their remaining leave, which are set at the same level as unemployment benefits.

The source of parental transfers and their measurement in the data also varies during parental leave spells. While on full salary replacement, parents are paid by employers, and payments are recorded as earnings in the BFL-register. Meanwhile, employers can claim back these expenses from the government, up to a cap.[9] Once entitlements to full replacement expire, benefits are paid directly by the government. [10] Monetary transfers from a firm to

[6]In Kleven et al. (2019b), the use of annual data allows analyses of child penalties dating back to the 1980s. Given our focus on the role of parental leave spells, we compromise on the length of the sample period to measure leave spells precisely.

[7]We drop from our sample individuals who move out of Denmark or die within 5 years of first birth.

[8]14 weeks are reserved for maternal leave, and up to 32 can be shared with the co-parent. In 2022, new regulations designated a larger share of leave to co-parents. We refer to rules in place during our sample period.

[9]The cap is adjusted annually to account for inflation. In 2024, the cap was 20,359 DKK per month ($\approx$ 2964 USD).

[10]For example, employees in administrative jobs would typically be covered by collective bargaining

a worker would, therefore, give an inconsistent measure of labor market attachment over a spell of parental leave.[11]

Skill Our primary measure of skill is the highest qualification obtained by the end of our 10-year panel, available from the UDDA-register. We consider four groups: compulsory schooling/high school dropouts (9.8% of our sample), high school/vocational school graduates (38.7%), college graduates (≈3 years of higher education/BA, 30.1%), university graduates/post-graduates (4+ years of higher education/MA, 21.4%). We treat completed education as time-invariant, as recorded at the end of our panel, as a fraction of individuals complete their education after first birth. In Appendix B, we show that our results are robust to defining skill groups by high school completion/grades available from the UDG-register. The decision to go to high school is typically made around finishing compulsory schooling at age 15/16, and exams are normally taken three years later. These grades are plausibly exogenous to preferences over completed fertility but are typically not available in other datasets commonly used in other contexts, so we focus on education level in the main body of the paper.

3 Parental Leave & Fertility Dynamics by Skill

We first document patterns in birth timing, spacing, and parental leave take-up before turning to the implications for estimating child penalties. Panel (a) in Figure 1 gives the distributions of age at first birth for mothers and fathers by completed levels of education. There is significant heterogeneity in the timing of first birth: less educated men and women have their first child at younger ages than more educated individuals. Among women, half of high-school dropouts have their first child by age 25, compared to age 31 for those with 4+ years of higher education. Differences in fertility timing by skill are less pronounced for men: median age at first birth for the least and highest-educated is 29 and 32 respectively.

The timing of first birth is closely related to the age of completing education for women but not for men. Panel (b) plots probabilities of finishing education around first birth. With

agreements between the HK trade union and employers. Through HK collective bargaining agreements, mothers in administrative jobs are entitled to 8 weeks of pre-birth leave if working for a municipality, 6 weeks if working in central government, and 4 weeks if working in the private sector. See details here: https://www.hk.dk/raadogstoette/barsel/barselsrettigheder/barselsrettigheder-i-staten.

[11]Kleven et al. (2024a) analyze child penalties across countries and argue that excluding paid leave from their definition of employment has a trivial impact on their results. However, they can only test this in countries where their data allow them to distinguish between paid leave and employment (see Figure A.1 notes). Many countries with longer paid leave entitlements, including Denmark, are excluded from this analysis. In five out of the eight countries included with more than 180-days paid leave entitlement, excluding paid leave has a significant impact on the estimated child penalty.

the exception of the least educated group, we note a sharp increase in the share of first births right after graduation for women but not for men. While the likelihood of graduating is discontinuous (and non-monotonic) around the year of first birth for women, it is smooth and monotonically decreasing for men.

The timing of higher-order births is also heterogeneous across skill groups. Figure 2 shows evidence on cumulative fertility over the sample window. By construction, this jumps from zero to one at first birth (Panel (a)). Over the next five years, most mothers and fathers have additional children, with marked variation across education groups. Highly educated women and men tend to have shorter birth spacing than the less educated, such that their total fertility rises faster in the years following first birth. At the end of year five, the least educated group of mothers has 1.72 children on average, compared to 1.94 for the most educated group. Panel (b) in Figure 2 shows the distribution of fertility at the end of year five after first birth. More than 80% of the highest educated group have additional children in this window, compared to 60% of the least educated group. However, we note that there are no clear differences in completed fertility by age 45 by skill group (see Table C.1).

Figure 3 shows evidence on spells of parental leave, alongside other benefits, around first birth. The less educated, especially high school dropouts, are less likely to be on leave than the highly educated throughout the sample window (Panel (a)). For example, 26% of the highest educated women are on leave three years after the first birth compared to 8% of the least educated. Even five years on, 10% and 4% of the most and least educated mothers, respectively, are on leave. These differences reflect the tighter spacing of births among highly educated women, as well as lower employment rates before birth among the less educated, who are thus less likely to be eligible for parental leave, conditional on fertility. These patterns imply that less educated mothers are more likely to be on other benefits, most notably education and welfare benefits (Panel (b)). Across the sample window and education levels, parental leave taking is low or negligible for fathers, though with a qualitatively similar education pattern as for women.[12]

4 Implications for Estimating "Child Penalties"

There is a large literature that estimates the impact of first births on the relative labor market outcomes of women and men. Kleven et al. (2019b) popularized an event study specification for this purpose, which has been widely used (see Cortés & Pan, 2023 for a review).

[12] In Appendix C, we show that similar patterns are evident for the cohort of mothers born between 1970 and 1975.

Kleven et al. (2019b) separately estimate the following regression for women and men to calculate the impact of first birth on labor market outcomes:

$$Y^g_{ity} = \sum_{j \neq -2} \delta^g_j \cdot \mathbb{1}[j = t] + Age^g_{yi} + Year^g_{yi} + \epsilon^g_{ity}, \tag{1}$$

where $g = \{m, f\}$ denotes gender, Y_{ity} represents the outcomes of interest, e.g., earnings, in calendar year y for individual i with first birth t years from year y. Age^g_{yi} is a set of age fixed effects that capture lifecycle dynamics in labor market outcomes, and $Year^g_{yi}$ is a set of calendar year fixed effects that control for aggregate trends.

Event-time effects δ^g_j are the coefficients of interest, which identify the effects of first birth on individual labor market outcomes relative to the omitted event year. These are identified from variation in age at first birth within event years. In so doing, the approach compares outcomes before and after first birth for individuals who give birth at different ages. The validity of this comparison requires that, conditional on the included controls, counterfactual outcomes would be invariant with respect to age at first birth and that outcomes would evolve smoothly in the absence of birth.

The findings of Section 3 are informative vis-à-vis these assumptions. Figure 1(a) showed that age at first birth varies systematically with education level. Moreover, there is evidence that age-earnings profiles are heterogeneous by skill (Adda et al., 2017). Thus, age at first birth is likely to be systematically related to earnings potential. This can be captured by including interactions between age and education effects in event-study specifications. In addition, the clustering of first births soon after graduation for women is problematic for the assumption of smooth labor market outcomes absent childbirth. This can be addressed by adding controls for time since graduation.

Section 3 also highlights that the measurement of Y^g_{ity} will be affected by the way a woman's labor market status is treated when she is on parental leave. Figures 2 and 3 showed that the majority of women will have additional children in the five years following first birth, with tighter birth spacing and greater use of parental leave among the highly skilled. Systematic differences across education groups in the availability and take-up of parental leave, and the duration of full-salary replacement (where transfers are recorded as earnings), implies that the chosen method for recording participation and earnings during leave could have quantitatively different consequences for the estimated child penalties by education.

Indeed, Figure D.1 shows that positive labor earnings provide an inconsistent measure of participation and work-related monetary transfers over the course of a leave spell in Denmark. Over the first part of spells, the data record positive labor earnings for the majority of mothers, as they are still in the period of full salary replacement paid by firms. However, when transitioning to leave benefits that are paid directly by the government, recorded labor market earnings fall significantly. Hence, using information on labor market outcomes directly from employment registers leads to an inconsistent treatment of outcomes during spells of leave, even when parents' actual hours worked consistently equal zero during a spell.

To address these fertility and parental leave dynamics, we propose to estimate Equation 1 separately by skill group, augmented by time-since-graduation effects,[13] and use monthly data to account for changes in the source of parental transfers:

$$Y_{imt}^{gs} = \sum_{j=-4, j\neq -2}^{5} \delta_j^{gs} \cdot \mathbb{1}[j=t] + Age_{im}^{gs} + Grad_{im}^{gs} + Time_{m}^{gs} + \epsilon_{imt}^{gs} \tag{2}$$

where Y_{imt}^{gs} is the outcome of interest for individual i of gender g and skill s, observed in calendar month m, t years after first birth. The fixed-effects on the right-hand side denote, in order, time-since-birth, age, time-since-graduation and calendar time. Event-time coefficients measure changes in outcomes relative to two years before birth, as the availability of parental leave up to two months before birth implies that earnings in the year before birth may be affected by the birth event.[14]

We measure parental leave spells directly in the benefit register to avoid a mixed treatment of labor market outcomes depending on whether transfers are made by firms or the government. We give child penalty results for two alternative treatments of labor market status during parental leave. In the first, we set labor market earnings, hours, and participation equal to zero throughout spells of leave. In the second, labor market earnings, hours, and participation are set equal to the level in the month just before going on leave, as these are the terms to which parents are entitled to return after leave. The latter approach is consistent with the ILO definition of employment, which counts as employed individuals who are temporarily absent from work for specific reasons, including annual, sickness, or parental leave. These exercises allow us to separate a temporary "incapacitation" effect due to parental leave from a permanent decline in earnings and participation due to labor force adjustments.

[13] For those with no education beyond compulsory schooling, years from graduation is years from finishing school.

[14] Outcomes and calendar time are measured at the monthly frequency, while event-time, age and time-since-graduation are aggregated at the annual frequency.

5 Results

Figure 4 shows event-study estimates for the impact of first birth on earnings by education group. On the plots shown, we divide the estimated event-time coefficients by predicted earnings in a given year, net of the contribution of event dummies. Specifically, we plot $P_t^g = \frac{\hat{\delta}_t^{gs}}{E(\tilde{Y}_{imt}^{gs})}$ for skill level s and gender g, where $\hat{\delta}_t^{gs}$ are estimated event-time coefficients and $E(\tilde{Y}_{imt}^{gs})$ denote predicted earnings net of the contribution of event dummies. The estimated five-year child penalty estimate, $P_5^m - P_5^f$, is given in the legend. This statistic captures the extent to which female earnings fall relative to male earnings due to childbirth, five years into parenthood and can be read as the distance between male and female outcomes at event time 5 in the event study graphs. Figures A.6-A.8 give corresponding results for participation, as well as hours of work and hourly wages conditional on participation. Panel (a) in Figure 4 estimates specification 2 by education group, but without the controls for time since graduation and sets earnings at zero during spells of parental leave, regardless of income replacement paid via payroll. Panel (b) adds controls for years since graduation (specification 2), leaving treatment of parental leave spells as in Panel (a). Panel (c) additionally sets earnings during parental leave equal to the level recorded in the month before starting the leave spell.

Panel (a) shows that maternal earnings drop by approximately 80% for all skill groups in the year of first birth relative to two years previously. This is followed by smaller but long-lasting earnings setbacks, which persist for all groups at least five years after first birth. These earnings drops reflect a combination of periods on parental leave and actual reductions in labor supply at the extensive and intensive margins. There is significant heterogeneity in the magnitude of long-run earnings declines across groups. For the highest and lowest education groups respectively, maternal earnings are predicted to be 11.9% and 31.2% lower five years after birth compared to their earnings in absence of childbirth. Paternal earnings are much less affected in the first year after birth, with a fertility premium in the longer run for highly educated fathers. Five years after first birth, the most educated fathers earn 14.9% more, and the least educated fathers earn 10.0% less, compared to their earnings in the absence of childbirth.

The inclusion of controls for years since graduation in Panel (b) absorbs part of the pre-trends observed in Panel (a) for women, and part of the earnings premia observed for highly educated men. As one would expect, this change in specification does little to the pattern observed for the less educated groups.

Panel (c) gives our preferred specification. In addition to including years-from-graduation fixed effects, it sets labor earnings during parental leave at their level in the month before going on leave. In this case, changes in earnings post-birth only reflect changes in labor supply behavior as opposed to incapacitation during parental leave.[15] As expected, this adjustment greatly dampens the drop in earnings in the year after first birth for all groups, as this mostly reflects leave take-up. Importantly, the long-run setback in maternal earnings is also reduced, and especially so for high-skilled mothers, because they are more likely to have additional births in the sample window, take additional periods of parental leave, and have higher pre-birth earnings. As take-up of paternity leave is low, men's earnings trajectories post-birth are not substantially affected by this change in measurement.

Figure 5 summarizes the impact of different specification choices for the five-year child penalty, $P_5^m - P_5^f$, for earnings and participation penalties, respectively. For each education group, we display three estimates that correspond to the child penalties implied by the different specifications given in Figure 4.

Specification (a), without years-from-graduation fixed effects, setting earnings on parental leave to zero (the first bars), implies that more highly educated women face a higher child penalty than those with less education. The child penalty for the two top groups is 29.9% and 26.7%, respectively, compared to 21.3% and 22.7% for the two bottom groups. The impact of adding years-since-graduation controls (second bars) is relatively small except for the highest educated groups. For BA graduates, controlling for years since graduation reduces the long-run penalty in earnings by nearly a quarter, from 30% to 23%. This is driven primarily by the reduction in fertility premia for men (see male outcomes in Panels (a) and (b) of Figure 4).

The difference between the second and third bars reflects the role played by parental leave in accounting for earnings declines. Setting earnings at the pre-leave level has a large impact on estimated child penalties. Parental leave for higher-order births explains 12% of the child penalty for the least educated group, 29% for high school graduates, 37% for BA graduates, and 43% for MA graduates. The adjustment has a large mechanical effect for the more than 10% of mothers on parental leave five years after first birth (Figure 3). Changing the measurement of outcomes during leave additionally alters the magnitude of age and years-from-graduation fixed-effects, which also contributes to the divergence. The sharp education gradient reflects differences in the availability and take up of maternity leave, as well as pre-birth earnings differentials.

The final set of estimates in Figure 5 aggregate child penalties across education groups by estimating a version of specification 2 for the full population of parents, including education

[15]This approach also affects the estimated age fixed effects that capture the counterfactual life-cycle earnings profiles of mothers, as labor outcomes will be recorded at their level before going on job protected parental leave.

dummies and their interaction with age and years since graduation FEs.[16] The first bar replicates quite closely the specification and results of Kleven et al. (2019b), yielding a 5-year child penalty of 23.3%. The inclusion of time-from-graduation effects in the second bar reduces the child penalty to 20.3%, and the treatment of parental leave as continuous employment in the third bar reduces it further to 12.8%, almost half the baseline estimate in the first bar.

Panel (b) in Figure 5 shows corresponding estimates for long-run penalties in participation rates. Although estimates that equate parental leave to non-employment are strikingly similar across education groups, the correction of participation rates during parental leave has a sizeable impact, with a strong education gradient. The inclusion of controls for years-since-graduation and the adjustment for leave spells accounts for 57% of the average long-run penalty in participation rates.[17]

In Appendix B, we show that these findings are robust to grouping mothers and fathers by high school grades rather than completed education. One may be concerned that completed education is endogenous to preferences over the timing of fertility. However, the decision to go to high school is typically made around finishing compulsory schooling at age 15/16, and exams are normally taken three years later. A skill measure based on high school participation/grade is therefore plausibly exogenous to fertility preferences. We split individuals with a high school grade into terciles and have those who have not completed high school (hence without a grade) as the lowest skill group. The resulting child penalties in earnings five years after first birth fall by 28.6% for the least educated and 44.6% for the highest educated when treating individuals as employed during spells of parental leave. For participation, we find decreases from 40.1% for the least educated to 65.0% for the most educated.

6 Conclusion

We use rich administrative data from Denmark to document substantial heterogeneity in the timing and spacing of births by maternal skill level. More educated mothers have their first child significantly later than less educated mothers, but their cumulative fertility rises faster

[16] Specifically, we estimate:

$$Y^g_{imt} = \sum_{j=-4, j\neq -2}^{5} \delta^g_j \cdot \mathbb{1}[j=t] + Age^g_{im} \times Edu^g_{im} + Grad^g_{im} \times Edu^g_{im} + Time^g_m + \epsilon^g_{imt} \tag{3}$$

where Edu^g_{im} indicate education fixed effects.

[17] Appendix C estimates child penalties separately by completed fertility at year-5. We find that much of the difference in estimated penalties by number of children is accounted for by the incapacitation effect from parental leave taking.

in the years following first birth. We show that these dynamics have important implications for the estimation of standard child-penalty event study designs that harness variation in the age at first birth to calculate the impact of children on labor market outcomes. We show that adjusting the canonical child-penalty specification to allow for heterogeneous age-earnings profiles by skill and years-from-graduation fixed effects, in addition to assigning individuals on parental leave the employment status they had in the month before leave, has large impacts on estimated child penalties and their incidence across skill groups. Our preferred specification yields a 45% smaller child penalty than the standard approach.

Our findings highlight the importance of institutional settings when estimating child penalties. In particular, our findings suggest that researchers exercise caution with the measurement of labor market status during periods of parental leave. Individuals on job-protected parental leave are entitled to return to their pre-birth employment conditions and would count as employed according to the standard ILO definition. In addition, the source of income replacement can vary during a spell of parental leave. Depending on the measurement approach taken, this variation in transfers can be wrongly interpreted as changes in the level of earnings. The approaches we advocate for in this paper, namely either to set earnings to zero or at their pre-leave level, permit a consistent treatment of periods of parental leave. These adjustments also provide information on the source of estimated child penalties, that is changes in labor supply behavior when not on parental leave versus the impact of subsequent births.

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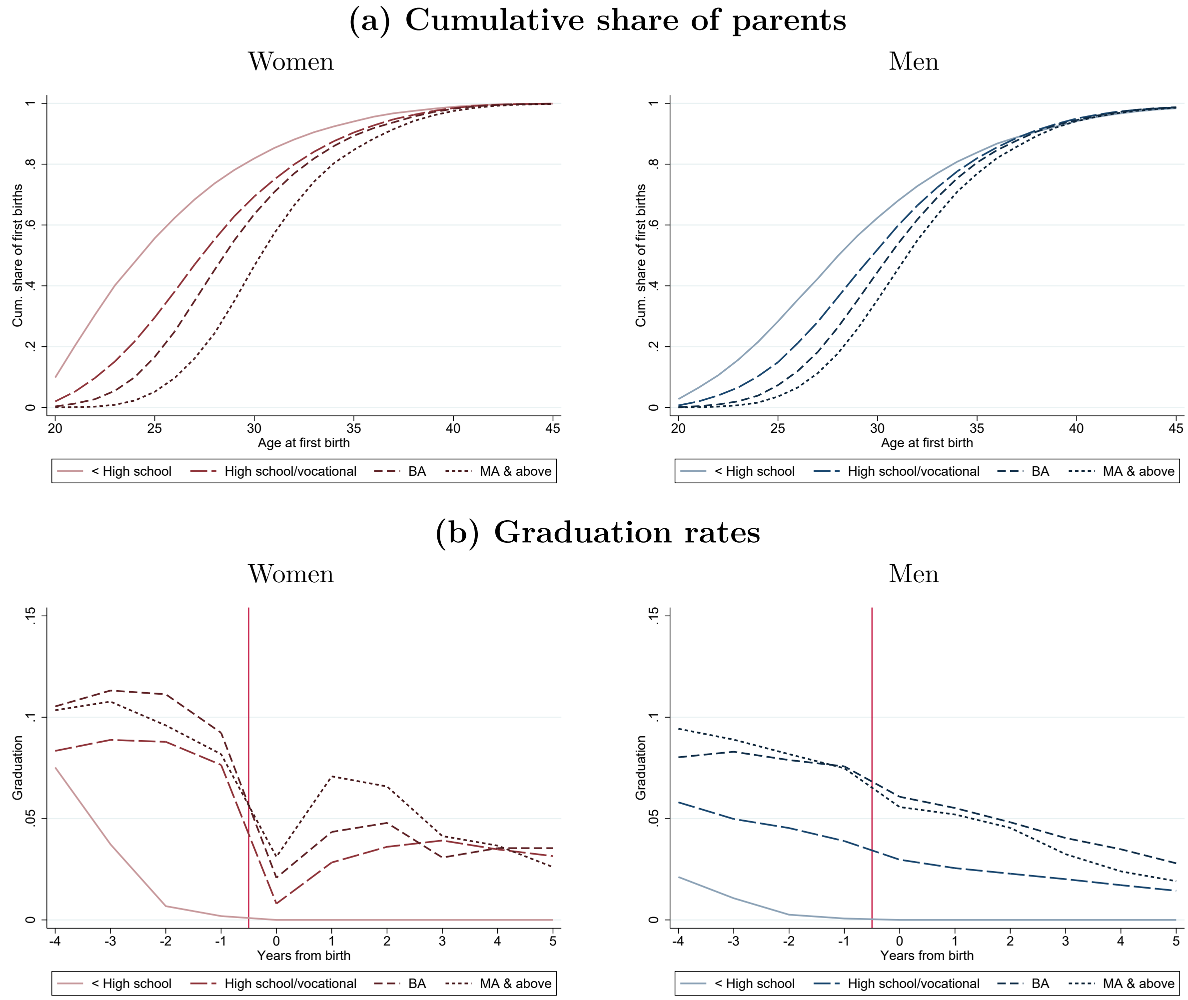
Figure 1: Age and graduation rates around first birth

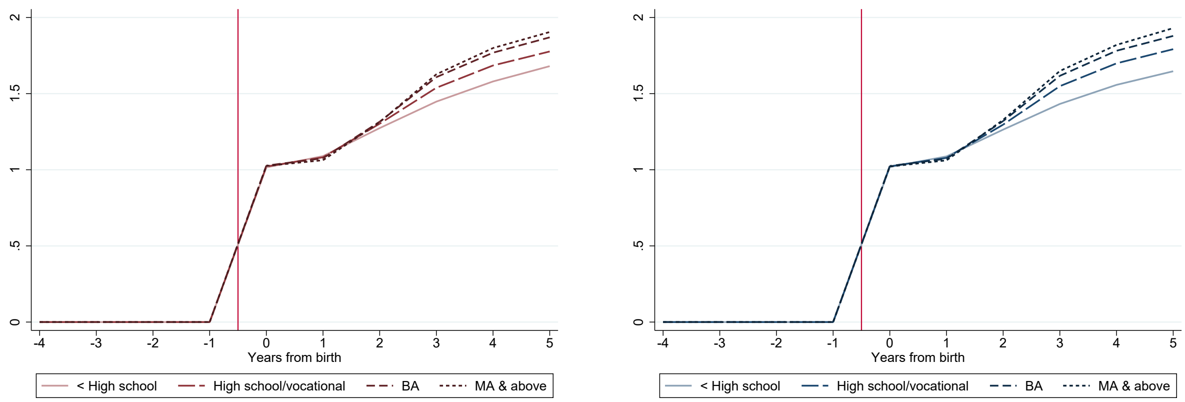


Notes: Panel (a) shows the cumulative share of first births by mothers' and fathers' age (the corresponding density functions are shown in Panel (a) of Appendix Figure A.1). Panel (b) shows the probability of graduating each year around first birth (Appendix Figure A.1 shows years from graduation at first birth). All analyses are undertaken separately by highest education level observed at the end of the panel. See Table A.1 for details on the sample.

Figure 2: Cumulative Fertility

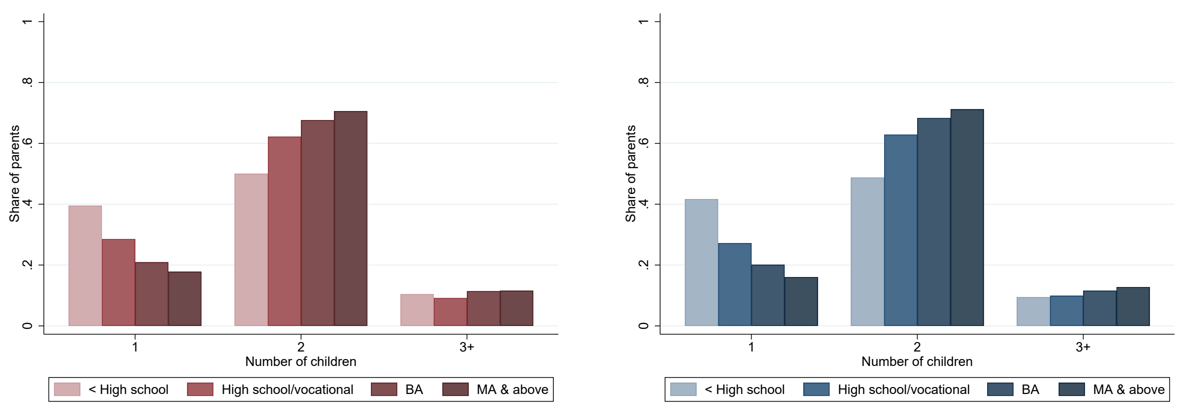
(a) Number of children by year from first birth

Women | Men



(b) Number of children 5 years from first birth

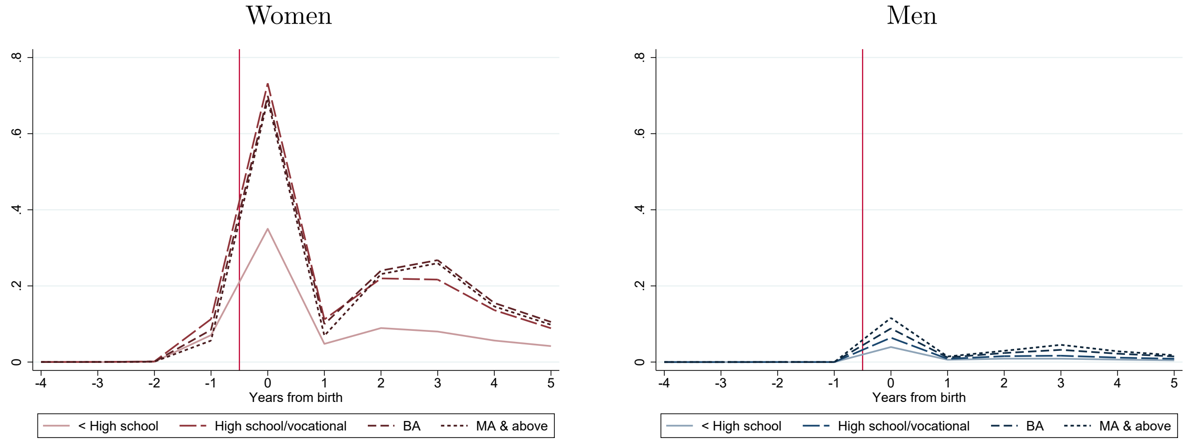
Women | Men



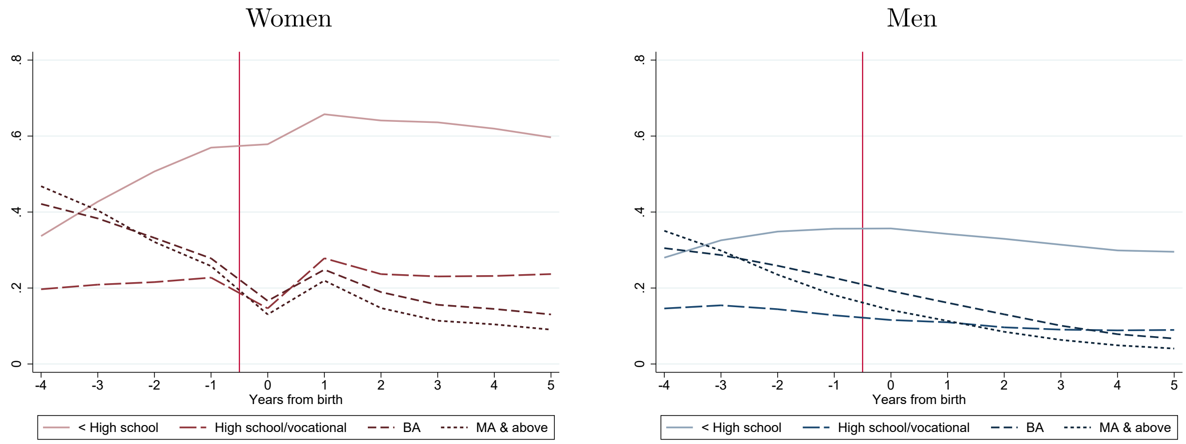
Notes: Panel (a) shows the cumulative number of children each year around first birth. Panel (b) shows the distribution of the number of children by the end of year 5 after first birth. All analyses are undertaken separately by highest education level observed at the end of the panel. See Table A.1 for details on the sample.

Figure 3: Parental leave and benefit spells around first birth

(a) Share on parental leave



(a) Share on other benefits

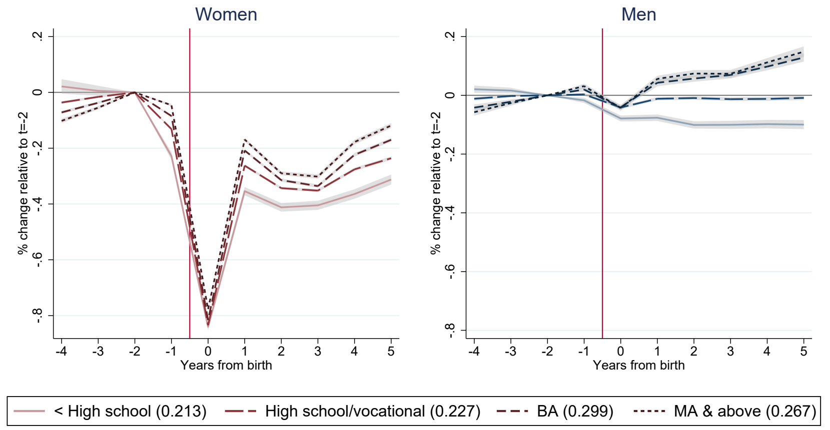


Notes: Panel (a) shows the average share of each year spent on parental leave. Panel (b) shows the average share of each year spent on other public subsistence benefits (from the OF-register). Note that most students in further education in Denmark are eligible for monthly student benefit payments from the government; these benefits are also included here. All analyses are undertaken separately by highest education level observed at the end of the panel. See Table A.1 for details on the sample.

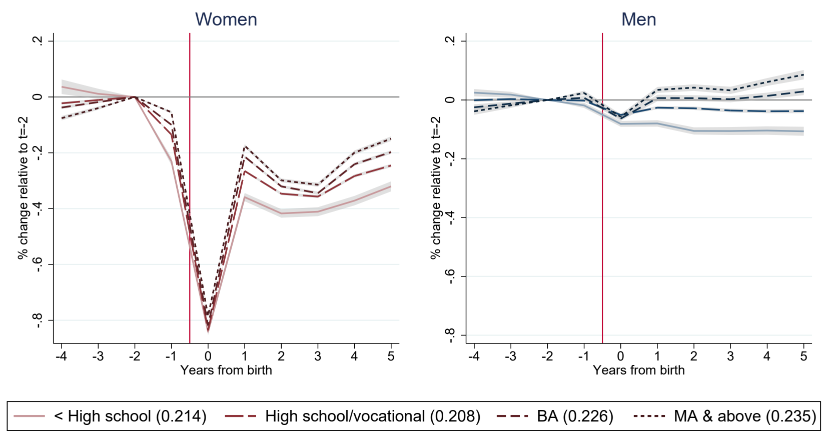
Figure 4: Child penalties in earnings by education

Results on alternative specifications

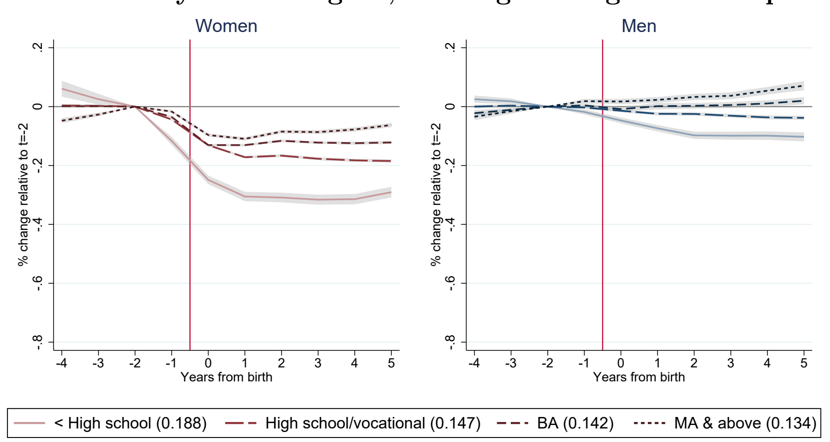
(a) Without controls for years from grad; Earnings set to 0 during PL



(b) With controls for years from grad; Earnings set to 0 during PL

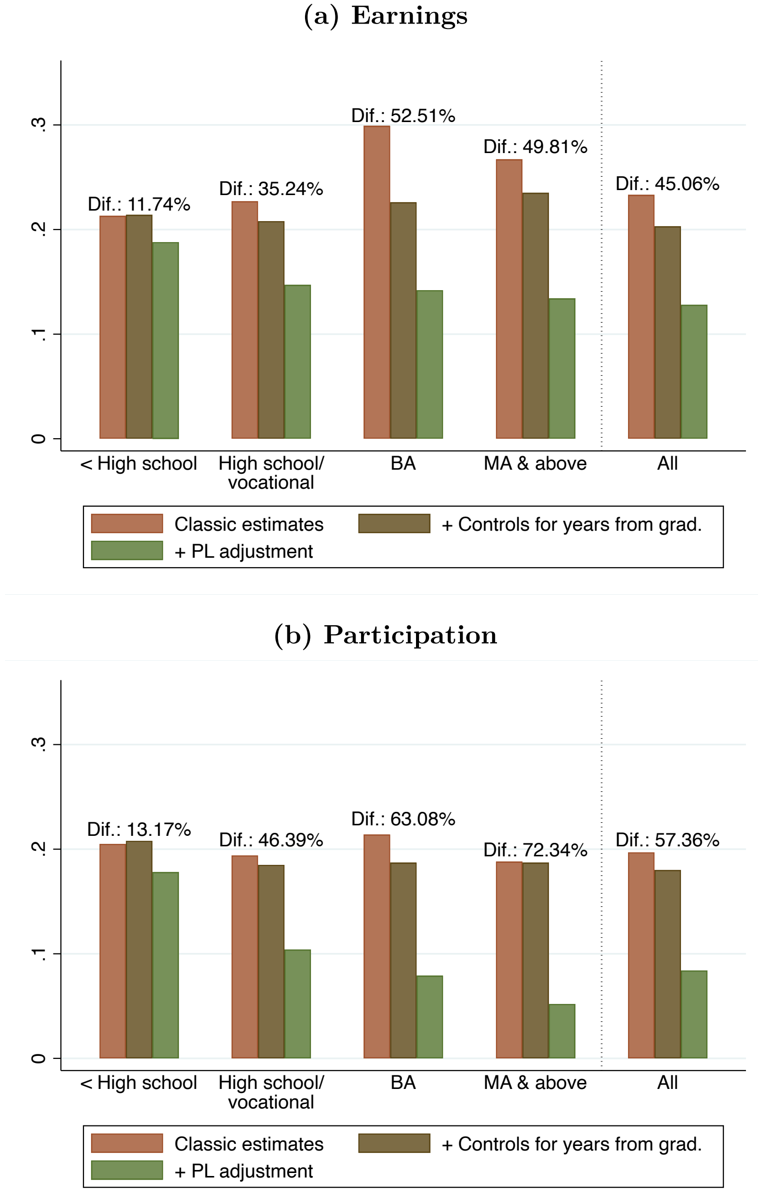


(c) With controls for years from grad; Earnings during PL set at pre-PL levels



Notes: Panel (a) illustrates estimates of child penalties by education level without controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (b) adds controls for years from graduation. Finally, Panel (c) adjusts outcomes during spells of parental leave to be equal to levels in the month immediately prior to the spell of parental leave. Child penalties by year 5 after first birth are included in legends. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample. 95% confidence intervals indicated.

Figure 5: Alternative specifications for estimating long-run child penalties



Notes: This figure includes estimates of child penalties by year 5 after first birth from various specifications. Brown bars show estimates of child penalties by education level without controls for years from graduation and treating outcomes as zeros during spells of parental leave. Olive bars show estimates with added controls for years from graduation. Finally, green bars show estimates when adjusting outcomes during spells of parental leave to be equal to levels in the month immediately prior to the spell of parental leave. For results by educational level, see Figure 4, and Figures A.6 to A.8 for illustrations of the underlying dynamics. Similarly, see Figure A.3 to A.5 for dynamics underlying the aggregate estimates. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample.

A Additional results

A.1 Stylized Facts

Table A.1: Summary statistics by education group, 12 months before 1st birth

Women

	Below high school	High school/vocational	BA	MA & above	All
Age	24.37	26.94	28.09	29.79	27.64
Married	0.12	0.19	0.21	0.26	0.21
Years from graduation	7.46	2.78	1.44	0.97	2.45
Employment	0.46	0.84	0.86	0.85	0.81
Share of month on benefit, ex. PL	0.54	0.22	0.29	0.28	0.28
Hours per month, inc. zeros	56.66	121.42	114.52	115.42	111.72
Hours per month, ex. zeros	124.23	144.53	133.23	135.06	137.68
Part-time	0.40	0.19	0.26	0.25	0.24
Earnings per month	8256.78	19479.56	20748.71	25617.97	20075.46
ln(earnings)	2.64	3.01	2.97	3.16	3.01
Hourly wage	152.39	167.34	184.02	218.93	183.45
ln(hourly wage)	4.94	5.03	5.16	5.32	5.13
High school grade rank	.	35.70	43.57	69.77	49.54
Number of children, end year 5	1.72	1.81	1.91	1.94	1.86
N	6201	24519	19083	13541	63344

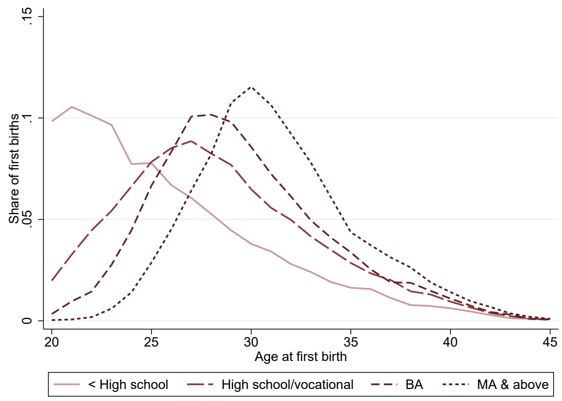
Men

	Below high school	High school/vocational	BA	MA & above	All
Age	27.90	29.22	30.20	31.02	29.53
Married	0.15	0.19	0.24	0.27	0.21
Years from graduation	10.96	5.39	1.88	1.98	4.95
Employment	0.63	0.89	0.85	0.88	0.84
Share of month on benefit, ex. PL	0.35	0.14	0.24	0.21	0.20
Hours per month, inc. zeros	90.31	132.97	120.87	126.35	123.97
Hours per month, ex. zeros	142.86	150.09	141.67	143.78	146.76
Part-time	0.25	0.17	0.20	0.19	0.19
Earnings per month	16311.29	26061.72	25972.41	31885.28	25776.93
ln(earnings)	3.07	3.26	3.20	3.36	3.25
Hourly wage	189.55	200.84	214.29	247.42	210.72
ln(hourly wage)	5.14	5.22	5.29	5.42	5.26
High school grade rank	.	32.88	43.91	65.39	48.55
Number of children, end year 5	1.68	1.83	1.92	1.97	1.85
N	8107	30869	9860	10808	59644

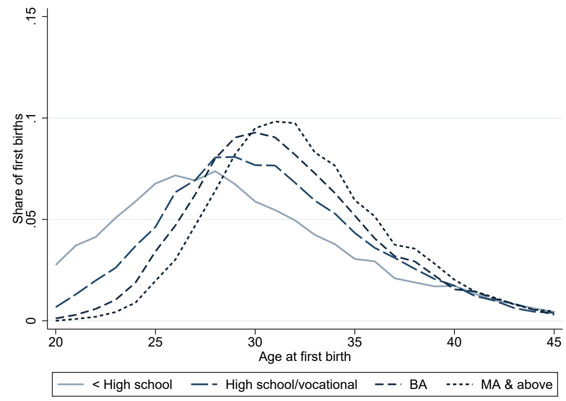
Notes: All characteristics are measured 12 months before first birth. Employment is defined as strictly positive hours of work. Share of month on benefits include all benefits except parental leave and are defined as days registered on benefits divided by total number of days in the given month. Part-time is defined as working less than 130 hours per month. High school grade ranks are defined within exam cohorts. All statistics are reported separately by education level by the end of year 5 after first birth; year 0 starts with the month of birth of the first child.

Figure A.1: Age and years from graduation at first birth

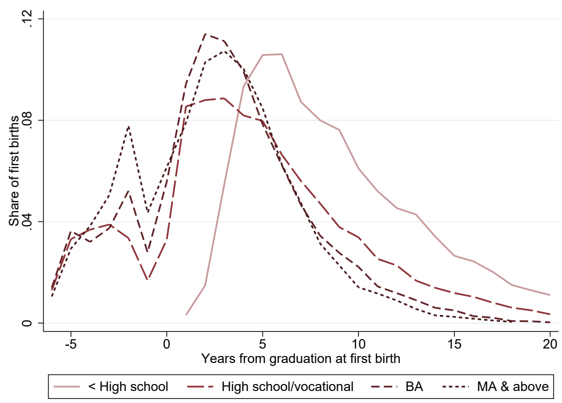
(a) Women: Share of 1st births



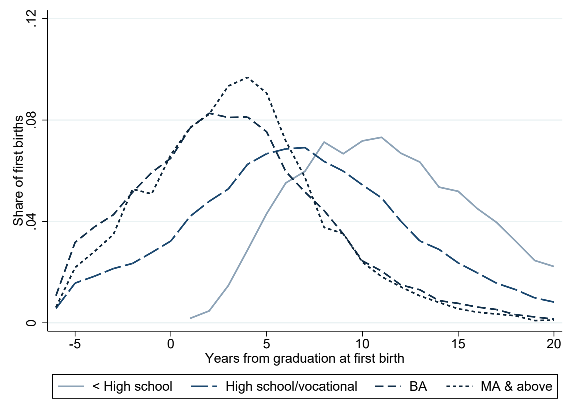
(b) Men: Share of 1st births



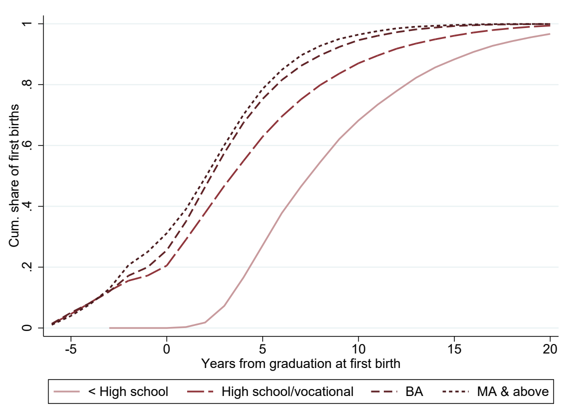
(c) Women: Years from graduation at 1st birth



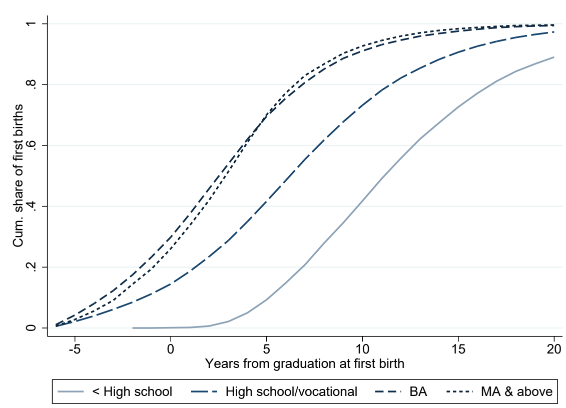
(d) Men: Years from graduation at 1st birth



(e) Women: Cumulative share of births, years from graduation at 1st birth



(f) Men: Cumulative share of births, years from graduation at 1st birth

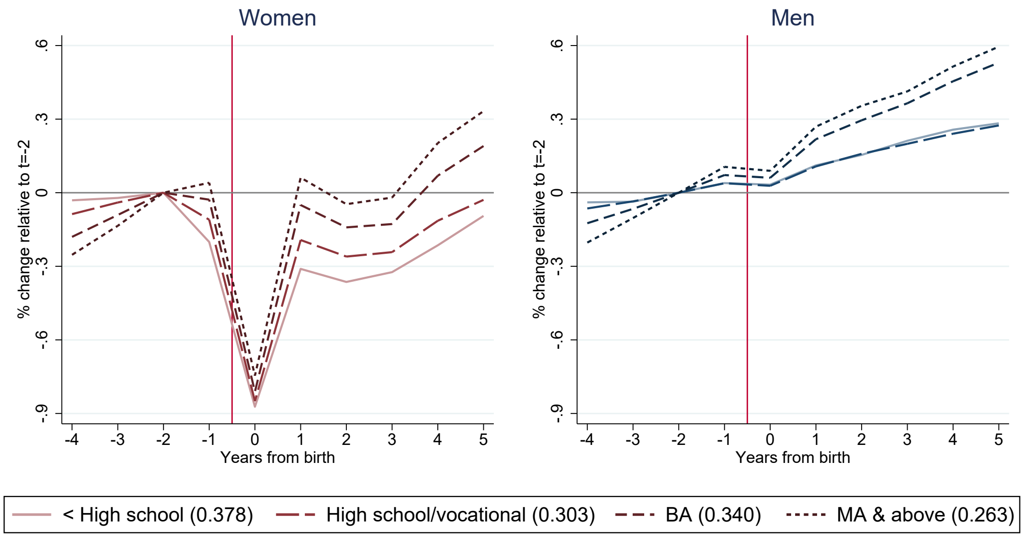


Notes: Panels (a) and (b) show the share of first births by mothers' and fathers' age at first birth. Panels (c) and (d) show years from graduation at first birth. Panels (e) and (f) show cumulative share of first births by mothers' and fathers' years from graduation. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample.

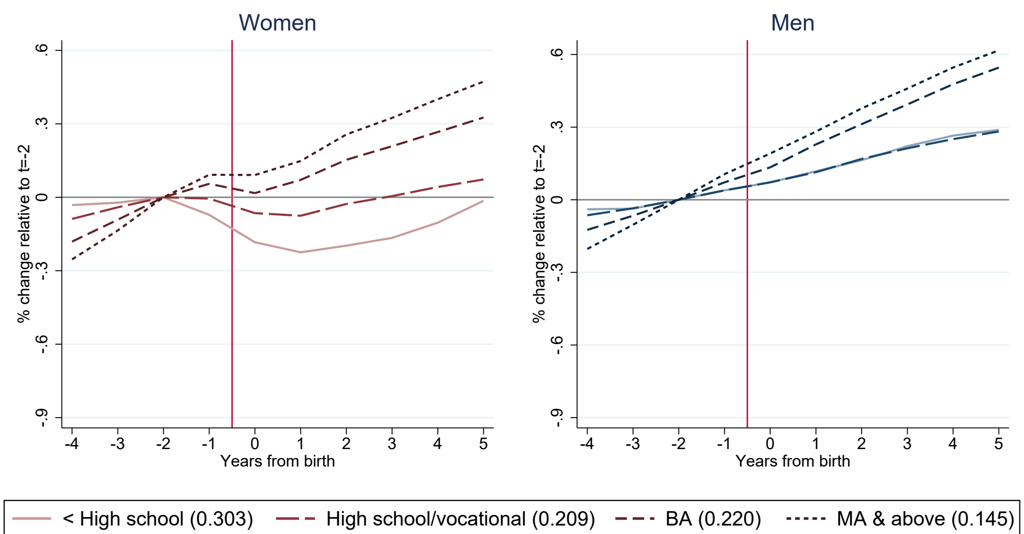
A.2 Child penalties

Figure A.2: Earnings around first birth

(a) Earnings (set to 0 during PL)



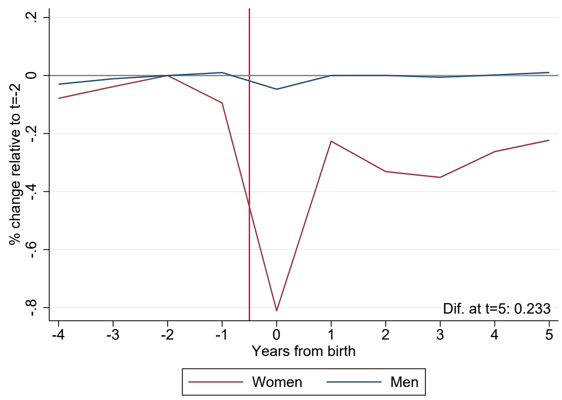
(b) Earnings (set at pre-PL levels during PL)



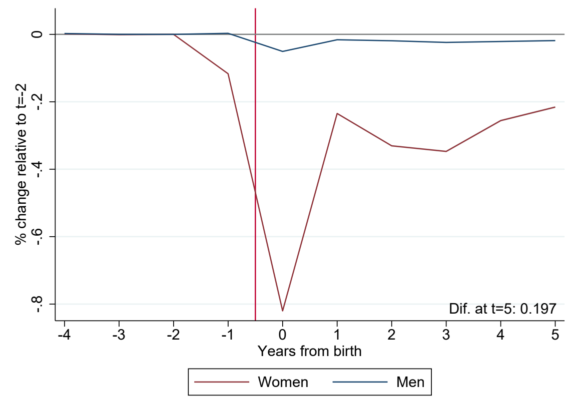
Notes: Panel (a) show earnings relative to two years before first birth when any earnings during spells of parental leave are set equal to zero. Panel (b) show earnings relative to two years before first birth when earnings during spells of parental leave are set equal to the level of earnings immediately preceding a spell of parental leave. Percentage point differences at year 5 between women and men with the same education level are included in parentheses in the legend. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample.

Figure A.3: Child penalties, all education levels:
No controls for education and years from graduation

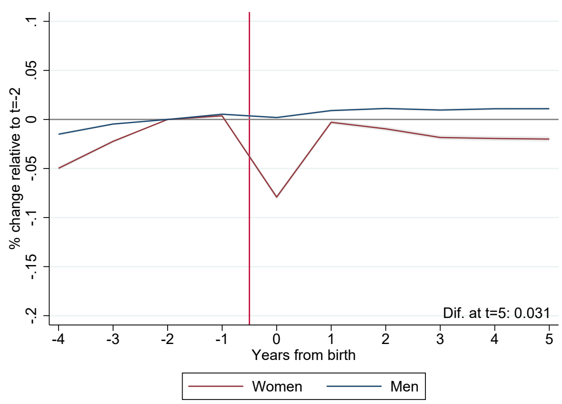
(a) Earnings



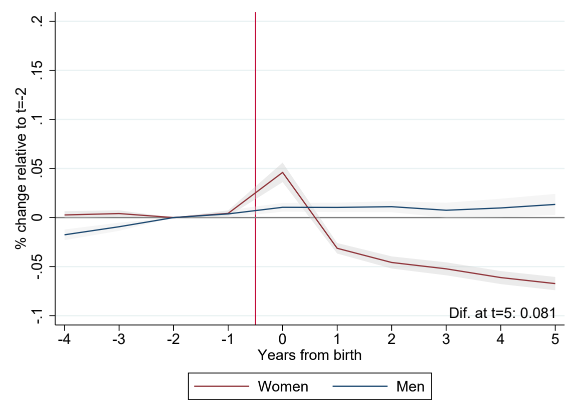
(b) Participation



(c) Hours worked

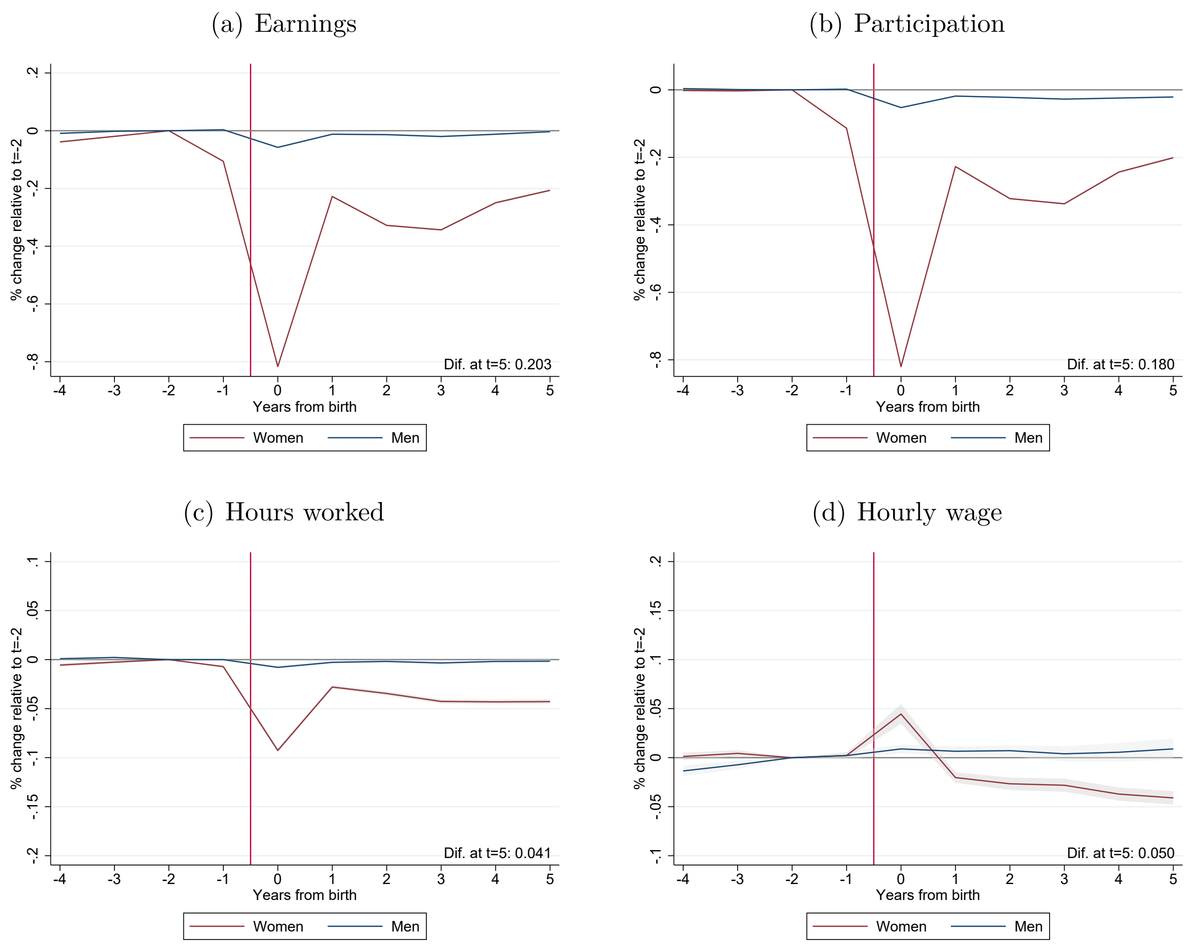


(d) Hourly wage



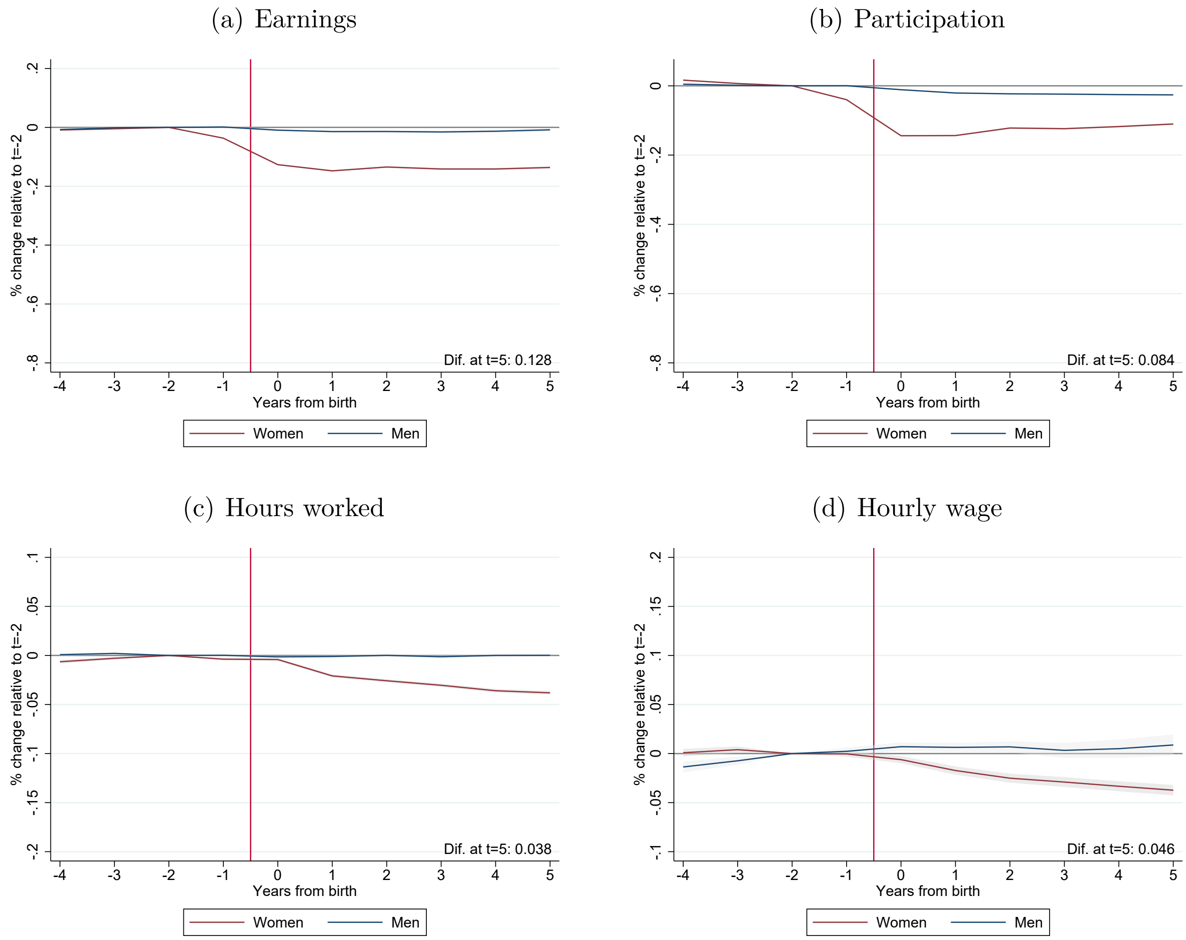
Notes: This figure shows estimated child penalties across education levels without controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly wage. Hours worked and hourly wages are conditional on strictly positive hours worked. Gender differences in estimated penalties are included as notes in the lower right corner of each panel. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample. 95% confidence intervals indicated.

Figure A.4: Child penalties, all education levels:
With controls for education level interactions with age and years from graduation



Notes: This figure shows estimated child penalties across education levels with controls for age and years from graduation interacted by education level, but treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly wage. Hours worked and hourly wages are conditional on strictly positive hours worked. Gender differences in estimated penalties are included as notes in the lower right corner of each panel. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample. 95% confidence intervals indicated.

Figure A.5: Child penalties, all education levels:
Adjusting outcomes during parental leave

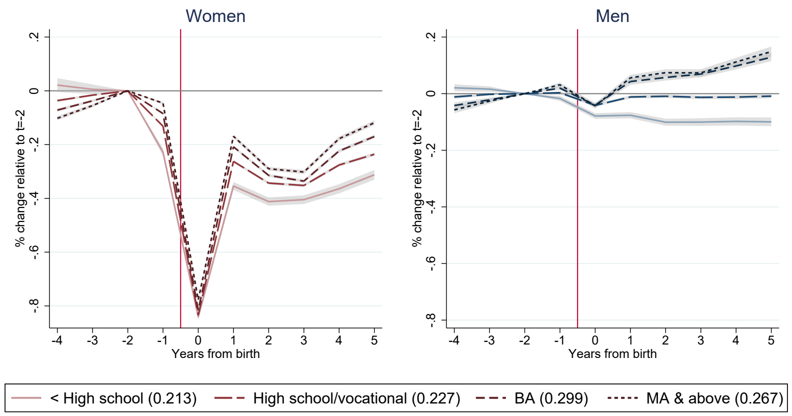


Notes: This figure shows estimated child penalties across education levels with controls for age and years from graduation interacted by education level, and adjusting outcomes during spells of parental leave to be equal to levels in the month immediately prior to the spell of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly wage. Hours worked and hourly wages are conditional on strictly positive hours worked. Gender differences in estimated penalties are included as notes in the lower right corner of each panel. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample. 95% confidence intervals indicated.

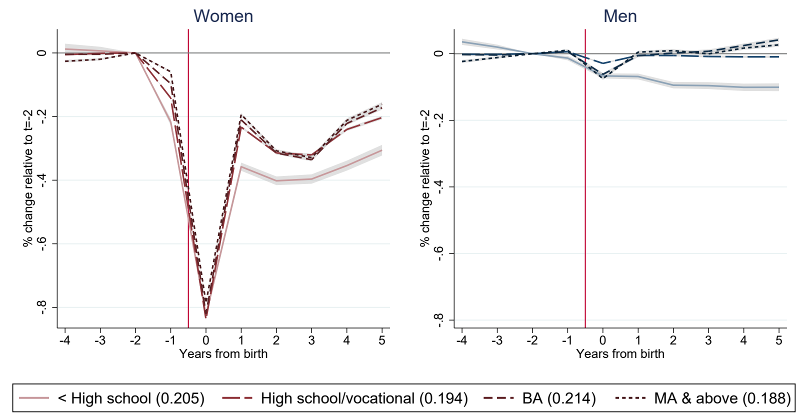
Figure A.6: Child penalties by education

Without controls for years from graduation; Earnings, participation and hours = 0 during PL

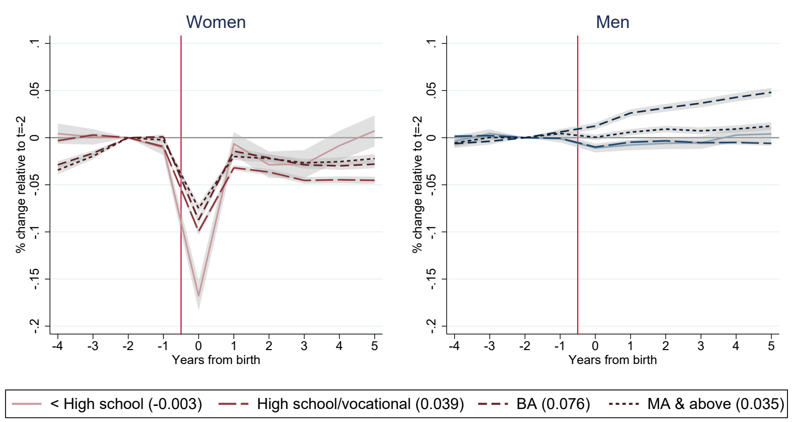
(a) Earnings



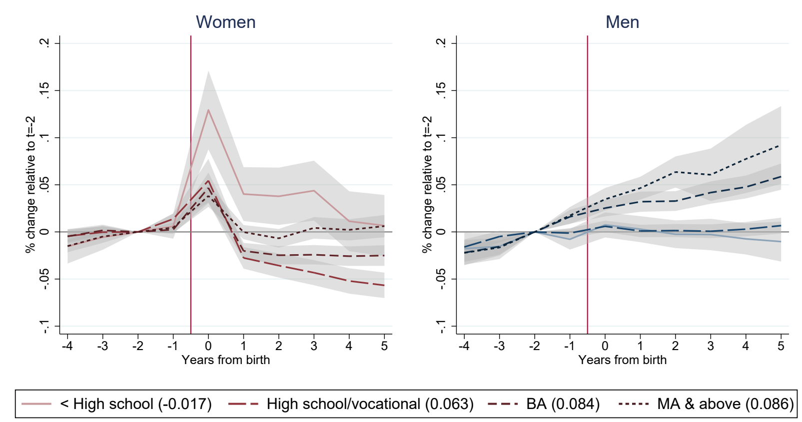
(b) Participation



(c) Hours worked



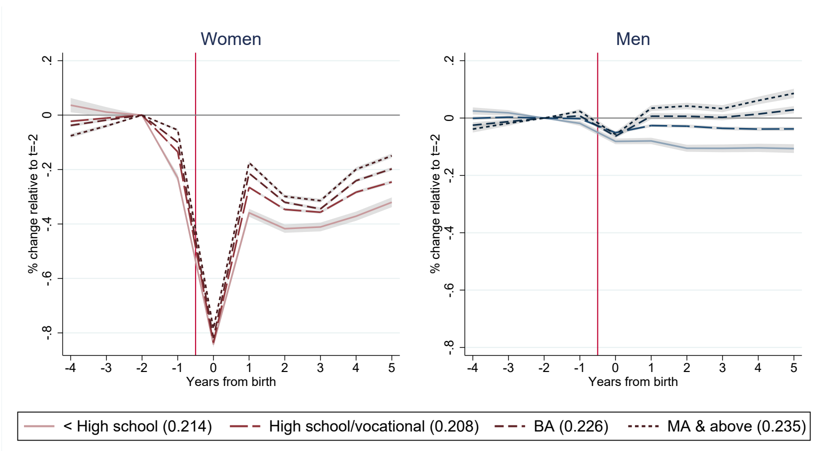
(d) Hourly wages



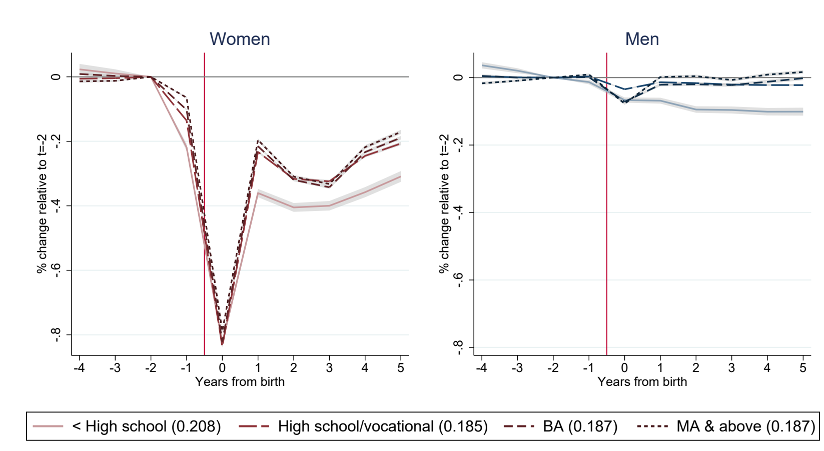
Notes: This figure illustrates estimates of child penalties by education level without controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly wage. Hours worked and hourly wages are conditional on strictly positive hours worked. Gender differences in child penalties by year 5 after first birth are included in legends. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample. 95% confidence intervals indicated.

Figure A.7: Child penalties by education levels:
With controls for years from graduation; Earnings, participation and hours = 0 during PL

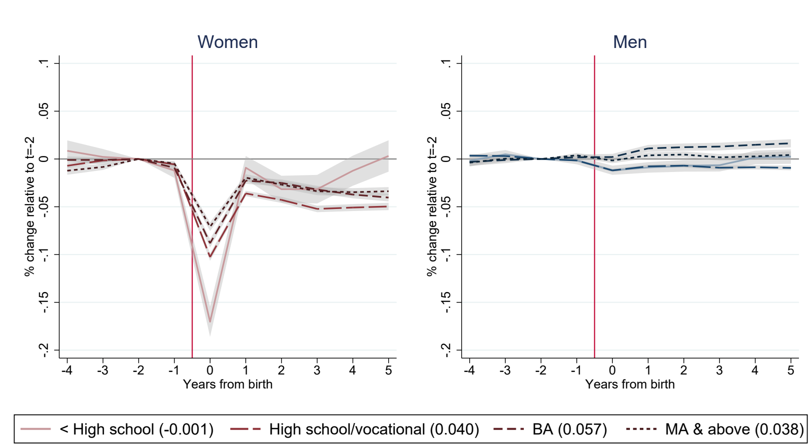
(a) Earnings



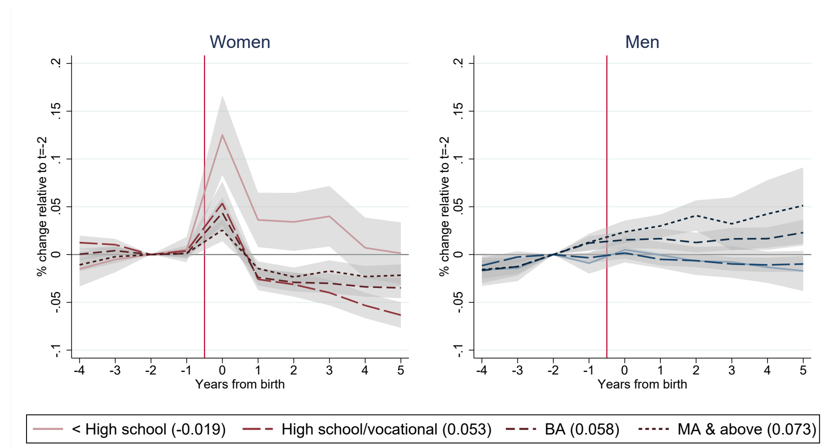
(b) Participation



(c) Hours worked



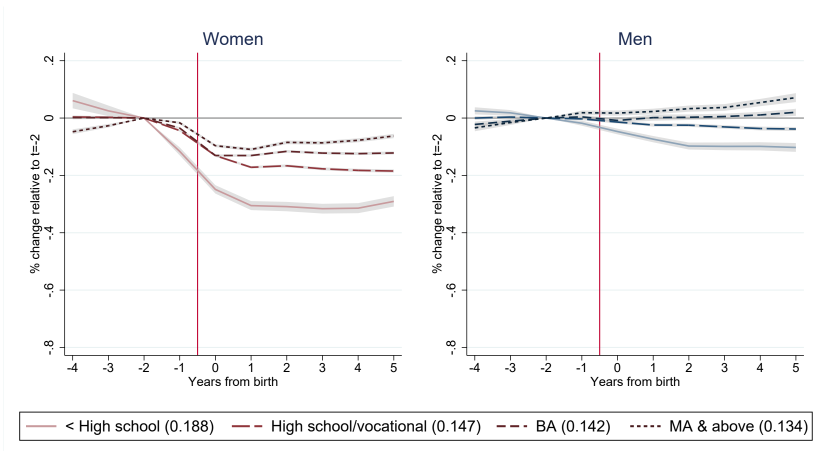
(d) Hourly wages



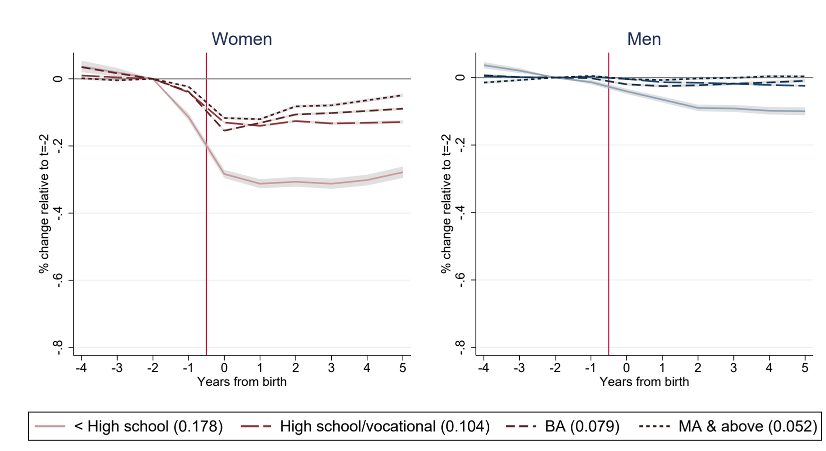
Notes: This figure illustrates estimates of child penalties by education level with controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly wage. Hours worked and hourly wages are conditional on strictly positive hours worked. Gender differences in child penalties by year 5 after first birth are included in legends. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample. 95% confidence intervals indicated.

Figure A.8: Child penalties by education levels:
With controls for years from graduation; Earnings, participation and hours during PL set at pre-PL levels

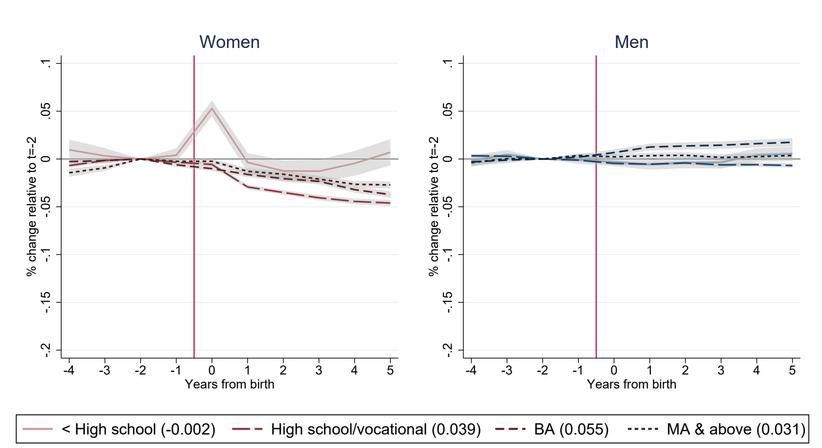
(a) Earnings



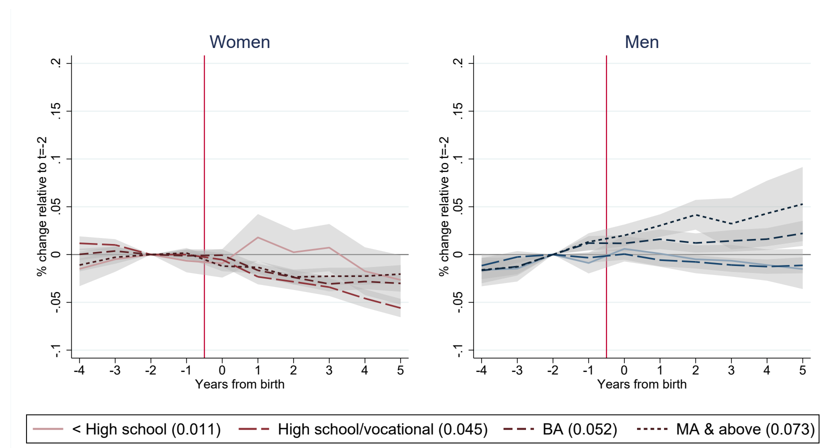
(b) Participation



(c) Hours worked



(d) Hourly wages



Notes: This figure illustrates estimates of child penalties by education level with controls for years from graduation and adjusting outcomes during spells of parental leave to be equal to levels in the month immediately prior to the spell of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly wage. Hours worked and hourly wages are conditional on strictly positive hours worked. Gender differences in child penalties by year 5 after first birth are included in legends. All analyses are undertaken separately by observed education levels at the end of the panel. See Table A.1 for details on the sample.

B Robustness: Grouping by high school completion/grades

Table B.1: Summary statistics by high school grade group, 12 months before 1st birth

Women

	No high school	Grade<p(33)	p(33)<=Grade<p(67)	p(67)<=Grade	All
Age	26.39	27.63	28.17	28.85	27.58
Married	0.14	0.17	0.20	0.25	0.18
Years from graduation	3.76	1.89	1.77	1.52	2.45
Employment	0.74	0.85	0.87	0.86	0.82
Share of month on benefit, ex. PL	0.32	0.28	0.26	0.26	0.29
Hours per month, inc. zeros	102.85	116.38	119.86	118.28	112.75
Hours per month, ex. zeros	138.65	137.45	137.37	137.44	137.82
Part-time	0.25	0.24	0.23	0.23	0.24
Earnings per month	16254.90	20081.80	22255.83	24855.88	20249.53
ln(earnings)	2.92	2.98	3.04	3.14	3.01
Hourly wage	165.94	177.32	188.12	208.50	183.40
ln(hourly wage)	5.02	5.11	5.17	5.28	5.13
High school grade rank	.	16.18	48.58	82.07	49.87
Number of children, end year 5	1.76	1.87	1.93	1.98	1.87
N	20815	11737	13032	12802	58386

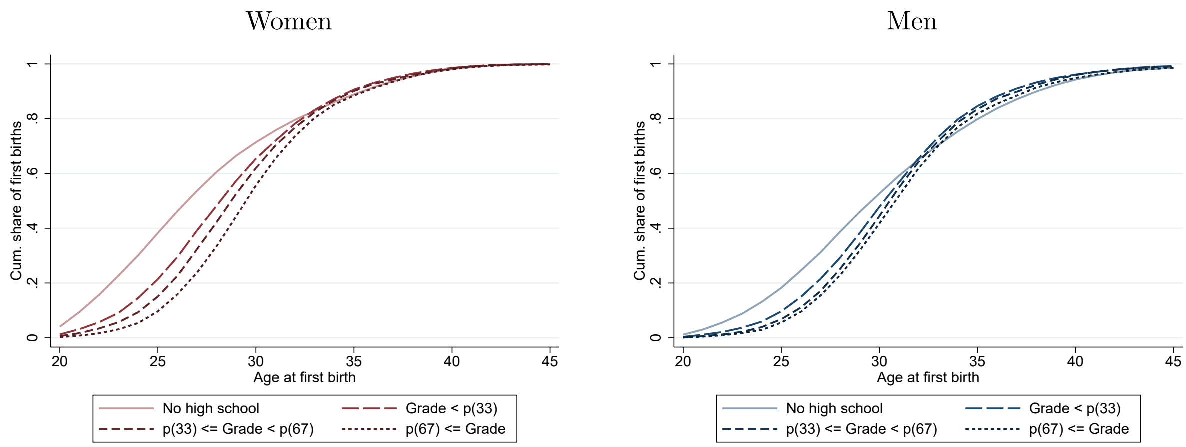
Men

	No high school	Grade<p(33)	p(33)<=Grade<p(67)	p(67)<=Grade	All
Age	29.17	29.55	29.95	30.28	29.48
Married	0.17	0.19	0.22	0.26	0.19
Years from graduation	6.57	3.06	2.73	2.50	4.98
Employment	0.83	0.87	0.88	0.89	0.85
Share of month on benefit, ex. PL	0.19	0.20	0.19	0.19	0.19
Hours per month, inc. zeros	122.86	126.66	127.77	129.19	124.95
Hours per month, ex. zeros	148.05	145.45	145.89	145.54	147.01
Part-time	0.19	0.18	0.17	0.17	0.18
Earnings per month	24390.88	25561.53	28294.02	31810.88	26124.70
ln(earnings)	3.24	3.19	3.29	3.36	3.26
Hourly wage	204.12	203.43	221.85	242.37	212.11
ln(hourly wage)	5.23	5.24	5.31	5.40	5.27
High school grade rank	.	15.81	48.22	82.37	48.93
Number of children, year 5	1.78	1.89	1.96	1.99	1.85
N	32375	7563	7779	7653	55370

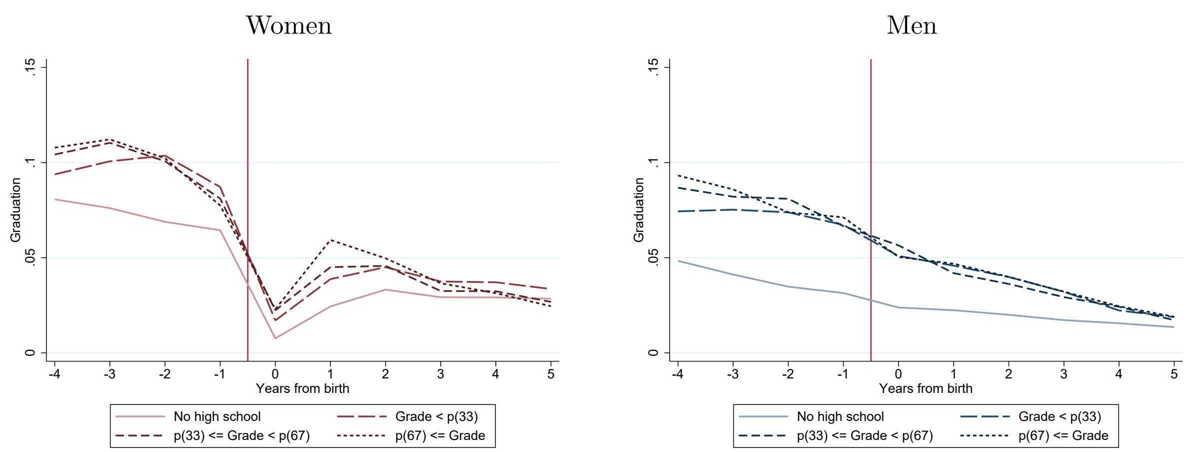
Notes: All characteristics are measured 12 months before first birth. Employment is defined as strictly positive hours of work. Share of month on benefits include all benefits except parental leave and are defined as days registered on benefits divided by total number of days in the given month. Part-time is defined as working less than 130 hours per month. High school grade ranks are defined within exam cohorts. Sample is split by skill level as defined by high school completion and grade rank.

Figure B.1: Age and graduation rates around first birth

(a) Cumulative share of parents



(b) Graduation rates

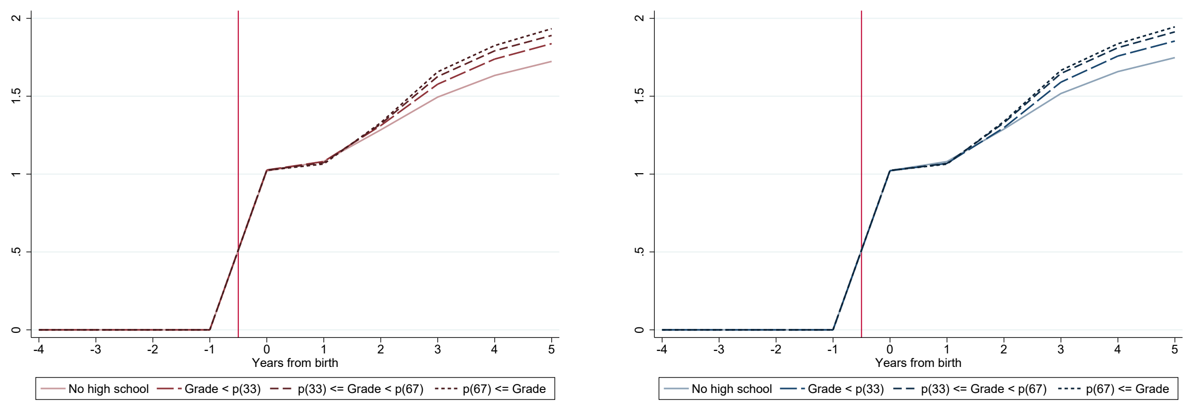


Notes: Panels (a) and (b) show the cumulative share of first births by mothers' and fathers' age (the corresponding density functions are shown in panels (a) and (b) of Appendix Figure B.4). Panels (c) and (d) show the probability of graduating in a given year around first birth (Appendix Figure B.4 shows years from graduation at first birth). All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table B.1 for details on the sample.

Figure B.2: Cumulative Fertility

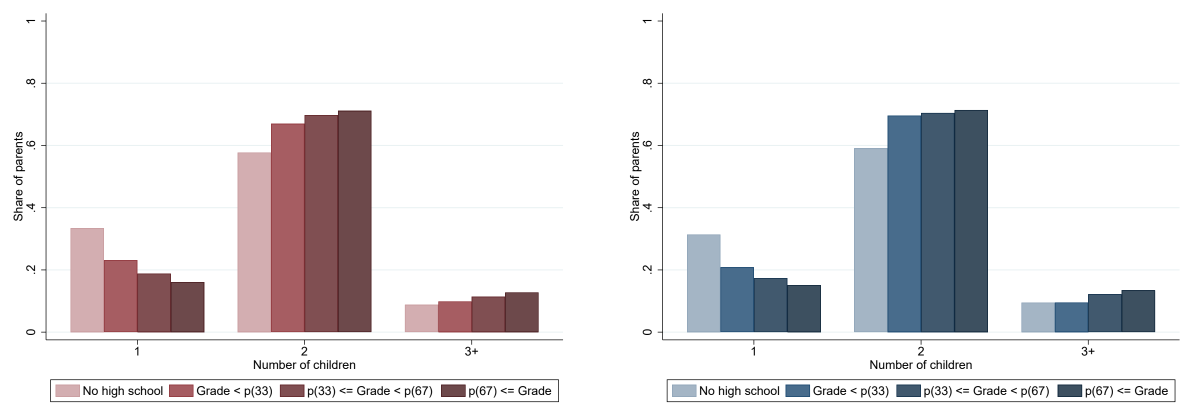
(a) Number of children by year from first birth

Women Men



(b) Number of children 5 years from first birth

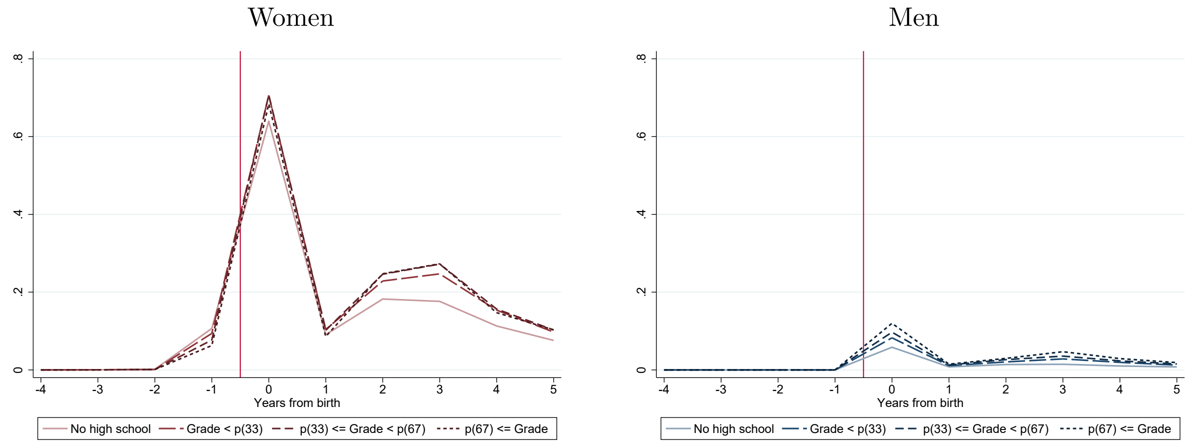
Women Men



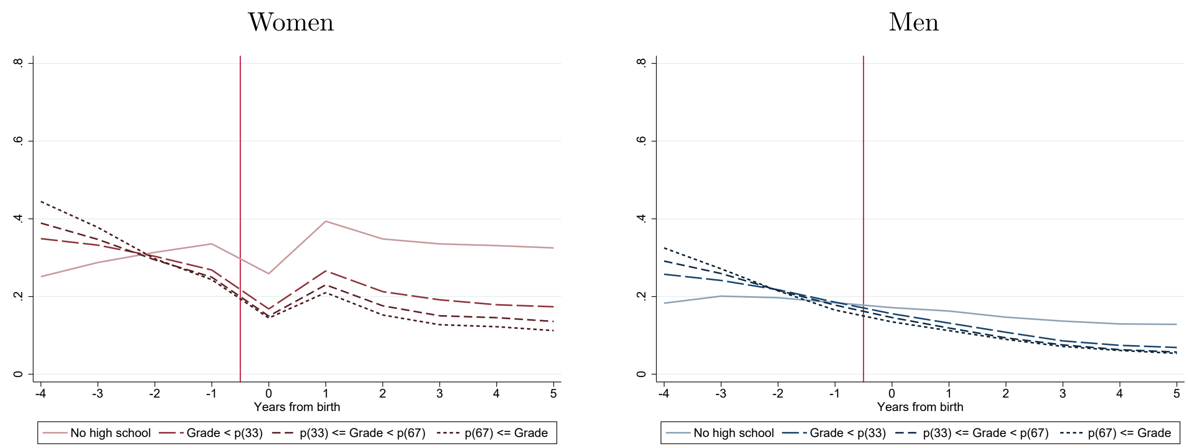
Notes: Notes: Panel (a) shows the cumulative number of children each year around first birth. Panel (b) shows the distribution of the number of children by the end of year 5 after first birth. All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table B.1 for details on the sample.

Figure B.3: Parental leave and benefit spells around first birth

(a) Share on parental leave



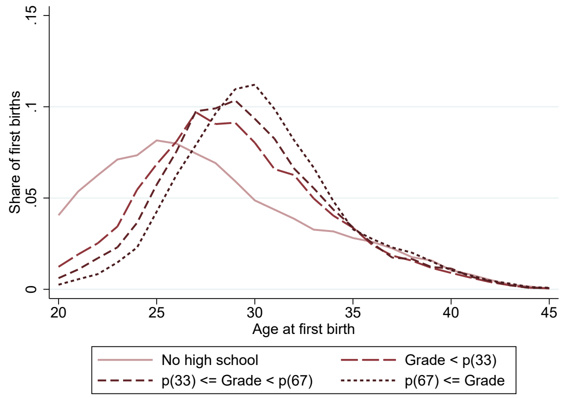
(a) Share on other benefits



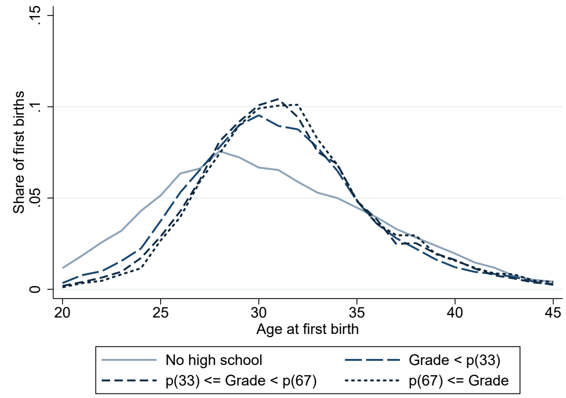
Notes: Panel (a) shows the average share of each year spent on parental leave. Panel (b) shows the average share of each year spent on other public subsistence benefits (from the OF-register). Note that most students in further education in Denmark are eligible for monthly student benefit payments from the government; these benefits are also included here. All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table B.1 for details on the sample.

Figure B.4: Age at first birth

(a) Women: Share of 1st births



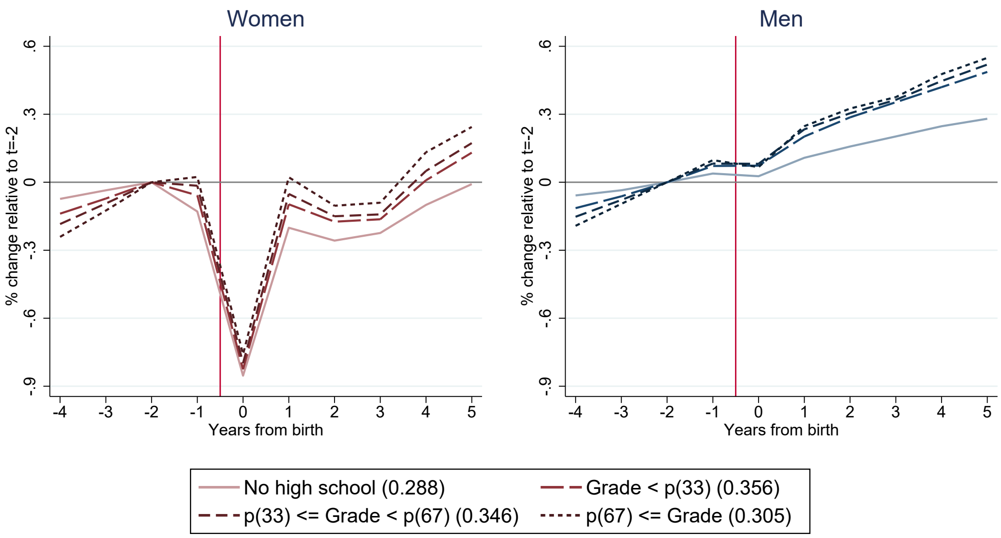
(b) Men: Share of 1st births



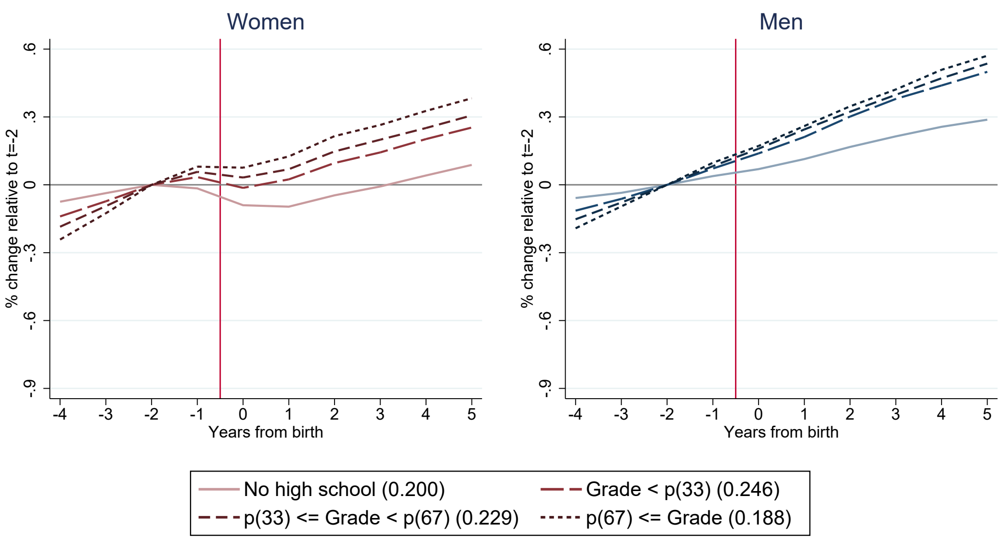
Notes: Panels (a) and (b) show the share of first births by mothers' and fathers' age at first birth. All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table B.1 for details on the sample.

Figure B.5: Earnings around first birth

(a) Earnings (set to 0 during PL)



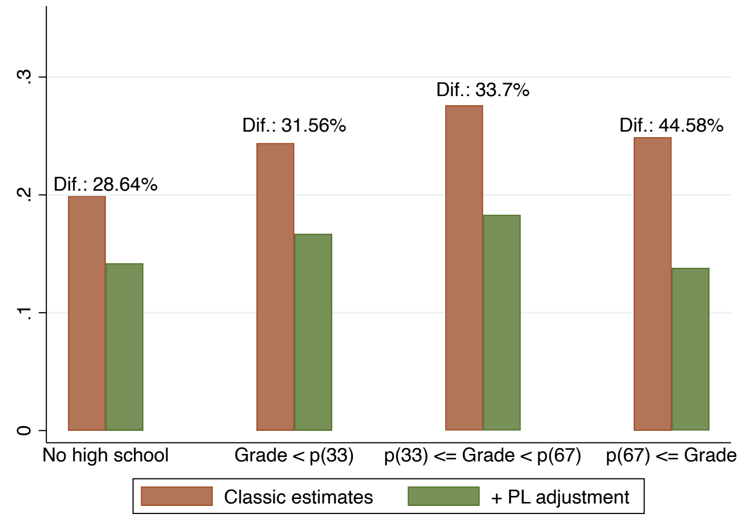
(b) Earnings (set at pre-PL levels during PL s)



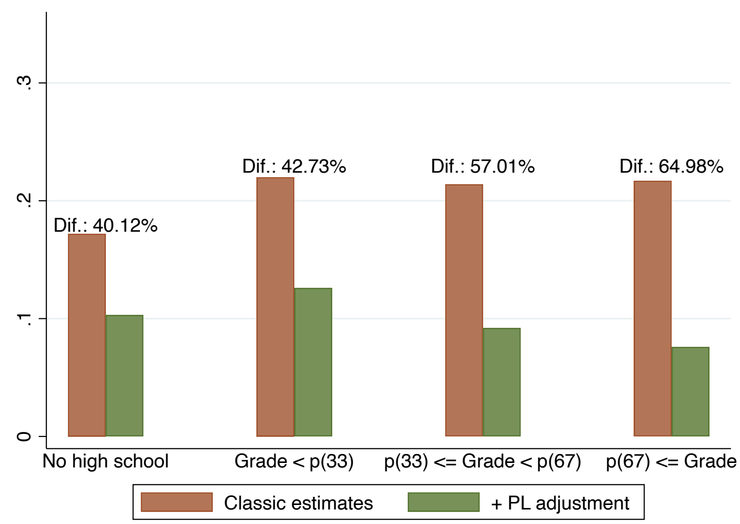
Notes: Panel (a) show earnings relative to two years before first birth when any earnings during spells of parental leave are set equal to zero. Panel (b) show earnings relative to two years before first birth when earnings during spells of parental leave are set equal to the level of earnings immediately preceding a spell of parental leave. Percentage point differences at year 5 between women and men with the same education level are included in parentheses in the legend. All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table B.1 for details on the sample.

Figure B.6: Alternative specifications for estimating long-run child penalties

(a) Earnings



(b) Participation



Notes: This figure includes estimates of child penalties by year 5 after first birth from various specifications. Brown bars show estimates of child penalties by skill level without controls for years from graduation and treating outcomes as zeros during spells of parental leave. Green bars show estimates when adjusting outcomes during spells of parental leave to be equal to levels in the month immediately prior to the spell of parental leave without controls for years from graduation. We do not control for years from graduation when considering skill levels defined by high school participation/grades to show that our results are unlikely to be driven by endogenous fertility preferences. All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table B.1 for details on the sample.

C Robustness: Parental cohorts born 1970-1975

Table C.1: Number of children by age 45, cohorts born 1970-1975

Women

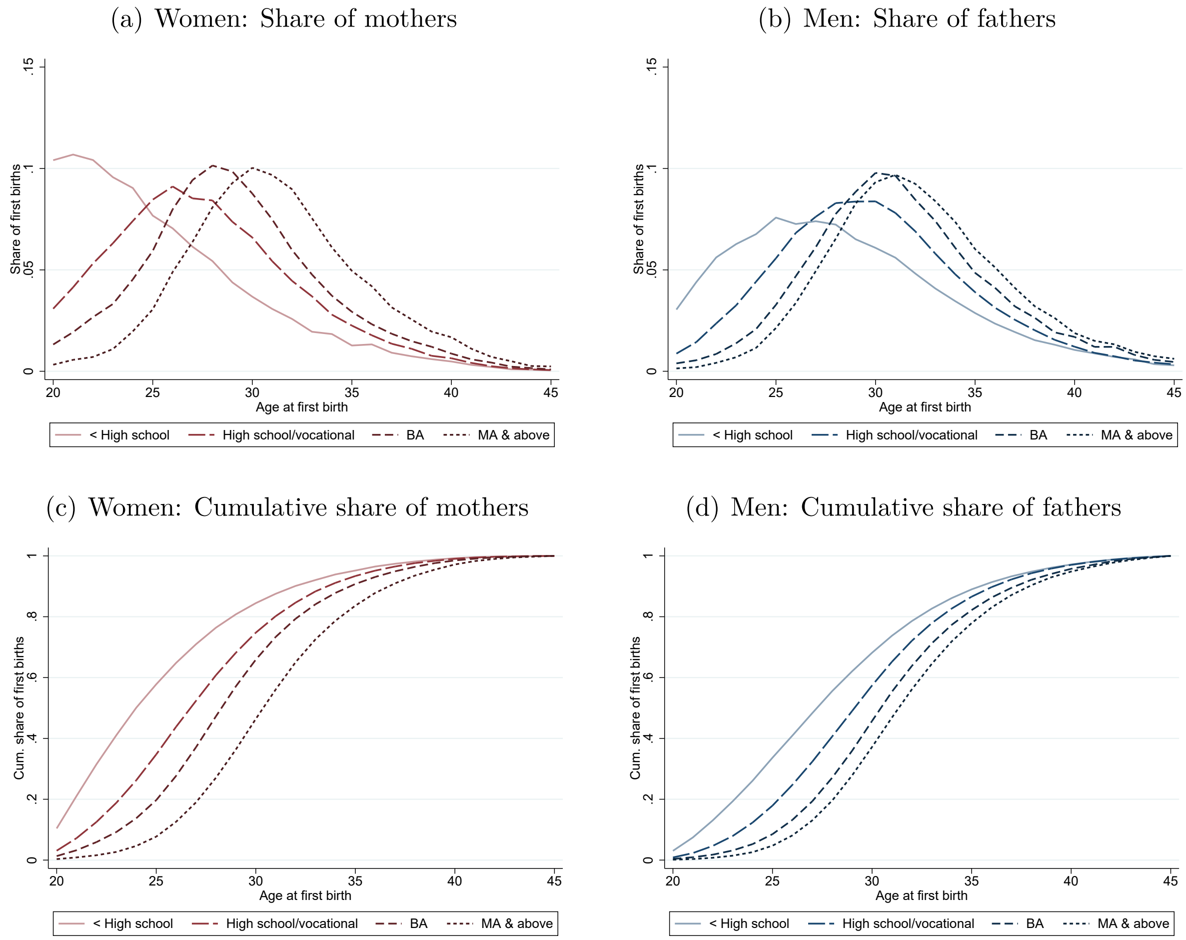
	< HS		HS/voca.		BA		MA & above		All	
No. of children	Count	Share	Count	Share	Count	Share	Count	Share	Count	Share
0	5618	0.20	13390	0.13	6420	0.11	4455	0.13	29883	0.13
1	4701	0.17	16307	0.16	8477	0.14	4780	0.14	34265	0.15
2	9728	0.34	48882	0.48	28741	0.48	16053	0.48	103404	0.46
3	5267	0.19	18506	0.18	13920	0.23	6917	0.21	44610	0.20
4 or more	2992	0.11	5035	0.05	2840	0.05	1028	0.03	11895	0.05
Total	28306	1.00	102120	1.00	60398	1.00	33233	1.00	224057	1.00

Men

	< HS		HS/voca.		BA		MA & above		All	
No. of children	Count	Share	Count	Share	Count	Share	Count	Share	Count	Share
0	12958	0.31	25771	0.21	5423	0.17	5258	0.16	49410	0.22
1	7135	0.17	19361	0.16	4262	0.13	3800	0.12	34558	0.15
2	12012	0.29	50665	0.41	14011	0.44	14239	0.44	90927	0.40
3	6335	0.15	21484	0.17	6747	0.21	7318	0.23	41884	0.18
4 or more	3633	0.09	6289	0.05	1533	0.05	1439	0.04	12894	0.06
Total	42073	1.00	123570	1.00	31976	1.00	32054	1.00	229673	1.00

Notes: In this table, we consider all individuals residing in Denmark (i.e. observable in BEF) at age 45 and born from 1970 to 1975. We measure education level and number of children at age 45.

Figure C.1: Age at first birth



Notes: Panels (a) and (b) show the share of first births by mothers' and fathers' age at first birth. Panels (c) and (d) show the cumulative share of first births by mothers' and fathers' age. All analyses are undertaken separately by observed education levels at age 45. See Table C.1 for details on the sample.

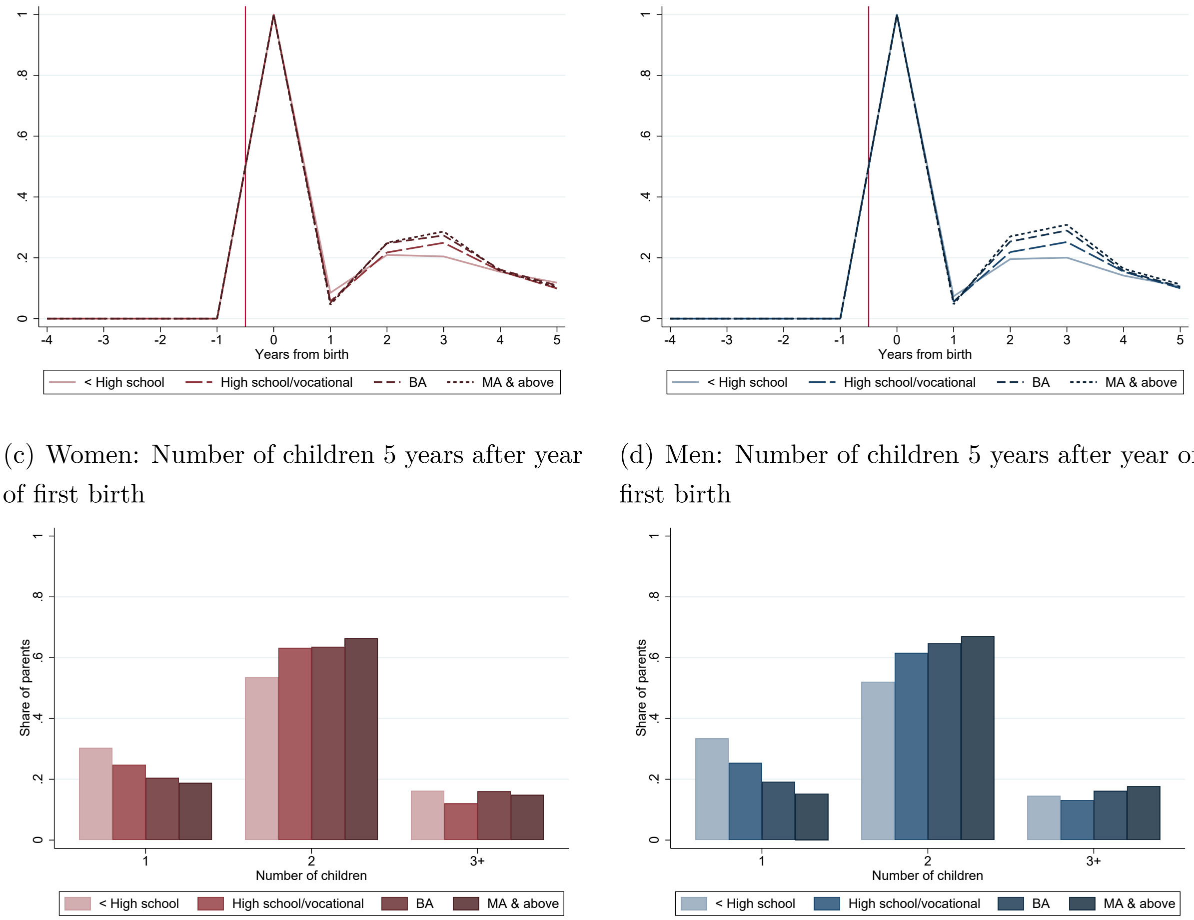
Figure C.2: Fertility and parental leave around first birth

(a) Women: Probability of birth

(b) Men: Probability of birth

(c) Women: Number of children 5 years after year of first birth

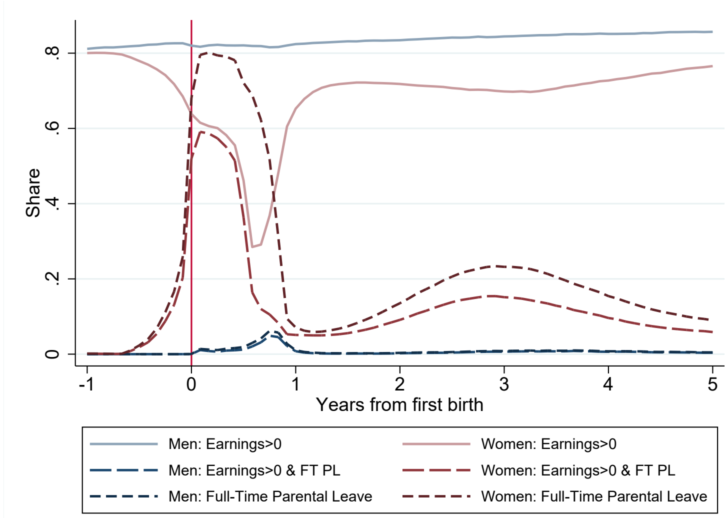
(d) Men: Number of children 5 years after year of first birth



Notes: Panels (a) and (b) show the probability of birth/having a child around first birth. Panels (c) and (d) show the distribution of number of children 5 years after year of first birth. All analyses are undertaken separately by observed education levels at the end of the panel. See Table C.1 for details on the sample.

D Data details

Figure D.1: Earnings & parental leave benefits around first birth:
Co-occurrence with employment



Notes: This figure highlights a technical detail in the Danish administrative registers. The figure shows that strictly positive earnings and parental leave overlap in registers during the period with full salary replacement. In the employment register (BFL), we see that during parental leave with full salary replacement (the first few months of parental leave) the majority of employees are still recorded with positive earnings. Note that strictly positive earnings also imply strictly positive hours of work in the BFL-register. When transitioning to parental leave benefits paid directly from the government, recorded labor market earnings reduce significantly. As such, using information on labor market outcomes directly from the BFL-register will lead to an inconsistent treatment of labor market outcomes during spells of parental leave, even though their actual number of hours worked remain zero, and they receive government-paid parental leave benefits throughout (directly and indirectly). "FT PL" refers to full-time parental leave. Full-time parental leave means that all days of the given month are recorded as leave.

E Literature Review

Table E.1: Summary of Child Penalty Literature Review

Authors (Cites)*	**Context**	**Parental leave classified as employment**	**Paid parental leave included in earnings**
Kleven et al. (2019b) (1691)	Denmark	Partly	Partly
Kleven et al. (2024a) (79)	Cross-country	Sometimes	N/A
Kleven et al. (2019a) (832)	Denmark, Sweden, US, UK, Germany, Australia	?	?
Cortés & Pan (2023) (238)	US	Yes	Yes
Andresen & Nix (2022) (126)	Norway	No	Yes
Lundborg et al. (2017) (375)	Denmark	?	?
Fitzenberger et al. (2013) (118)	Germany	?	N/A
Fernández-Kranz et al. (2013) (115)	Spain	?	?
Agüero & Marks (2008) (260)	Peru, Guatemala, Colombia, Bolivia, Nicaragua, Dominican Rep.	?	N/A
Angelov et al. (2016) (732)	Sweden	?	?
Kuziemko et al. (2018) (280)	US and UK	No	N/A
Hotz et al. (2018) (92)	Sweden	Yes	Yes
Adda et al. (2017) (816)	Germany	No	Yes

*Citations taken from Google Scholar on 18 October 2024.