Measuring labor market dynamics around childbirth: The role of parental leave*

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This version June 18, 2026

Abstract

How labor market outcomes are measured during parental leave has a first-order, often overlooked, effect on estimates of child penalties. Using Danish administrative data, we show that more educated mothers have their first child later but space subsequent births more tightly, spending substantially more time on parental leave in the years following first birth. In event studies by skill, much of the medium-run penalty in earnings and participation, especially for the highly educated, reflects leave around higher-order births. Adjusting for skill, graduation timing, and a consistent treatment of parental leave reduces the five-year earnings penalty by almost half, to 12.8%.

JEL classifications: J13, J16

Keywords: fertility, child penalty, parental leave

*We thank Alison Andrew, Oriana Bandiera, Moira Daly, Fane Groes, Thomas Jørgensen, Camille Landais, Henrik Kleven, Amalia Miller, Jakob Søgaard, Ning Zhang and seminar participants in AEA, Bocconi, Mannheim, Oxford, and Warwick for helpful comments and discussions. Adams thanks the European Research Council for funding through grant number 948070 and the Leverhulme Foundation. Jensen thanks the Independent Research Fund Denmark for funding through grant number 1058-00011B. Petrongolo thanks the Economic and Social Research Council for funding through grant number ES/T014431/1. The order of authors is alphabetical.

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1 Introduction

Women's role as the primary caregiver is a key hurdle to their entry into and retention in paid employment. Evidence from several countries shows that, while parenthood is roughly neutral – or even beneficial – to the careers of men, it drives large and persistent setbacks in the careers of women. This line of research has gained significant momentum in labor and family economics, driven by methodological innovations, access to richer data, and new attention to the role of gender identity norms in shaping economic decisions (Olivetti et al., 2024). Recent approaches in this literature have leveraged life-cycle models of occupational choices, family formation and labor supply (e.g. Adda et al., 2017), instrumental variables for fertility (e.g. Lundborg et al., 2017; Bensnes et al., 2023), and event studies centered around childbirth (e.g. Angelov et al., 2016; Kleven et al., 2019). While available approaches differ on assumptions and strengths and weaknesses, there is broad consensus that parenthood drives widening gaps in parental earnings.

Our paper tackles an important, though largely overlooked, issue at the core of this literature, namely the consistent measurement of the economic outcomes of interest after childbirth. We argue that this issue is central to interpreting the career cost of children, especially when the spacing of births varies systematically with age at first birth.

Around childbirth, participation and earnings can be measured in different ways depending on whether a parent is actively working or on job-protected parental leave. Consider a new parent who takes job-protected parental leave following the birth of their child before returning to their pre-birth job. During leave, they may be entitled to parental leave benefits, which may, in turn, combine employer contributions and government transfers. A key issue in estimating the impact of childbirth on labor market outcomes is whether parents should be treated as employed or non-employed during parental leave and whether their earnings should be measured as usual earnings, zero, or a combination of the two, depending on actual parental leave benefits. This issue has implications for analyzing and interpreting post-birth labor market trajectories, particularly because mothers spend substantial periods on parental leave following first and subsequent births.

The appropriate measurement choice depends on which economic quantity we want to capture, mapping onto efficiency and equity readings of the impact of childbirth. The first concerns the loss of productive labor input: while a parent is on leave, actual hours worked and output fall to zero, irrespective of any entitlement to return to work, salary replacement, and benefits paid. Treating outcomes as zero during leave captures the loss of hours – the efficiency cost of childbirth, in the sense of market work that is not undertaken while parents care for children. The second concerns the potential adjustments in labor supply induced by childbirth, net of the temporary and – to some extent – mechanical incapacitation of parental leave. Compared to fathers, mothers may adjust their labor supply more persistently after childbirth. This differential adjustment – rather than the temporary withdrawal of hours while on parental leave – speaks directly to the unequal labor market impacts of childbirth across genders, independently of entitlements to parental leave. Setting labor market outcomes at their pre-leave level for the duration of a parental leave spell isolates this margin.

While the appropriate choice of measurement of labor market outcomes depends on the specific research objective, the existing literature on the career costs of children is often unclear or inconsistent in how it measures labor market outcomes during parental leave. In this paper, we use Danish administrative data to examine whether these measurement choices materially affect estimates of child penalties in the medium run and propose a consistent measurement of the outcomes of interest during parental leave. To do so, we analyze the dynamics of parental participation and earnings around childbirth using an event-study design. Our sample includes over 74,000 first-born children born between 2012 and 2014, and we track their parents' outcomes over a 10-year period centered around 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,[1] in the medium run, this is driven by higher-order births. We document 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 their first birth, compared to just 60% in the least educated group.

This variation reflects two related patterns. First, while highly skilled women historically had fewer children than less skilled women in many high-income countries, the negative gradient between completed fertility and women's market skills has flattened in recent decades, and even reversed towards the top of the skill distribution (Doepke et al., 2023; Virtanen et al., 2024). Second, more educated mothers typically have their first child at older ages than the less educated, often close to graduation: the average woman in the least educated group in Denmark is 25.4 years old at first birth, compared to 30.8 years for the average woman with a university degree. These findings are in line with evidence from comparable countries; see e.g. OECD (2024) for an overview. As the ability to conceive declines with age, higher-educated women who realize similar completed fertility tend to space multiple births more closely because they have their first child later in life.

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

Heterogeneity in the spacing of births creates significant differences in the likelihood that a mother is on parental leave in the five years following her first birth by education group. We find that the most educated mothers on average spend 25% of that time on leave, 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.

We then show that these dynamics are relevant for harnessing event-study designs to estimate the child penalty, measuring the degree to which female outcomes fall behind male outcomes 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 doing so, 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. First, systematic variation in age at first birth by skill level and the clustering of first births soon after 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 university 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 that correlations between age, graduation, and time of first birth differ substantially between women and men.

Second, because women spend a substantial fraction of their time on parental leave for higher-order births, even medium-run estimates of the child penalty are affected by how parental leave is treated when measuring labor market outcomes. We provide plausible bounds on the impact of different approaches to treating parental leave on the child penalty. As a lower bound, we set labor market outcomes to zero while on leave. This measure is appropriate if one is interested in the value of productive work supplied to the labor market. As an upper bound, we set an individual's outcomes equal to those observed in the month before they start parental leave for the entire duration of the leave, as these are the terms to which parents are entitled to return. This approach is appropriate when researchers are interested in labor market attachment and the characteristics of jobs held by individuals, net of the incapacitation effects of leave. This is the margin emphasized in a growing body of work on the role of family-(un)friendly workplace practices and persistent social norms in shaping the unequal gender consequences of parenthood (for an overview, see Olivetti et al. 2024). It is also a highly relevant margin in policy discussions that overwhelmingly focus on preserving mothers' connection to the labor force – often emphasizing job-protected and non-disruptive leave arrangements as a potential way to do so.[2] Consistent with this view, ILO guidelines suggest that job-protected parental leave should be assimilated to employment. As salary replacement rates during parental leave are high in the Danish context, our upper-bound measure is close to total income, including all transfers and benefits during parental leave. In institutional contexts with less generous parental leave benefits, total income will fall between our two bounds.

We find a substantial gap in the five-year child penalty implied by these bounds, with markedly heterogeneous patterns across education groups. The different approaches affect estimates of the child penalty directly through the measurement of the outcome variable, and indirectly via the impact on estimated age-earnings profiles. Relative to the standard specification, which implies a five-year earnings penalty of 23.3%, our preferred specification (which includes interactions of education controls with age and years since graduation) reduces this to 20.3% under the lower-bound measure and to 12.8% under the upper-bound measure – a 45% reduction. The corresponding penalties in participation rates are 19.7%, 18.0%, and 8.4%, respectively – an even larger reduction of 57%. Since birth spacing is shorter, parental leave take-up is higher, and pre-leave participation rates and earnings are greater among higher-educated mothers, the estimated penalties for this group are particularly sensitive to the treatment of parental leave. Lastly, we show that child penalties in total personal income – combining labor earnings with government transfers – are substantially smaller, 9.0%, in our preferred specification. This reflects that transfers fully offset the penalty for the least educated, whose pre-leave earnings are close to the cap on government-paid parental leave benefits, whereas penalties remain substantial for more educated mothers.

By establishing that the interaction between fertility patterns and parental leave policies is central to estimating child penalties, 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.

We establish that a sizable share of estimated child penalties reflects the incapacitation effects of parental leave rather than persistent labor supply adjustments following career interruptions. Distinguishing between these mechanisms is particularly important for the

[2] See e.g. https://www.weforum.org/stories/2022/05/reduce-motherhood-penalty-gender-pay-gap/.

design of policies aimed at reducing child penalties. While our empirical application focuses on Denmark, the underlying measurement issue arises in any setting with substantial job-protected parental leave entitlements. Across the EU, mothers are entitled to an average of 70 weeks of paid, job-protected leave,[3] compared with 46 weeks in Denmark during our sample period. The corresponding average across OECD countries is 55 weeks. As a result, our conclusions regarding the measurement of employment and earnings during parental leave are relevant for estimating child penalties in most high-income countries. Indeed, our findings about large incapacitation effects align with evidence from evaluations of parental leave extensions in Germany (Schönberg and Ludsteck, 2014), Austria (Lalive and Zweimüller, 2009; Kleven et al., 2024), and Denmark (Jakobsen et al., 2022), which show that longer leave entitlements primarily delay mothers' return to work, while having little effect in the longer-run. Moreover, when limited public provision is supplemented by employer-provided leave, as is often the case in the US and the UK, access to leave is likely to be even more strongly correlated with worker skill than take-up of public support. Taken together, these findings highlight the importance of institutional context when comparing child penalties across countries or evaluating policy reforms. Differences in parental leave generosity can mechanically affect estimated penalties even when the underlying career consequences of parenthood are unchanged.

In our second contribution, we highlight that systematic patterns in the timing of first and subsequent births have important implications for estimating child penalties in event-study specifications. This finding complements existing work examining how skill and income relate to fertility, whether descriptively (e.g. Bailey and Hershbein, 2018) or causally (e.g. Monstad et al., 2008; Dalton et al., 2020).[4] Our paper is also related to research on the impact of delaying motherhood on labor market outcomes (Miller, 2011; Gallen et al., 2023), and it contributes to recent work on heterogeneous treatment effects in event-study specifications of child penalties (Melentyeva and Riedel, 2023; Thakral and Tô, 2026). Our work shows that differences in post-birth earnings trends by age at first birth are primarily driven by skill-level variation in birth spacing and parental leave dynamics. We account for these factors by stratifying the analysis by skill level and carefully addressing the measurement of labor market outcomes during parental leave.

Our 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, Bensnes et al., 2023, and

[3] OECD (2025), Table PF2.1.A.

[4] See Doepke et al. (2023) for a review of the recent literature on the economics of fertility.

recent discussions of this approach in Bögl et al., 2024, and Groes et al., 2024). The timing of fertility and the shape of age-earnings profiles systematically differ 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.

The rest of the paper is organized as follows. Section 2 describes our data. In Section 3, we present stylized facts on birth timing and spacing by skill, and how they relate to parental leave dynamics. In Section 4, we discuss how our stylized facts affect the estimation of child penalties, and we suggest a set of modifications to the standard event-study approach. Section 5 presents our updated estimates of child penalties by skill level and Section 6 concludes.

2 Data

We draw on Danish administrative data that cover the entire resident population and can be linked at the individual level through unique personal identifiers. The data provide high-frequency information on earnings, benefits, hours worked, parental leave, education, and family composition, allowing us to follow each parent and their children month by month around first birth. Our 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 a 10-year window centered on first birth using monthly data, which are available from 2008 onwards. 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.[5] We focus on "medium-term" outcomes 5 years following first birth as the assessment of longer-run trends in event-study contexts may require stronger assumptions and/or comparisons to never-treated control groups (see Kleven et al. 2019 for a discussion).

Fertility and demographics. We obtain demographic information from the population register (BEF), including individuals' gender, dates of birth, and parent–child links. We use these links to identify each parent's first and higher-order births and to measure cumulative fertility within our panel window.

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

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 primarily self-employed in a given year (in the AKM-register), we drop the corresponding individual-year observation from our sample. Whenever 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. During our sample period, parental leave entitlement for a newborn in Denmark is 48 weeks: 14 weeks are reserved for mothers, 2 weeks for co-parents, and 32 weeks can be shared between parents.[6] In addition, mothers are eligible for up to 8 weeks of pre-birth leave. We extract information on parental leave from the OF-register, which contains exact start and end dates of leave spells.

The first part of parental leave is covered by full salary replacement for a duration determined by the applicable collective agreement. This period hovers around 14 weeks in the private sector and is generally longer in the public sector.[7] After this period, parents are entitled to government benefits for their remaining leave, replacing their pre-leave salary up to a cap of 20,359 DKK per month in 2024 prices (approximately USD 2,964). The cap is adjusted annually for inflation, as are unemployment benefits. See, among others, Jørgensen and Søgaard (2024) for a more detailed description of parental leave provisions in Denmark.

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, again up to a cap of 20,359 DKK per month in 2024-level prices. Once entitlement to full replacement expires, benefits are paid directly by the government. Payments from a firm to a worker can therefore give an inconsistent measure of labor market attachment over a spell of parental leave.[8]

[6]In 2022, new regulations designated a larger share of leave to co-parents. In the whole analysis, we refer to rules in place during our sample period.

[7]For example, employees in administrative jobs would typically be covered by collective bargaining agreements between the HK trade union and employers. Through these agreements, mothers are also 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.

[8]Kleven et al. (2025) analyze child penalties across countries and conclude that excluding paid leave from their definition of employment has a trivial impact on their results. However, this robustness test can only be performed for countries where their data allow them to distinguish between paid leave and employment (see notes to Figure A.1 in Kleven et al., 2025). Many countries with longer paid leave entitlements, including Denmark, are excluded from this analysis.

Other benefits and transfers. We draw on the OF register to capture additional aspects of public transfers, including monthly information on take-up of education, unemployment, disability, and welfare benefits. We combine data on take-up with information on transfer income by benefit type from the ILME register. In any given month, total personal income is given by the sum of ILME transfer amounts and labor earnings from the BFL-register.

Skill. Our primary measure of skill is the highest qualification obtained by the end of our 10-year panel, which we obtain from the education register (UDDA). From the same register we extract exact graduation dates, used to build the time-since-graduation controls.

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 and 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 datasets commonly used in other contexts, hence we focus on education level in the main body of the paper.

Pooling these registers yields a balanced monthly panel that tracks each individual's fertility, parental leave, and labor market outcomes across the full 10-year window centered on first birth. Table A.1 shows summary statistics on our sample.

3 Parental Leave & Fertility Dynamics by Skill

This section documents patterns in birth timing, spacing, and parental leave take-up, before turning to the implications for estimating child penalties in the next section. Panel (a) in Figure 1 gives the distributions of age at first birth for mothers and fathers by completed level 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: the 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. For those with educational qualifications, the outcome variable is equal to 1 in the year they graduate and zero otherwise; for the high-school dropouts, the outcome variable is equal to 1 in the year they leave high school. With the exception of high-school dropouts, we note a sharp increase in the share of first births right after finishing education 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.

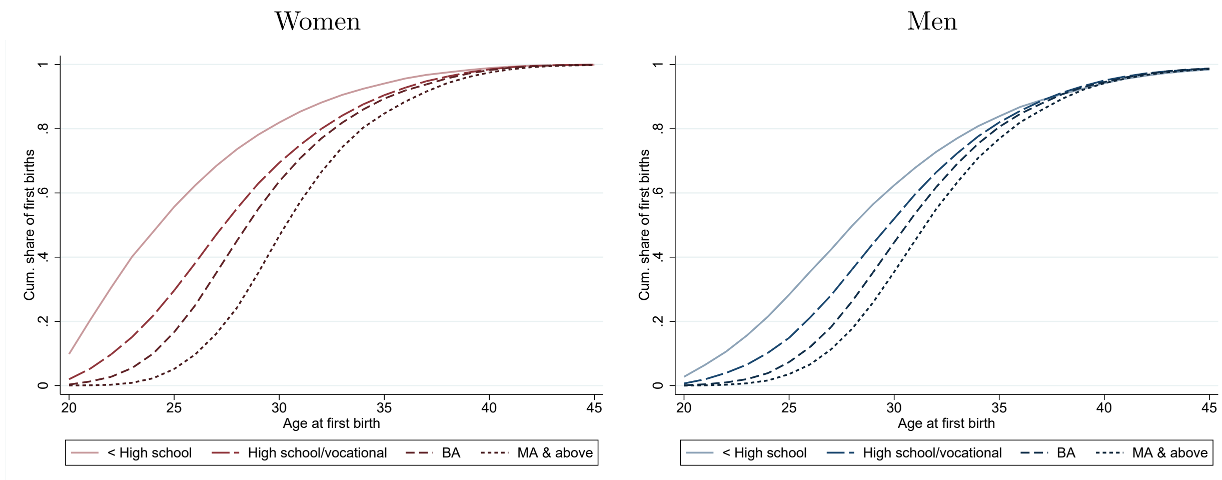
The timing of higher-order births is also heterogeneous across skill groups. Figure 2 shows evidence on cumulative fertility over the sample window. By construction, this jumps from zero to one at first birth (Panel (a)). Over the next five years, most mothers and fathers have additional children, with 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 cumulative fertility at the end of year five after first birth. The share of parents with only one child decreases monotonically with education, while the share with two children rises monotonically with it. More than 80% of the highest educated group have additional children in this window, compared to 60% of the least educated group.

Although these cohorts may be still too young to determine their completed fertility, summary statistics for 45 year-old individuals from 1970-1975 birth cohorts reported in Table D.1 establish that completed fertility (conditional on having at least one child) is essentially flat with levels of completed education. Thus, if higher-educated mothers are to realize similar completed fertility despite having their first child later, they face a shorter reproductive window and tend to space subsequent births more tightly than less-educated mothers. Indeed, Figures D.1 and D.2 show that the timing and spacing of births in these older cohorts are very similar to that observed in our main sample.

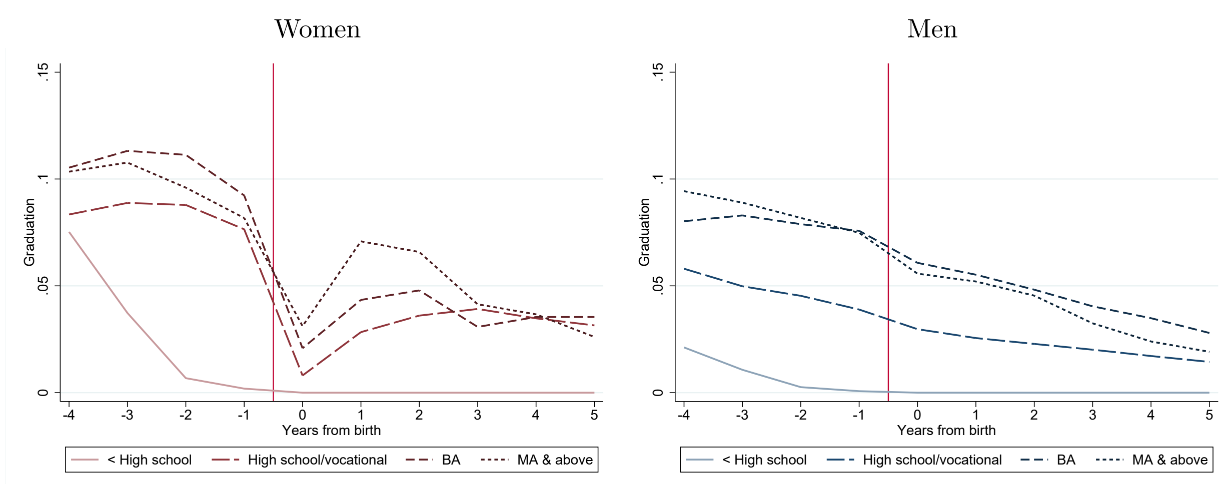
Figure 3 shows evidence on spells of parental leave, alongside other benefits, around first birth. The less educated, especially high school dropouts, are less likely to be on parental leave than the highly educated throughout the sample window (Panel (a)). For example, 26% of the highest educated women 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

Figure 1: Age and graduation rates around first birth

(a) Cumulative share of parenthood



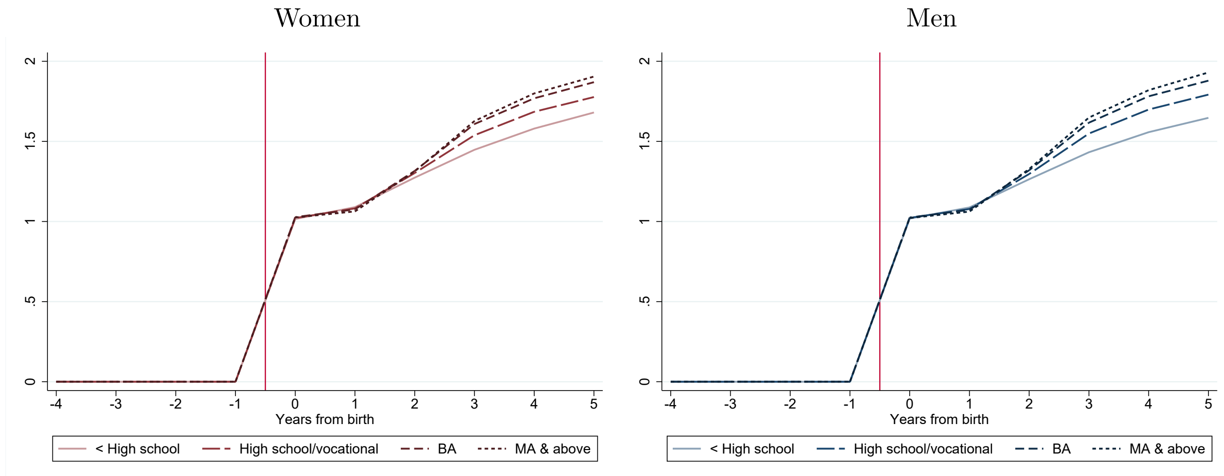
(b) Graduation rates



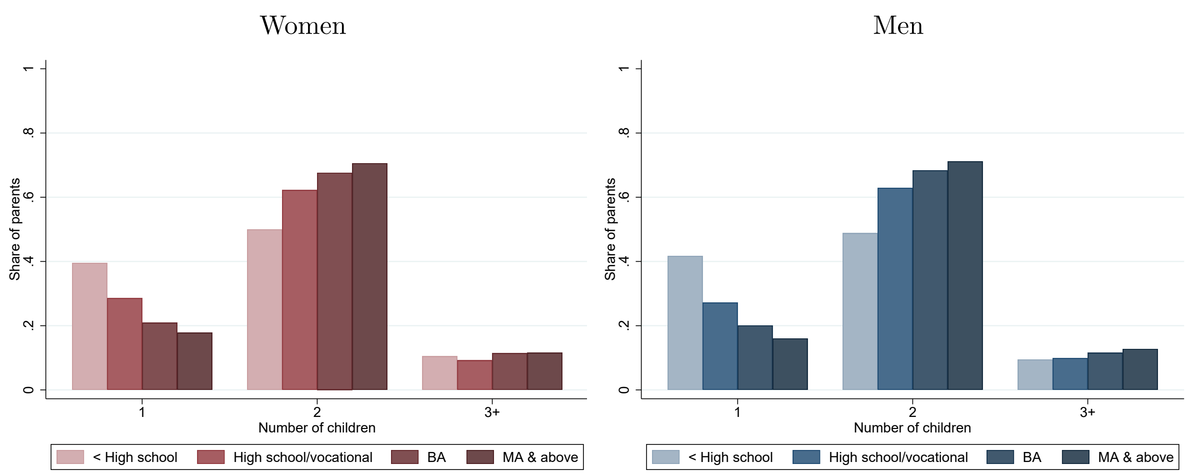
Notes: Panel (a) shows the cumulative share of first births by mothers' and fathers' age (the corresponding density functions are shown in Panel (a) of Appendix Figure A.1). Panel (b) shows the probability of graduating each year around first birth (Appendix Figure A.1, Panel (b), shows years from graduation 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



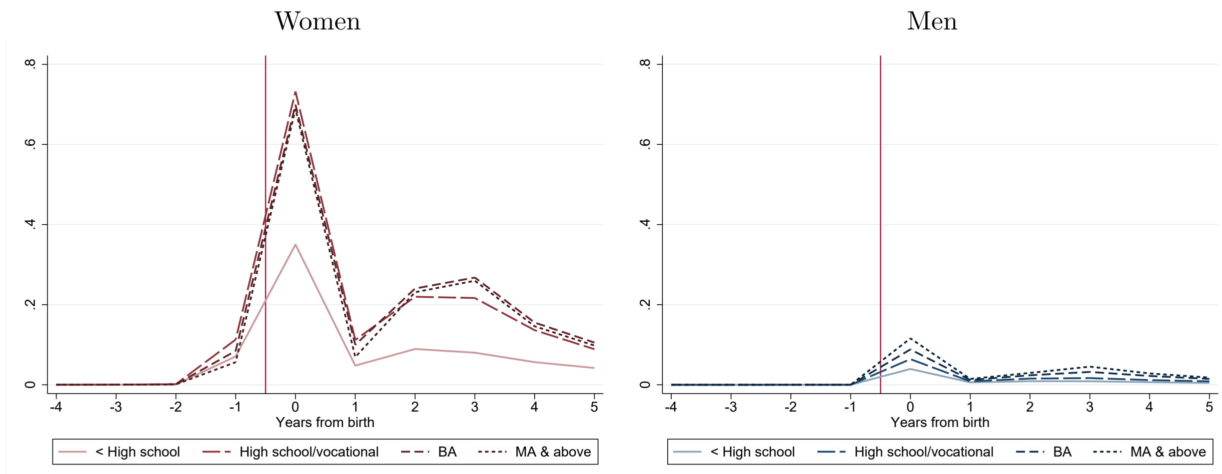
(b) Number of children 5 years from first birth



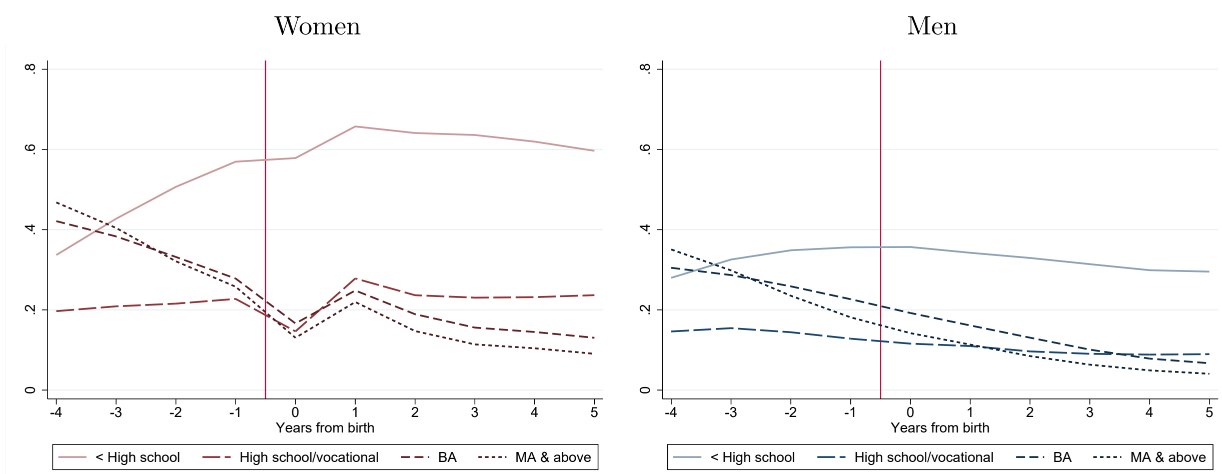
Notes: Panel (a) shows the cumulative number of children each year around first birth. Panel (b) shows the distribution of the number of children by the end of year 5 after first birth. All analyses are undertaken separately by highest education level observed at the end of the panel. See Table A.1 for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

Figure 3: Parental leave and benefit spells around first birth

(a) Share on parental leave



(b) 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.

on fertility. By contrast, less educated mothers are more likely to be on other – non-job protected – benefits, most notably welfare and education 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.[9]

Finally, the implications of these parental leave dynamics are already visible in raw earnings. Figure 4 plots earnings around first birth relative to the level observed two years prior, without any regression adjustment. We use year -2 as the reference period because the year immediately preceding birth may already be affected by leave-taking and benefit receipt prior to childbirth.

Panel (a) treats earnings as zero during parental leave, whereas Panel (b) holds them at their pre-leave level. Two patterns stand out. First, in Panel (a), we see that mothers' earnings fall sharply at first birth in every education group, with no comparable drop for fathers, and the gap remains wide five years on. Second, the size of mothers' drop in earnings, and to what degree it persists, depends strongly on how parental leave is treated: holding earnings at their pre-leave level markedly compresses both the initial fall and the medium-run gap, especially for the most educated, who spend the most time on leave for higher-order births. These raw patterns in earnings preview the central role of parental-leave measurement that we formalize in Section 4.

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. (2019) popularized an event-study specification for this purpose, which has been widely used (see Cortés and Pan, 2023 for a review).

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

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

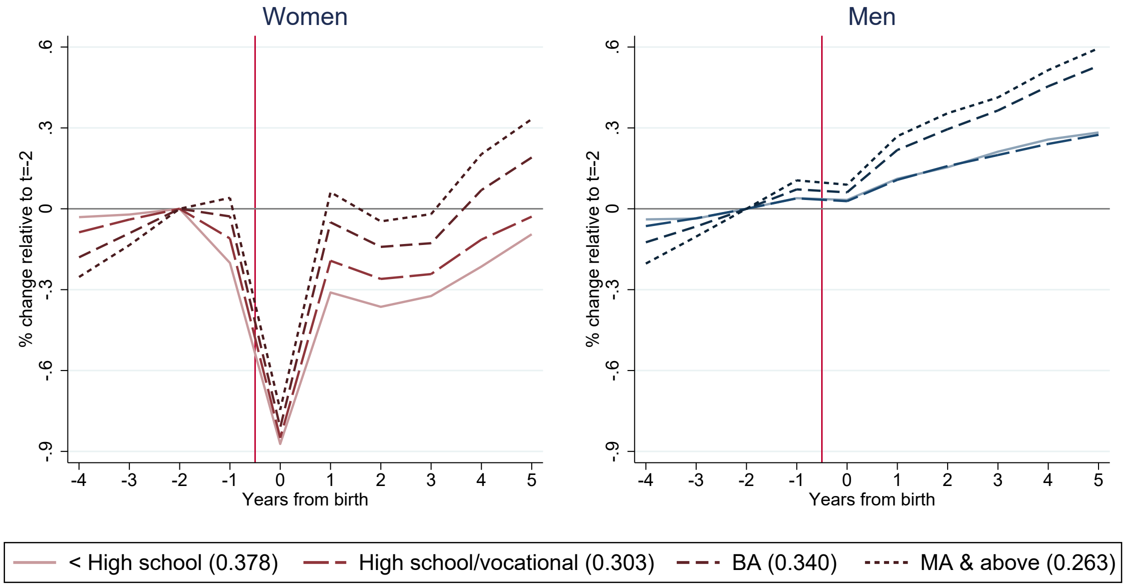
where $g = \{m, f\}$ denotes gender, Y_{ity} represents the outcomes of interest, e.g., earnings, in calendar year y for individual i with first birth t years from year y. Age^g_{yi} is a set of age fixed effects that capture lifecycle dynamics in labor market outcomes, and $Year^g_{yi}$ is a set of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

Event-time effects δ^g_j are the coefficients of interest, which identify the impacts of first

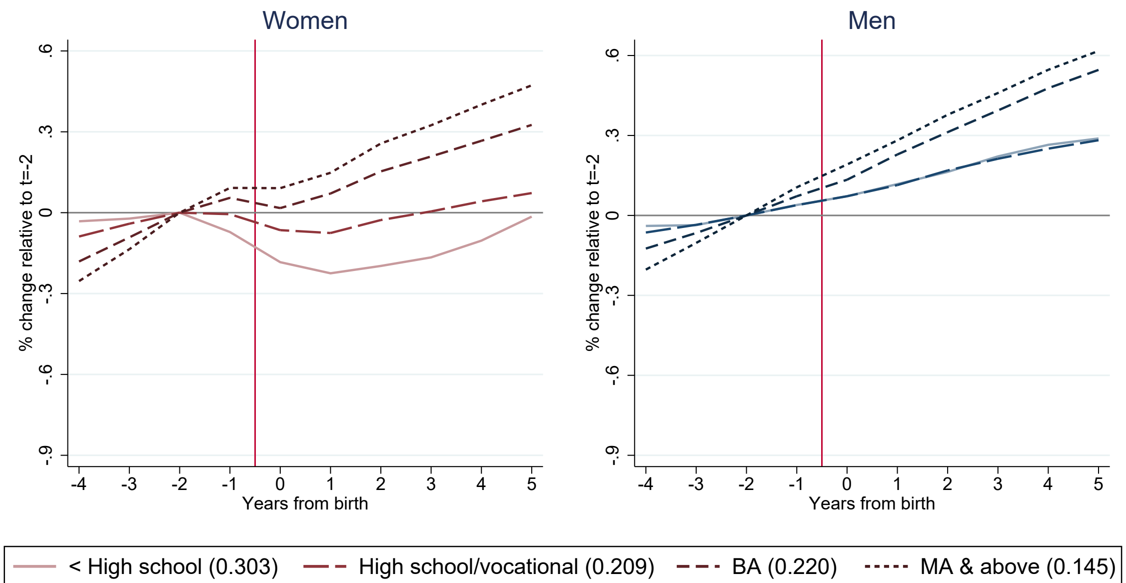
[9] In Appendix D, we show that similar patterns are clearly evident for the cohort of mothers born between 1970 and 1975.

Figure 4: Earnings around first birth

(a) Earnings set to 0 during PL



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



Notes: Panel (a) shows earnings relative to two years before first birth when any earnings during spells of parental leave are set equal to zero. Panel (b) shows 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.

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 two conditions are satisfied. First, conditional on the included controls, counterfactual outcomes need to be invariant with respect to age at first birth. Second, outcomes should evolve smoothly in the absence of birth.

The findings of Section 3 are informative vis-à-vis the plausibility of these assumptions. First, Figure 1(a) showed that age at first birth varies systematically with education level. As age-earnings profiles are heterogeneous by levels of completed education (see for example Adda et al. 2017), age at first birth is likely to be systematically related to earnings potential. This can be captured by estimating separate event-study regressions by groups defined by completed education.

One may interpret group-specific estimates as a restricted version of an enriched event-study model with heterogeneous effects by age at first birth, or parenthood "cohort". The problem with the unrestricted model is that event-time (years since childbirth) and cohort add up to parental age. The most common approach circumvents the issue of multicollinearity by omitting the cohort dimension, a restriction that is tested and rejected by Thakral and Tô (2026) on US data. Allowing for heterogeneous child penalties by skill provides an estimable approximation to an unrestricted model with heterogeneous effects by parenthood cohort, as we have shown that age at first birth is systematically correlated with levels of completed education.

Second, the clustering of first births soon after graduation for women is problematic for the assumption of smooth labor market outcomes absent childbirth. In order to make the smoothness assumption more plausible, we flexibly control for time since graduation.

Finally, Section 3 also highlighted 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), imply 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 A.2 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 (around six months on average after birth), 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 are consistently 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, and use monthly data to account for changes in the source of parental transfers:

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

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.[10]

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. As a lower bound on outcomes during parental leave, we set labor market earnings, hours, and participation equal to zero throughout spells of leave. As an upper-bound measure, 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.[11] 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.

5 Results

Figure 5 shows event-study estimates for the impact of first birth on our lower- and upper-bound measures of labor market 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

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

[11] Throughout, we use "lower bound" and "upper bound" to refer to the value assigned to measured labor market outcomes while an individual is on leave; the two measures correspond to different estimands, not to lower and upper bounds on a single structural parameter.

contribution of event dummies. Specifically, we plot

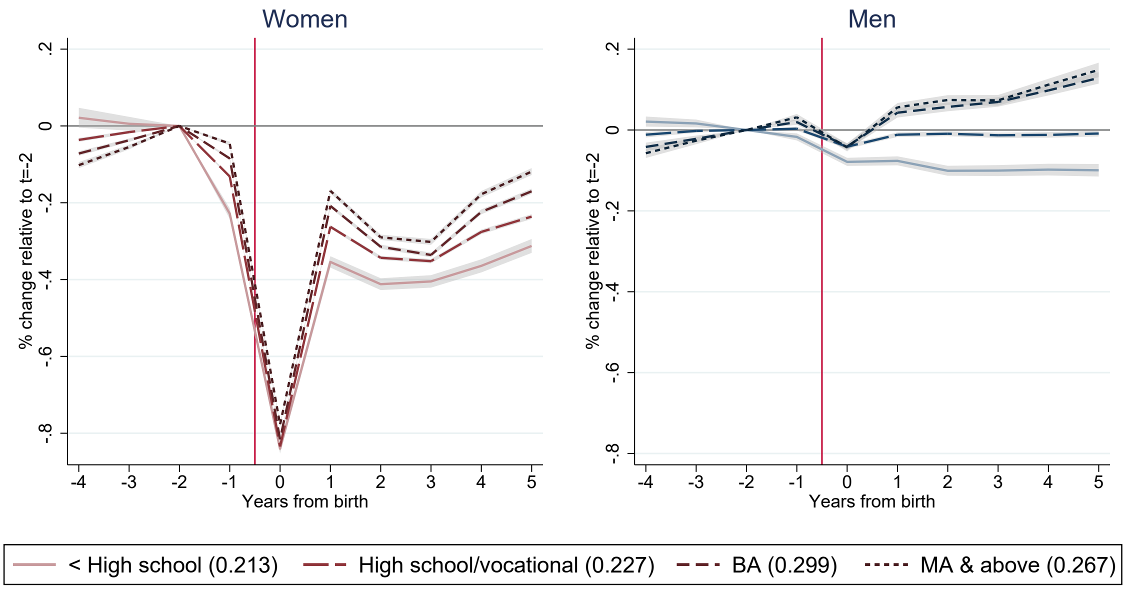
$$P_t^g = \frac{\hat{\delta}_t^{gs}}{E(\tilde{Y}_{imt}^{gs})}$$

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, $P_5^m - P_5^f$, 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.

The panels of Figure 5 differ based on whether the lower- or upper-bound measures of labor market earnings are utilized and whether controls for years since graduation are incorporated into the empirical specification. Panel (a) in Figure 5 estimates specification (2) by education group, but without the controls for years since graduation and sets earnings at zero during spells of parental leave, regardless of income replacement paid via payroll (the lower bound). Panel (b) adds controls for years since graduation (specification (2)) and again uses the lower-bound measure of earnings. Panel (c) uses the upper-bound measure of earnings during parental leave and includes fixed effects for years since graduation.

Figure 5: 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

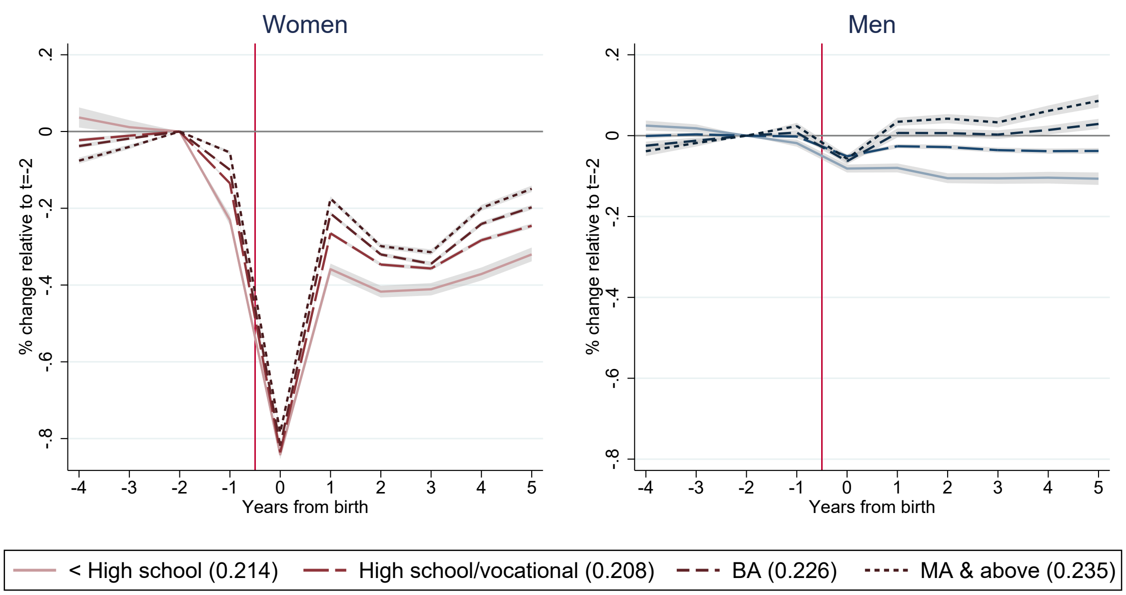
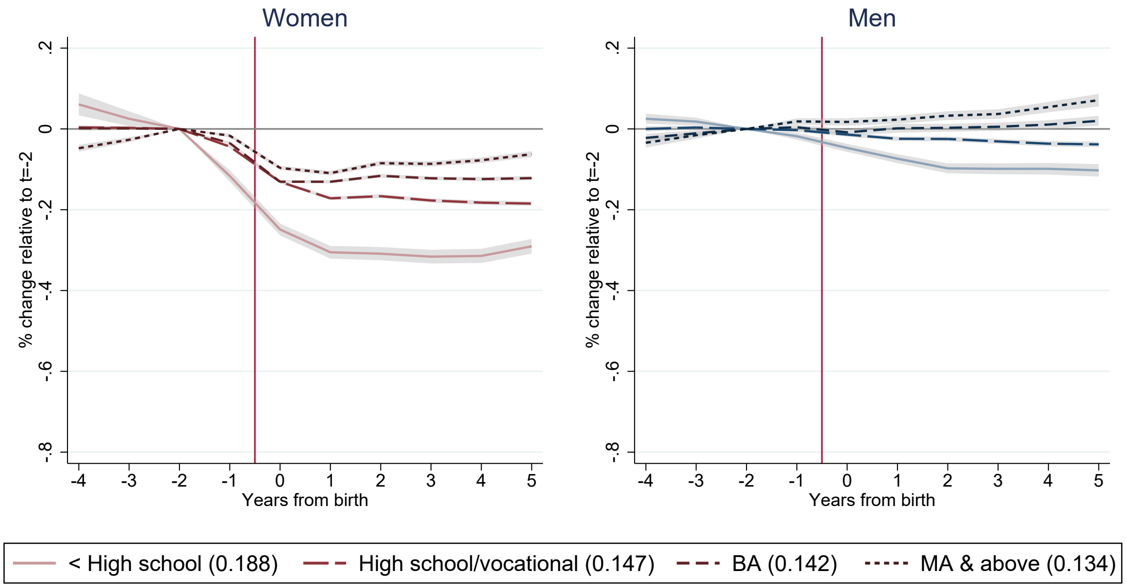


Figure 5: (continued)

(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.

Panel (a) shows that maternal earnings decrease by approximately 80% for all skill groups in the year of the first birth compared to two years prior. 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. The intuition is that canonical child-penalty estimates may conflate higher returns to potential experience for the highly educated with genuine penalties or premia. In particular, for men, both earnings and the arrival of first birth rise steeply after graduation from university degrees. Thus, childbirth appears to be associated with relatively large earnings premia, which are partly absorbed by controls for years since graduation. As one would expect, this change in specification does little to the pattern observed for the less educated groups.

Panel (c) uses the upper-bound measure of earnings. For this measure, changes in earnings post-birth only reflect changes in labor supply behavior as opposed to incapacitation during parental leave. 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 highly educated 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 6 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 5.

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 5).

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 sharp education gradient in the quantitative importance of this adjustment reflects three mechanisms. First, the more educated have higher fertility than the less educated during the first five years after first birth. Second, conditional on having a child, more educated mothers are more likely to be employed and therefore eligible for job-protected parental leave. Third, changing the measurement of outcomes during leave also alters the estimated age and years-since-graduation fixed effects, which contributes to the observed divergence. In particular, earnings trajectories are substantially smoother when outcomes on parental leave are imputed (see Panels (a) and (b) in Figure 4).

The final set of estimates in Figure 6 aggregates child penalties across education groups by estimating a version of specification 2 for the full population of parents, including education dummies and their interaction with age and years since graduation fixed effects.[12] The first bar replicates quite closely the specification and results of Kleven et al. (2019), yielding a five-year child penalty of 23.3%. The inclusion of years-since-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. The remaining earnings penalty is primarily an extensive-margin phenomenon. Five years after first birth, and conditional on working, mothers' hours fall by 3.8% and their hourly wages by 4.6% (see Panels (c) and (d) in Figure A.5), against a decline of 8.4% in participation (Panel (b)).

Panel (b) in Figure 6 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.

The margin that drives the earnings penalty and its variation with maternal education – time out of work due to parental leave or otherwise – is also the one that government transfers do the most to offset, whether with parental leave benefits or other, means-tested benefits. This motivates our final analysis on an outcome measure of total income, including earnings and transfer income from any source.

Our event-study estimates of child penalties in total income are shown in Figure 7, with and without controls for years since graduation. The addition of these controls in Panel (b) mostly reduces the fertility premia observed for fathers in Panel (a), as we already observed in Figure 5. Focusing on the more complete specification in Panel (b), we note that short-run penalties in total maternal income are much reduced for all groups relative to

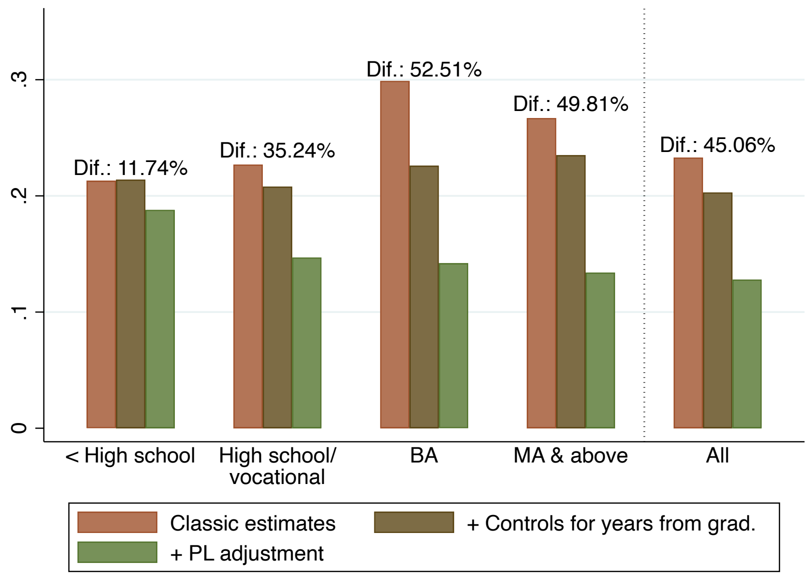
[12] Specifically, we estimate:

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

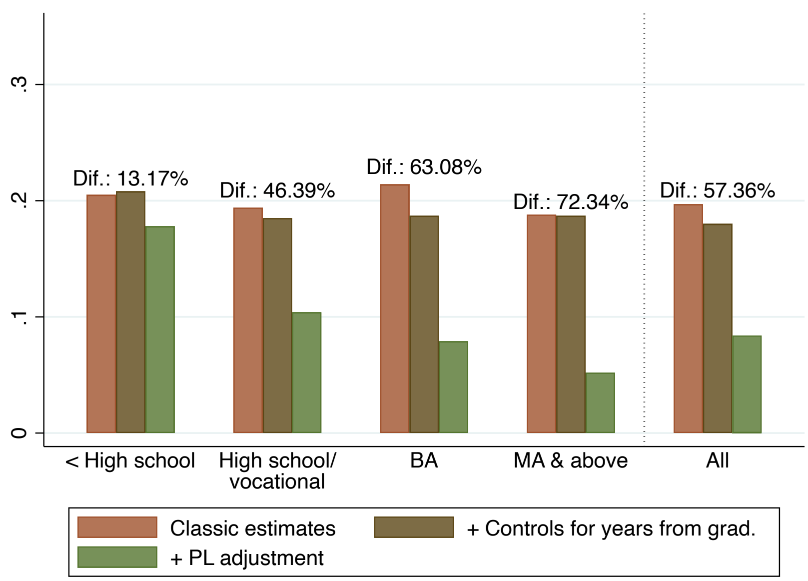
where Edu^g_{im} denotes education fixed effects.

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

(a) Earnings



(b) Participation



Notes: The figure includes estimates of child penalties by year 5 after first birth from various specifications; the first four sets of bars provide estimates for each education level (specification 2) and the last set of bars for the aggregate sample (specification 3). Brown bars show estimates of child penalties 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. The number above each set of bars, indicated with "Dif:" gives the difference between the brown bars and green bars in percent. For results by educational level, see Figure 5, 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. See Table A.1 for details on the sample.

the corresponding results on labor earnings (see Panel (b) in Figure 5, noting the different scale on the vertical axis). While labor earnings fall by about 80% for all skill groups in the year of first birth, total income only falls by less than 15% for those who have secondary school qualifications or above, and actually increases by 8% for those without qualifications thanks to welfare transfers. In the medium run, transfers virtually eliminate the five-year penalty for the least educated (−0.9%), while penalties of 8.9%, 11.5%, and 12.8% remain for the high school/vocational, BA, and MA groups, respectively. This gradient reflects that parental leave benefits and welfare transfers provides near-full income replacement for the least educated – whose pre-leave earnings, if any, are close to the cap – but substantially lower replacement rates for highly educated mothers with higher pre-leave earnings. As a result, total income penalties remain sizeable for the most educated groups even when transfers are included. As expected, these penalties are much closer to our upper-bound estimates (Panel (c) in Figure 5) than to the lower bound (Panel (b)). The aggregate five-year penalty falls from 11.4% without education and graduation controls to 9.0% in our preferred specification (see Figure A.10).

5.1 Extensions

In Appendix B, we show that our 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 and completion grades is therefore plausibly exogenous to fertility preferences.

We assign individuals with a high school degree to terciles of the distribution of final grades, and place individuals without a high school degree in 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 individuals are treated as employed during spells of parental leave.[13] For participation, the corresponding reductions range from 40.1% for the least educated to 65.0% for the most educated.

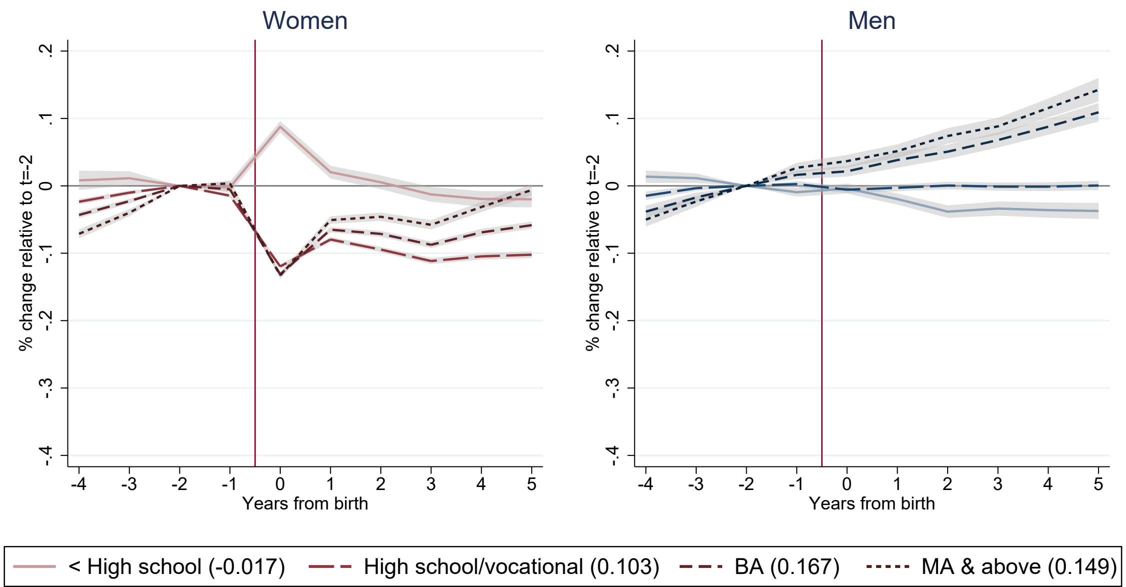
Finally, we directly explore the role of multiple births. The amount of time spent on parental leave in the years following first birth primarily reflects the number and timing of subsequent births. As such, the measurement of labor market outcomes during parental leave

[13] Table B.1 reports summary statistics for the high-school-grade groups. Figure B.1 shows birth-timing and graduation patterns by high-school-grade group; Figure B.2 shows cumulative fertility; Figure B.3 shows parental leave and other benefit spells; Figure B.4 shows the age-at-first-birth distribution; Figure B.5 shows raw earnings dynamics; and Figure B.6 summarizes the event-study estimates.

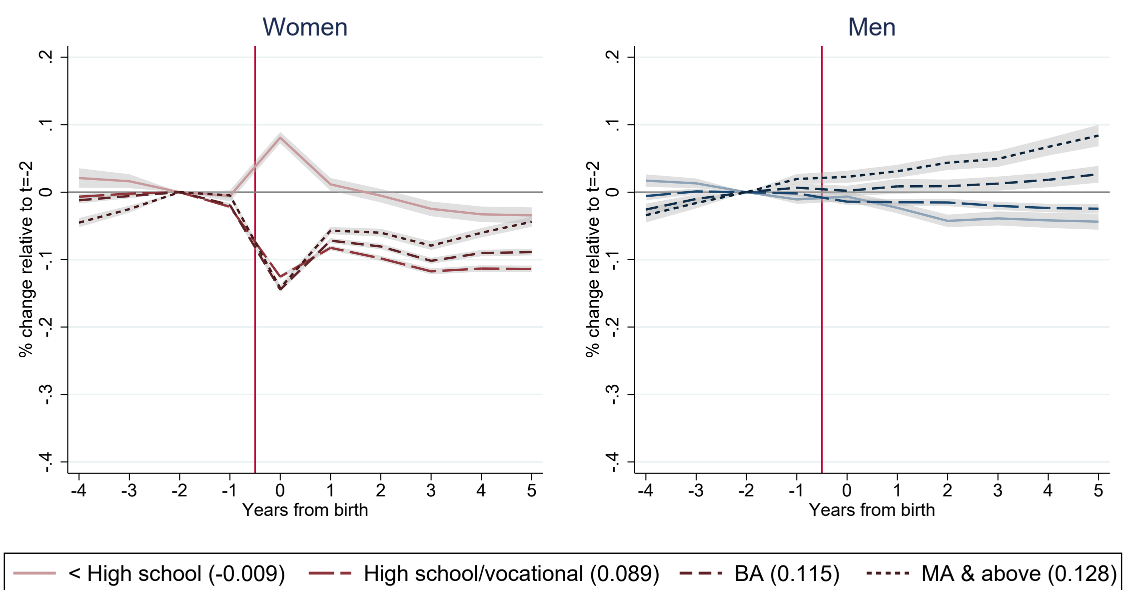
Figure 7: Child penalties by education levels

Earnings and transfer income

(a) Without controls for years from graduation



(b) With controls for years from graduation



Notes: The figure shows estimated child penalties in total income (labor earnings plus personal transfers) by education levels. Panel (a) without controls for years from graduation, Panel (b) adds controls for years from graduation. 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. Figure A.9 reports the corresponding raw changes in total income around first birth. See Table A.1 for details on the sample. 95% confidence intervals indicated.

becomes especially important when comparing parents with different numbers of children. Previous evidence suggests markedly larger child penalties for mothers with more children (see e.g. Figure A.III in Kleven et al., 2019).

To assess how this finding relates to parental-leave measurement, we estimate child penalties separately for parents with different numbers of children five years after their first birth in Appendix C. For simplicity, we focus on differences by cumulative fertility and abstract from heterogeneity by skill level in this analysis. When controlling for years from graduation and setting earnings equal to zero during parental leave in Figure C.2, we find child penalties in earnings five years after first birth of 11.2%, 18.3%, and 48.0% for parents with 1, 2, and 3 children, respectively.[14] If we instead set earnings during leave at the level recorded the month before starting the leave spell (Figure C.3), we find penalties of 9.1%, 12.9%, and 20%, respectively. This suggests that much of the difference in estimated child penalties by the number of children is driven by parental leave taking, and especially so at higher parities. This again highlights the relevance of institutional settings for the estimation of child penalties.

6 Conclusion

We use Danish administrative data to show that the timing and spacing of births are systematically related to maternal skill level. More educated mothers have their first child later in life, but their subsequent births are spaced closer together; as a result, they spend a larger share of the years after first birth on parental leave. While this pattern is interesting in its own right, it also matters for child-penalty estimation for two reasons. First, event-study designs identify child penalties from variation in age at first birth, which is systematically correlated with completed levels of education. Second, the measurement of labor market outcomes during parental leave is consequential, especially for estimating child penalties among highly educated parents.

Our results call for two adjustments to conventional event-study specifications. First, event-study specifications should allow age profiles and years-since-graduation effects to differ by skill, since childbirth is concentrated around graduation for the highly educated, when earnings profiles are steep. Second, researchers should state explicitly how job-protected parental leave is coded; individuals on such leave are entitled to return to their pre-birth jobs and count as employed under the standard ILO definition. Setting earnings, hours, and participation to zero during leave measures the contemporaneous loss of productive labor

[14] Table C.1 reports summary statistics by cumulative fertility at the end of the panel. Figure C.1 shows the corresponding child-penalty estimates without controls for years since graduation.

input – the efficiency cost of childbirth. Holding these outcomes at their pre-leave level instead measures labor-market attachment, isolating persistent labor-supply adjustments outside leave, which are borne disproportionately by mothers and capture an equity-relevant component of the child penalty. In our application, switching from the first measure to the second reduces the estimated five-year earnings penalty by 37%; combined with the skill-specific controls, our preferred specification lies 45% below the standard approach, with a different skill gradient across education groups.

These distinctions matter for how child penalties are interpreted. A sizeable part of the medium-run penalty in Denmark reflects leave-taking around higher-order births rather than persistent detachment from work. This does not imply that parental leave is costless: even under our upper-bound, attachment-based measure, mothers' participation and earnings remain significantly below those of fathers five years after first birth. It does imply that comparisons across countries, over time, or across skill groups can confound underlying career impacts with institutional differences in the generosity, financing, and measurement of parental leave.

The same logic applies to the evaluation of family policy. Reforms that extend job-protected leave may mechanically increase measured child penalties if leave is coded as non-employment, even when they do not reduce long-run attachment to work; conversely, systems that replace earnings through employers rather than government transfers may mechanically reduce measured penalties in earnings registers. Accounting for total personal income changes the distributional reading further: transfers largely insure lower-earning mothers, whose pre-leave earnings fall close to the benefit cap, while penalties remain substantial for higher-earning mothers. Evidence on child penalties should therefore distinguish between income replacement, labor-market attachment, and time spent out of productive work – a distinction that clarifies where gender gaps arise and which policy margins can plausibly be targeted.

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ONLINE APPENDIX:
Measuring labor market dynamics around childbirth: The role of parental leave

A Additional results

A.1 Stylized Facts

Table A.1: Summary statistics by education group

Women

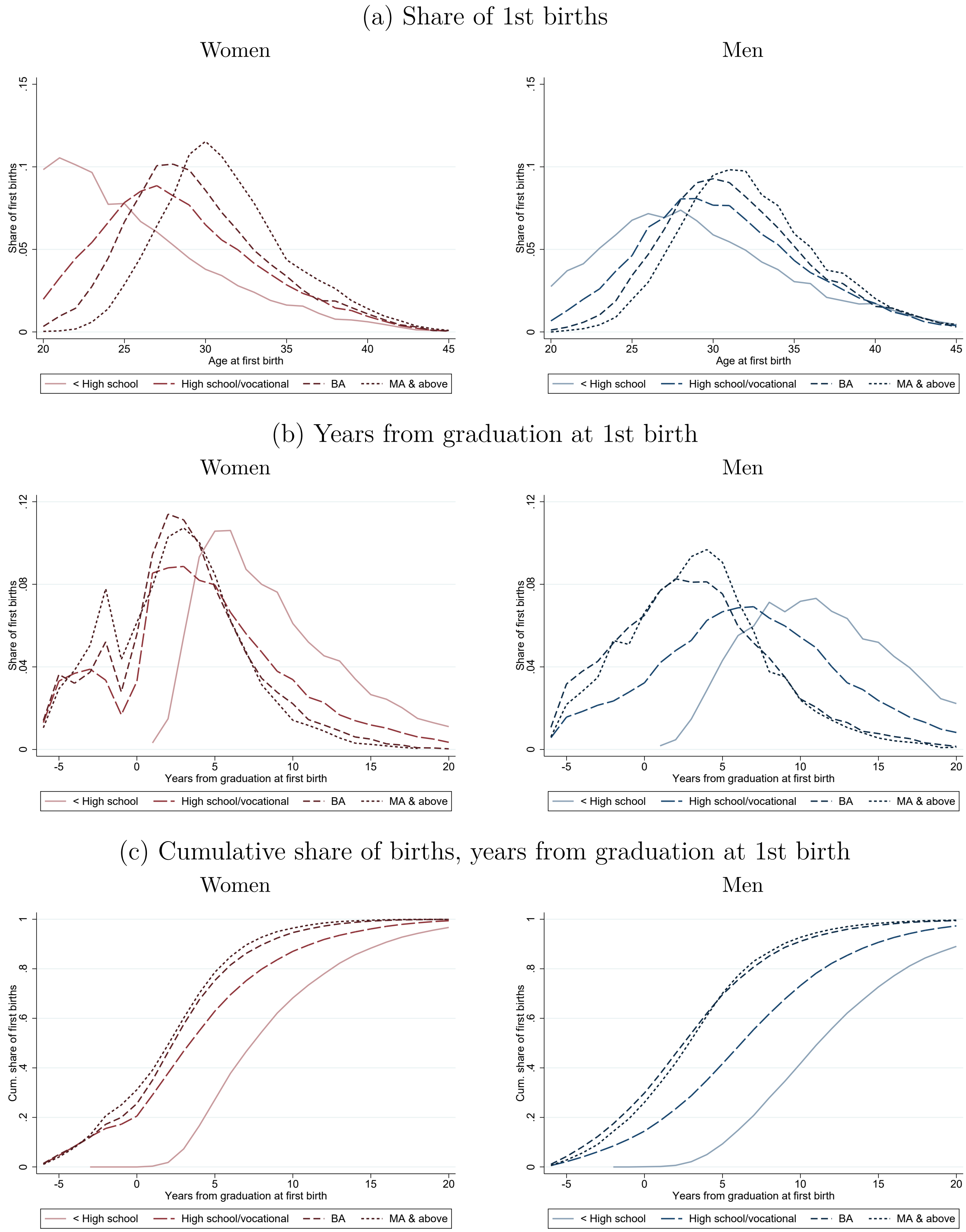
	Below high school	High school/vocational	BA	MA & above	All
Age	24.37	26.94	28.09	29.79	27.64
Married	0.12	0.19	0.21	0.26	0.21
Years from graduation	7.46	2.78	1.44	0.97	2.45
Employment	0.46	0.84	0.86	0.85	0.81
Share of month on benefit, ex. PL	0.54	0.22	0.29	0.28	0.28
Hours per month, inc. zeros	56.66	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

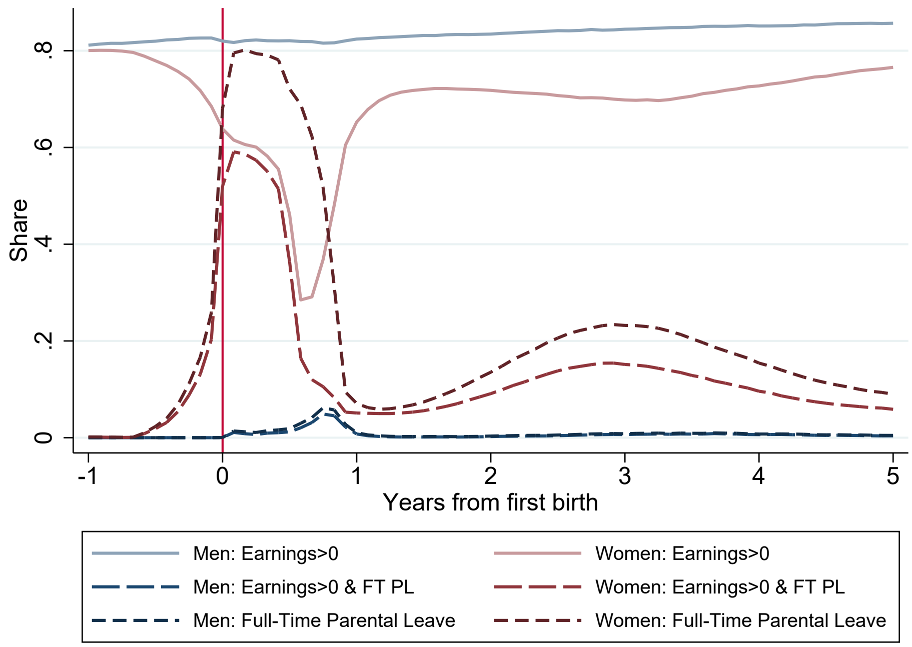
Notes: All characteristics are measured 12 months before first birth, except cumulative fertility and completed levels of education, which are measured at the end of the panel. For the high-school dropouts, years "from graduation" are measured from the year they left school. Employment is defined as working positive hours. Share of month on benefits include all benefits except parental leave (which is not relevant 12 months before first birth) 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. Year 0 starts with the month of birth of the first child.

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



Notes: Panel (a) shows the share of first births by mothers' and fathers' age at first birth. Panel (b) shows years from graduation at first birth. Panel (c) shows the 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.

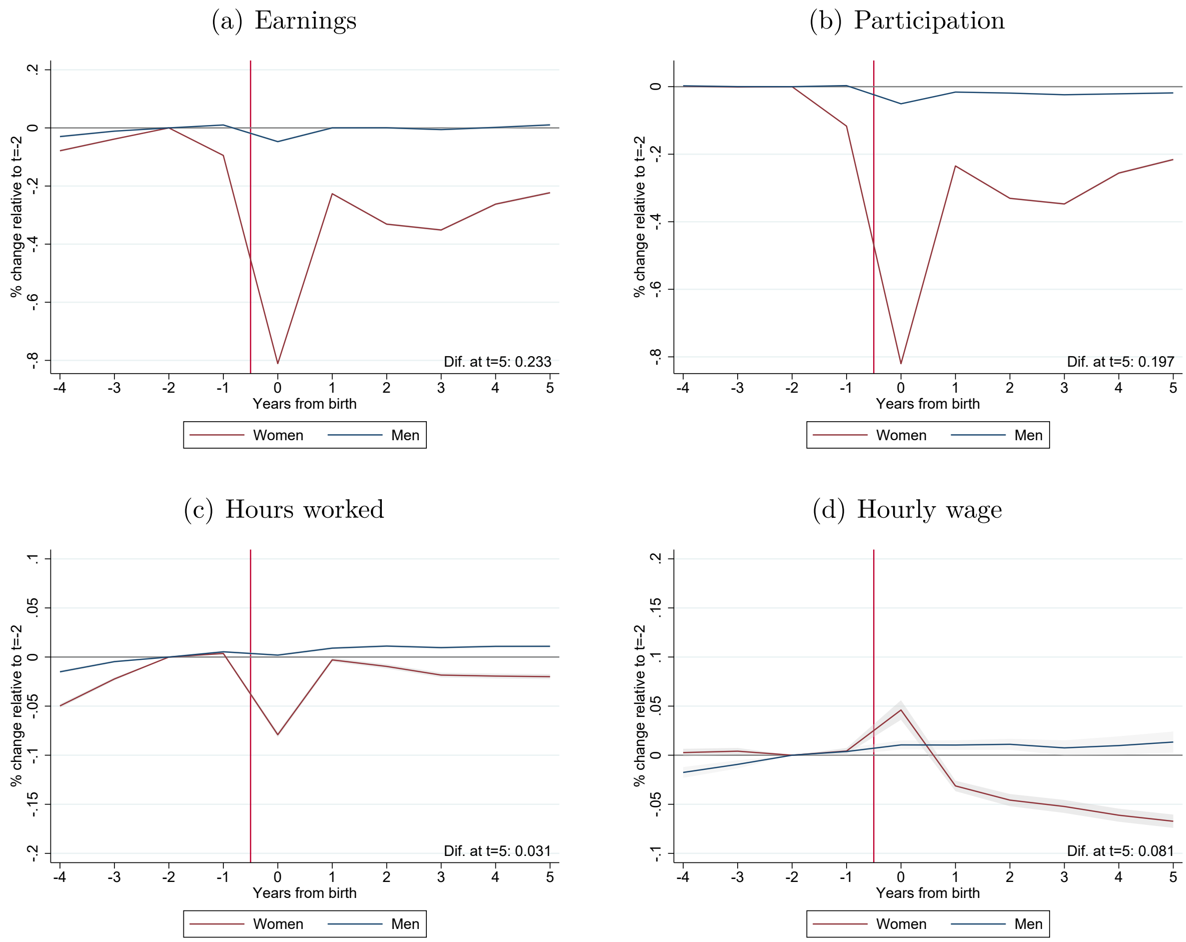
Figure A.2: Earnings & parental leave benefits around first birth:
Overlap with labor earnings



Notes: The 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.

A.2 Child penalties

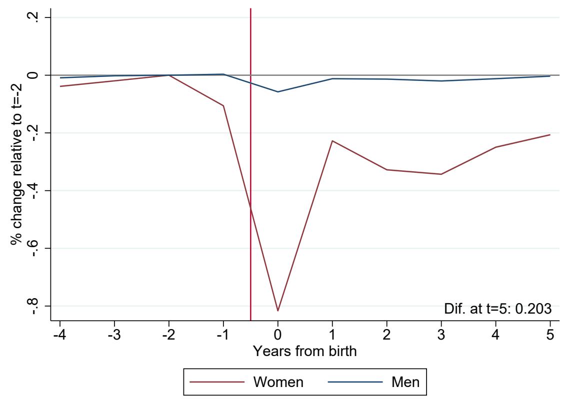
Figure A.3: Child penalties, all education levels:
No controls for education or years from graduation



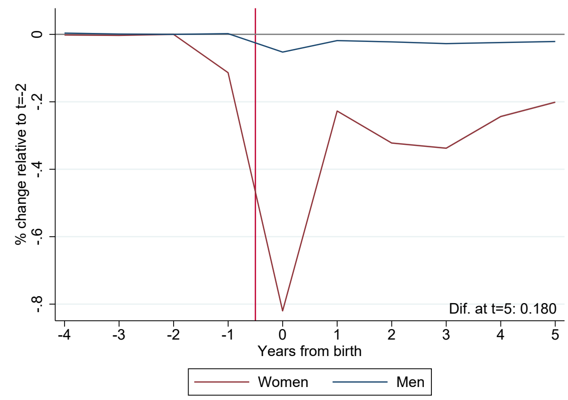
Notes: The figure shows estimated child penalties across education levels without controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly 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. 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, interacted with age and years from graduation

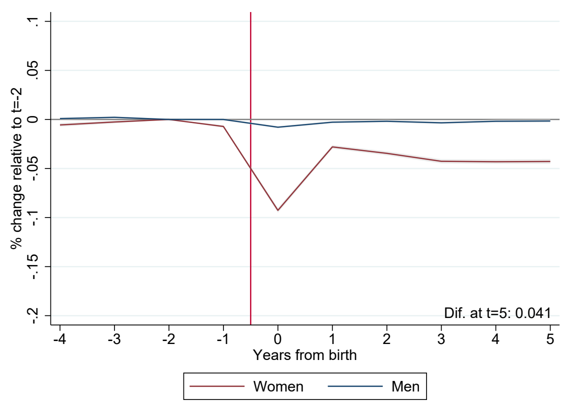
(a) Earnings



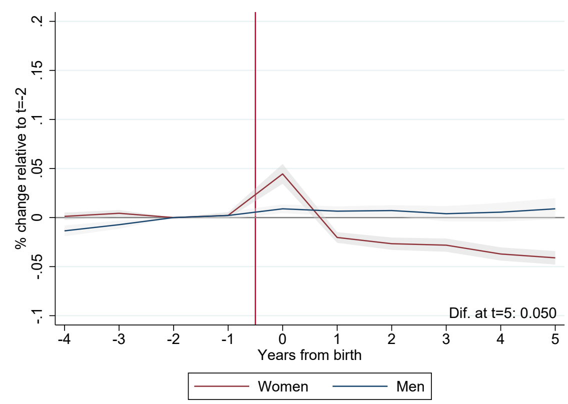
(b) Participation



(c) Hours worked

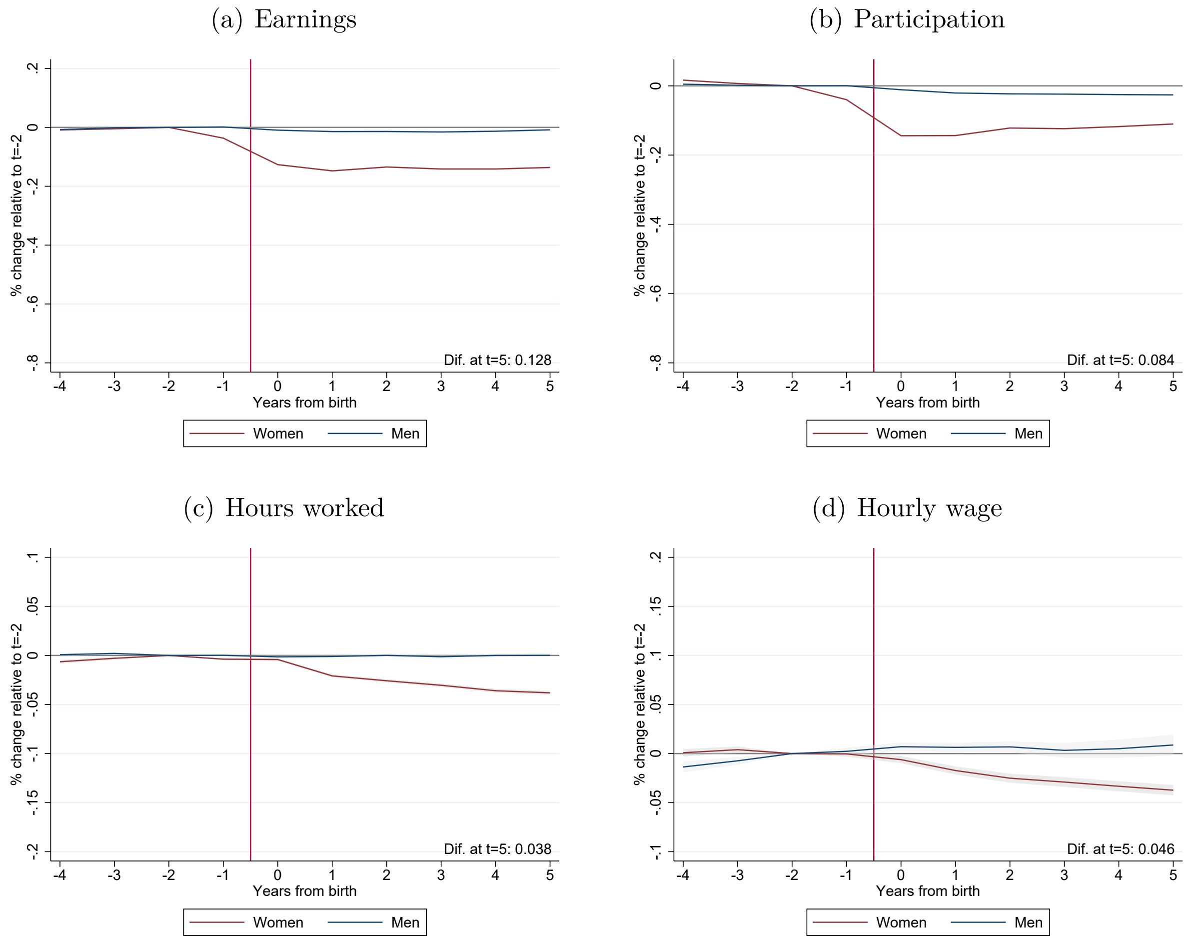


(d) Hourly wage



Notes: The 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. 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

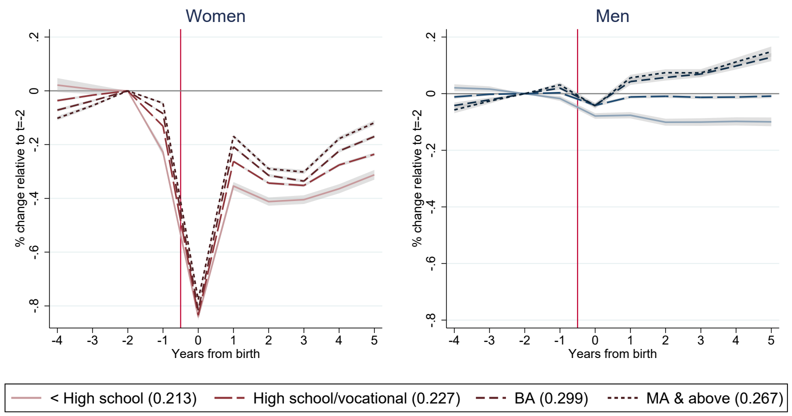


Notes: The figure shows estimated child penalties across education levels with controls for age and years from graduation interacted by education level, and adjusting outcomes during spells of parental leave to be equal to levels in the month immediately prior to the spell of parental leave. Panel (a) 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. See Table A.1 for details on the sample. 95% confidence intervals indicated.

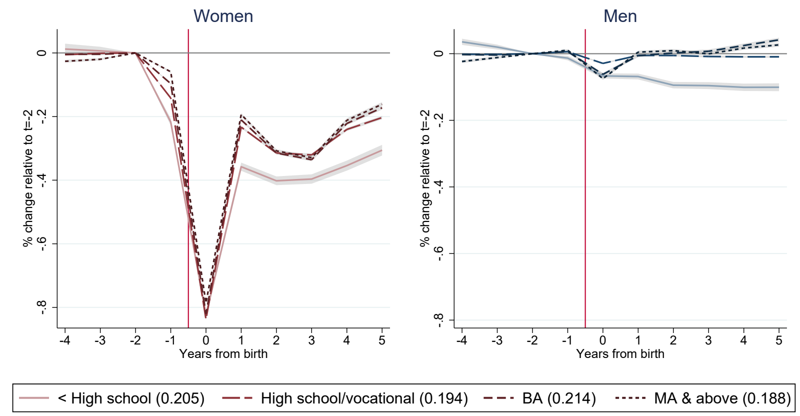
Figure A.6: Child penalties by education

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

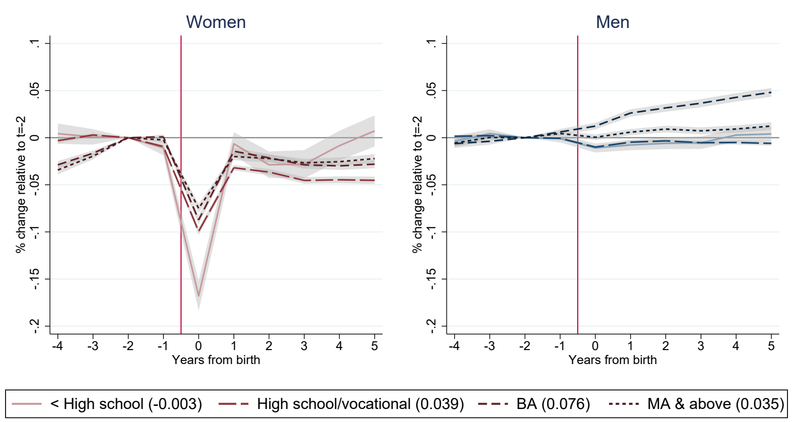
(a) Earnings



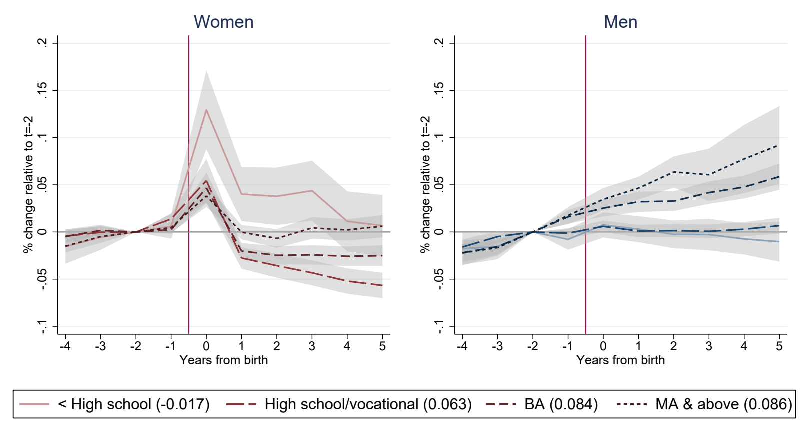
(b) Participation



(c) Hours worked



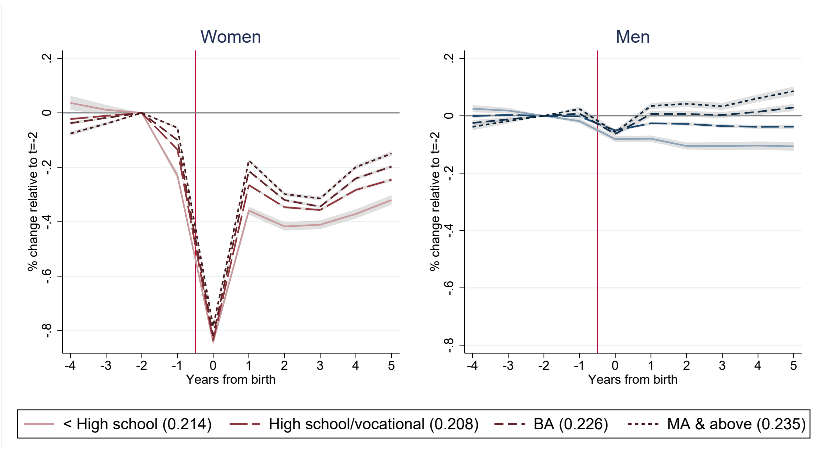
(d) Hourly wages



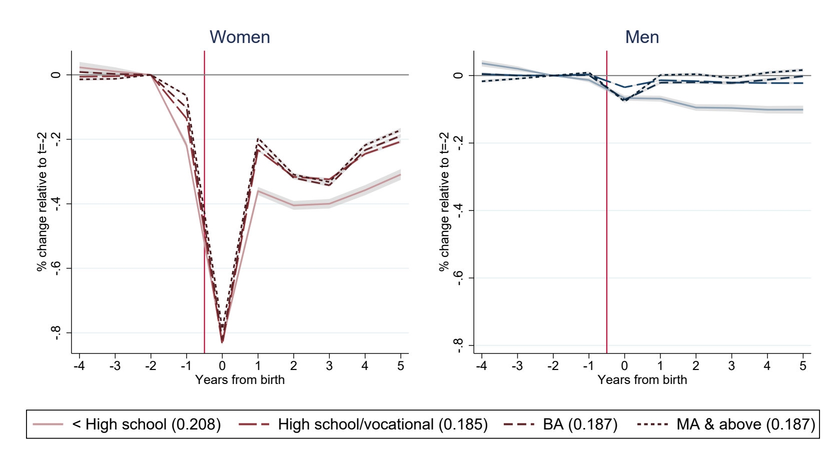
Notes: The figure illustrates estimates of child penalties by education level without controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly 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

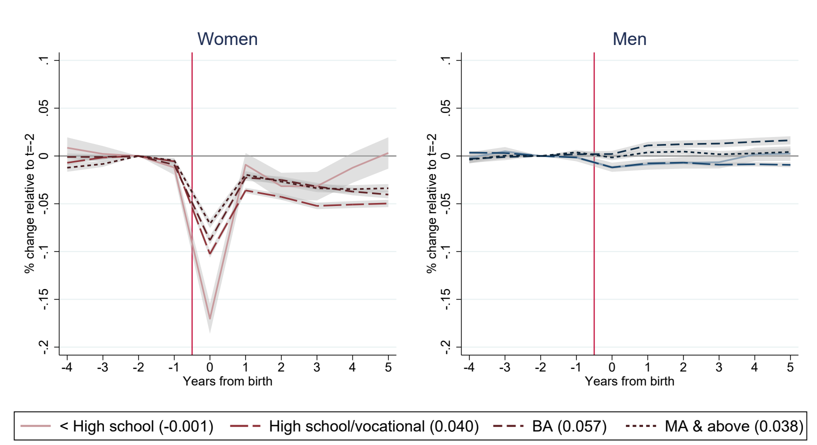
(a) Earnings



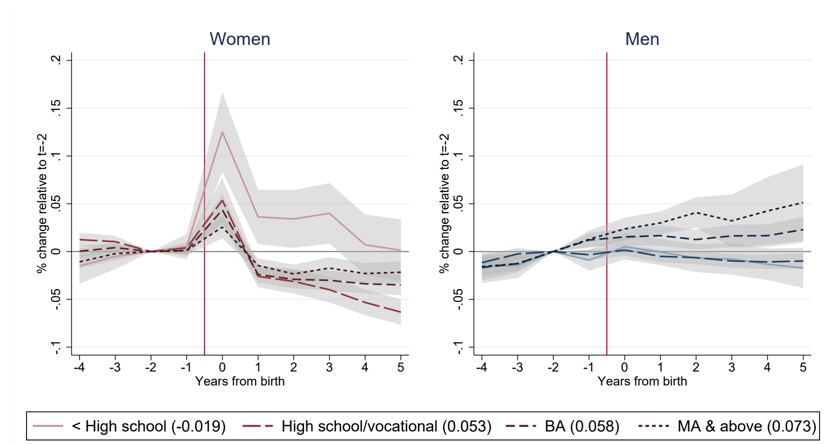
(b) Participation



(c) Hours worked



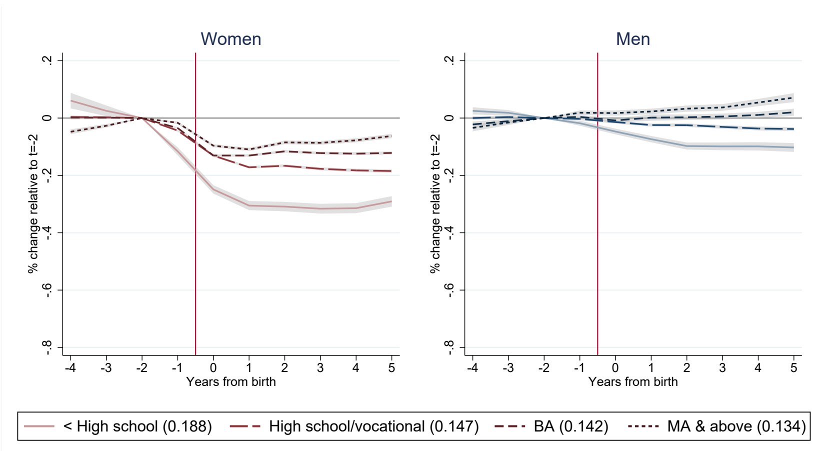
(d) Hourly wages



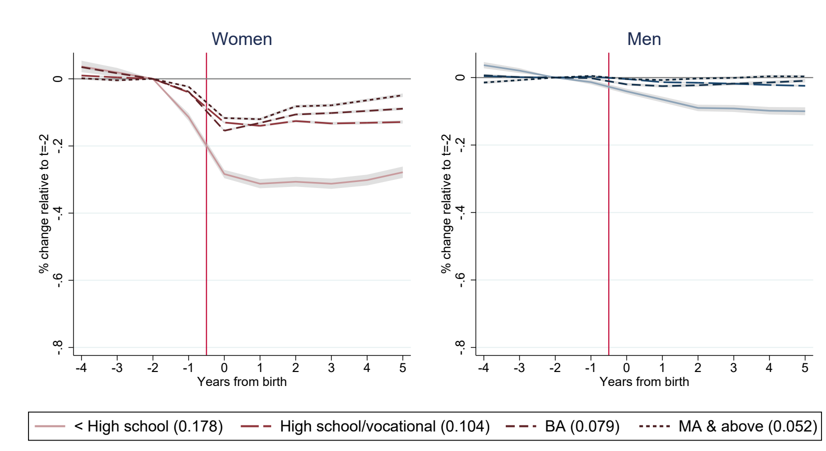
Notes: The figure illustrates estimates of child penalties by education level with controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, and Panel (d) for hourly 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

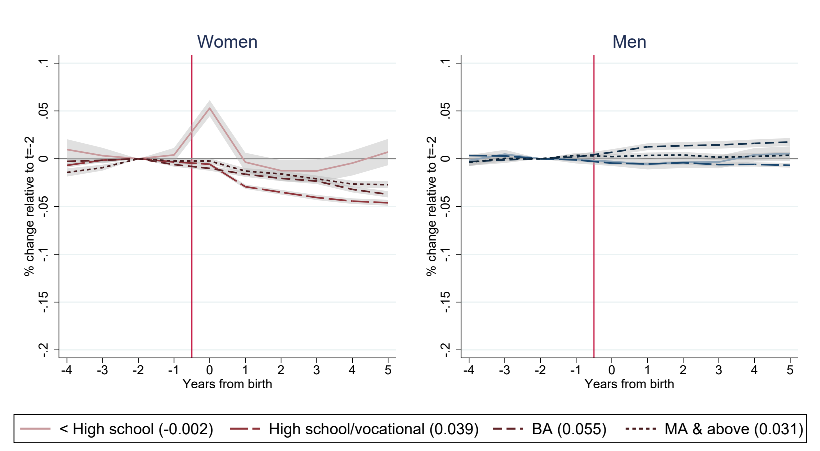
(a) Earnings



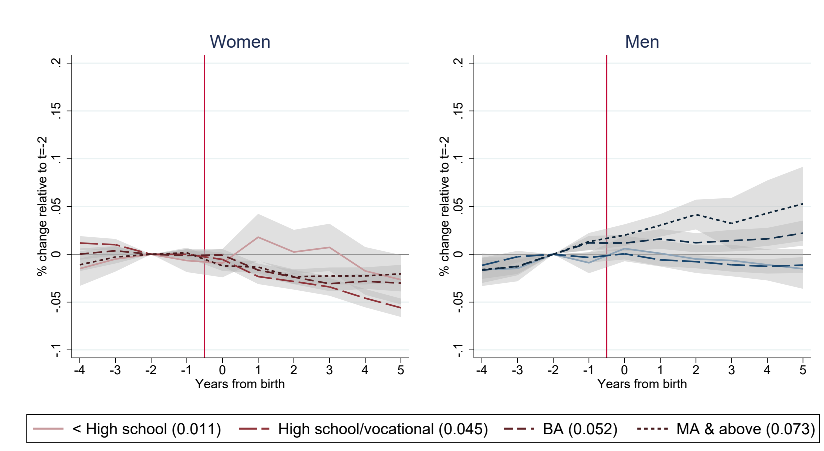
(b) Participation



(c) Hours worked



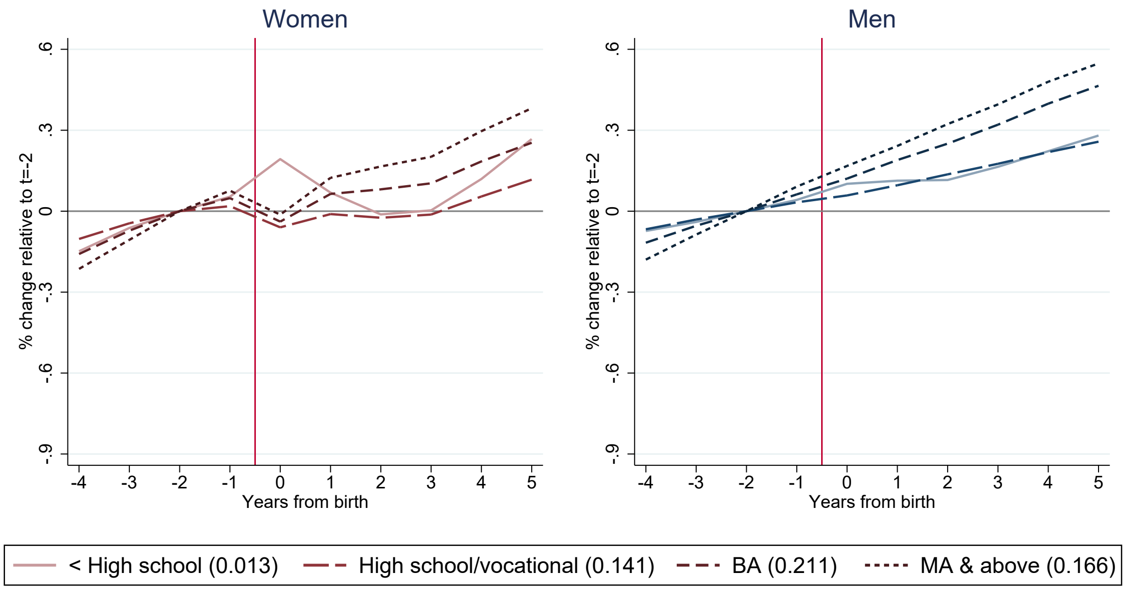
(d) Hourly wages



Notes: The 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.

A.3 Total income

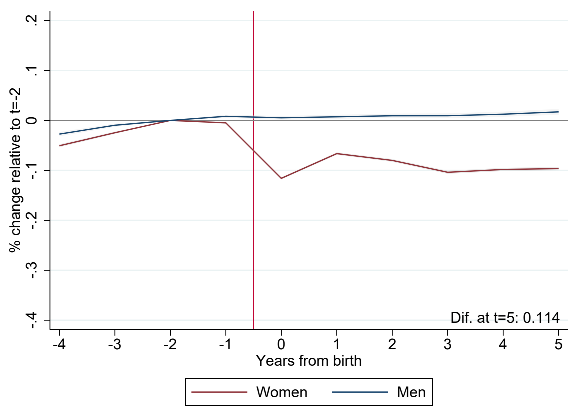
Figure A.9: Earnings and transfer income around first birth



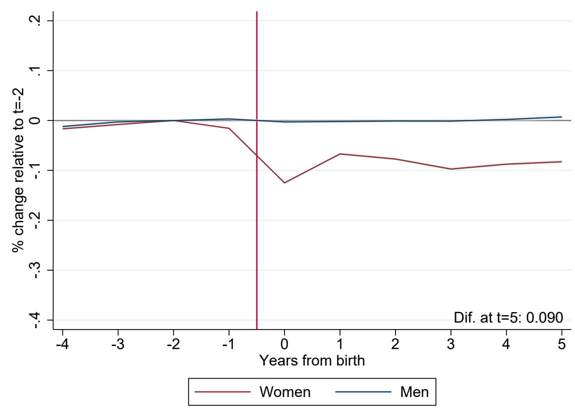
Notes: The figure plots total income relative to two years before first birth, including labor earnings (from the BFL register) and personal transfers (from the ILME register). Percentage point differences at year 5 between women and men with the same education level are included in parentheses in the 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.10: Child penalties, all education levels:
Earnings and transfer income

(a) Without controls for education or years from graduation



(b) With controls for education, interacted with age and years from graduation



Notes: The figure shows estimated child penalties in total income across education levels. Gender differences in estimated five-year penalties are included as notes in the lower right corner of each panel. See Table A.1 for details on the sample. 95% confidence intervals indicated.

B Analysis by high school completion/grades

Table B.1: Summary statistics by high school grade group

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

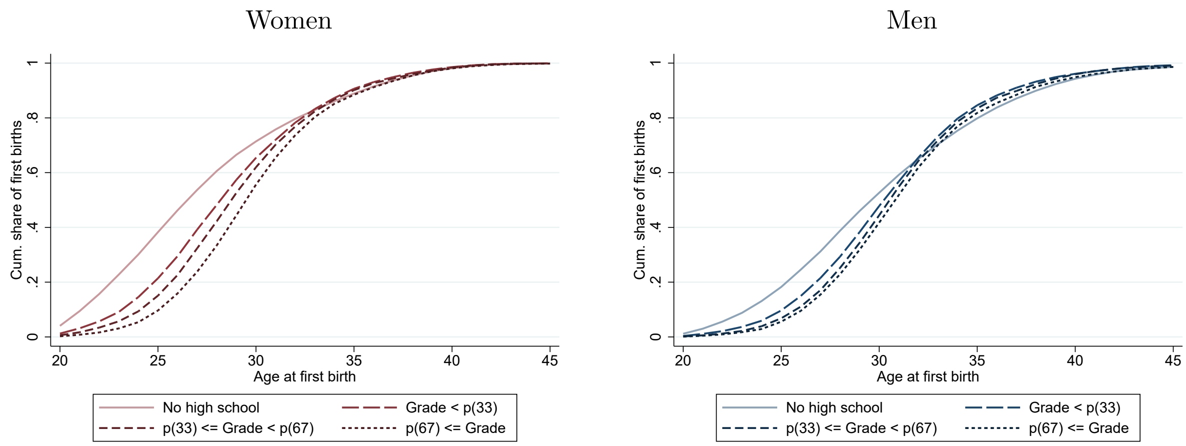
Men

	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

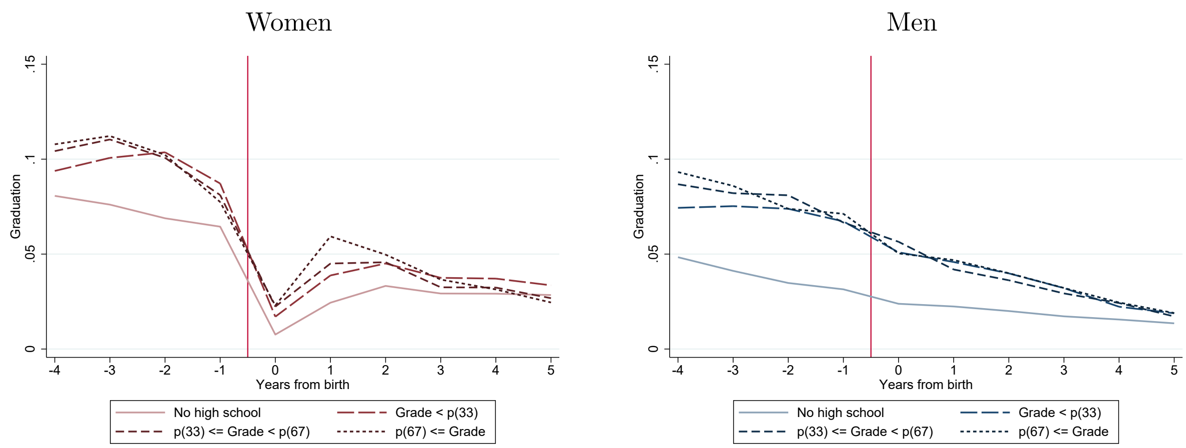
Notes: All characteristics are measured 12 months before first birth, except cumulative fertility, which is measured at the end of the panel. Employment is defined as strictly positive hours of work. Share of month on benefits include all benefits except parental leave and are defined as days registered 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

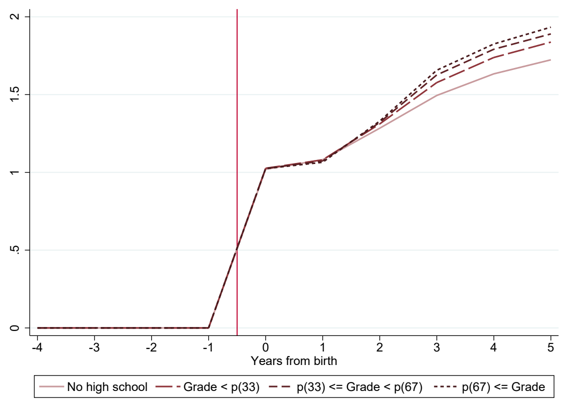


Notes: Panel (a) shows the cumulative share of first births by mothers' and fathers' age (the corresponding density functions are shown in Appendix Figure B.4). Panel (b) shows the probability of graduating in a given year around first birth. All analyses are undertaken separately by skill level as defined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

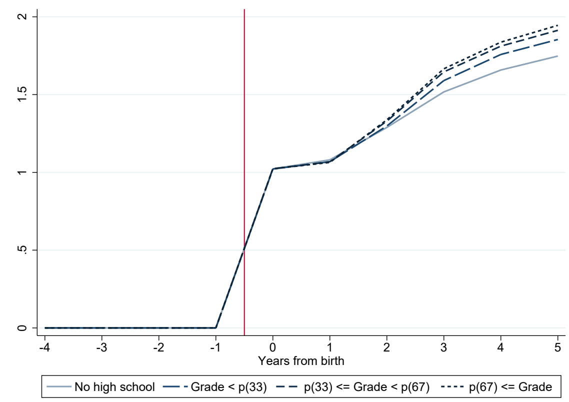
Figure B.2: Cumulative Fertility

(a) Number of children by year from first birth

Women

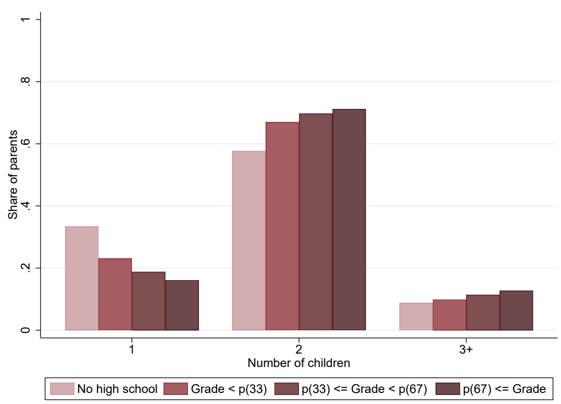


Men

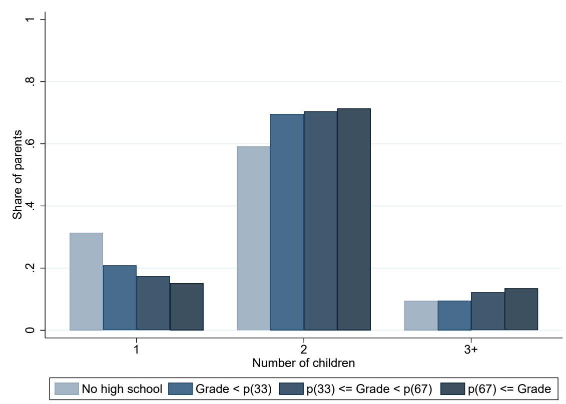


(b) Number of children 5 years from first birth

Women



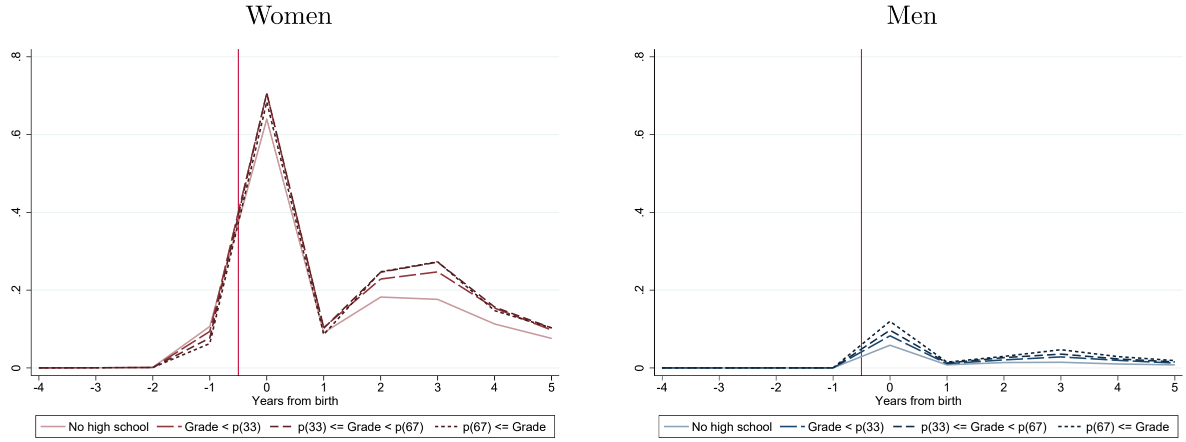
Men



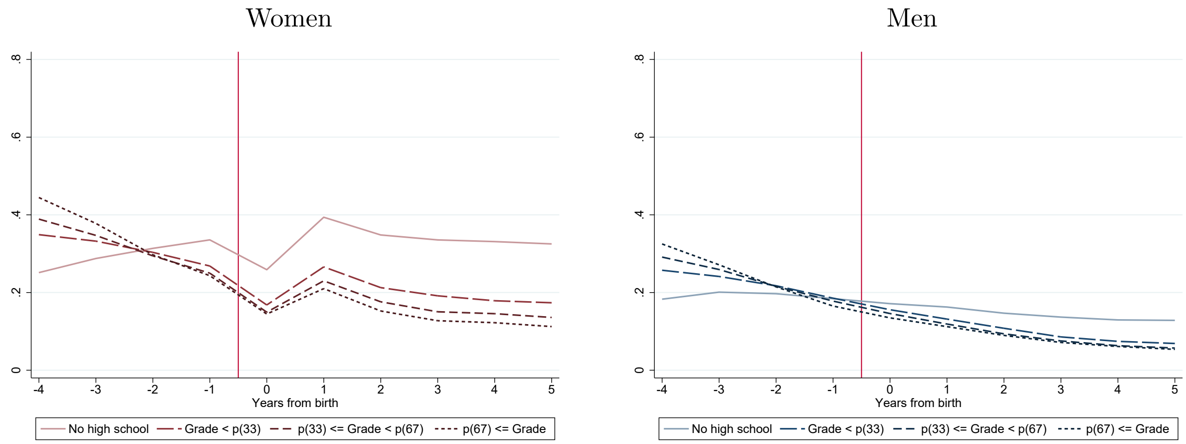
Notes: Panel (a) shows the cumulative number of children each year around first birth. Panel (b) shows the distribution of the number of children by the end of year 5 after first birth. All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table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

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

(a) Share on parental leave

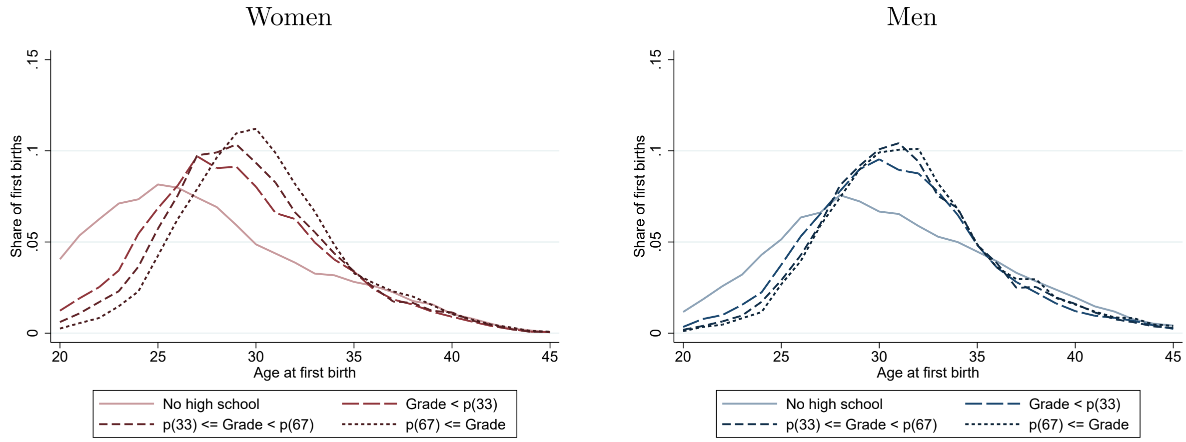


(b) Share on other benefits



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.

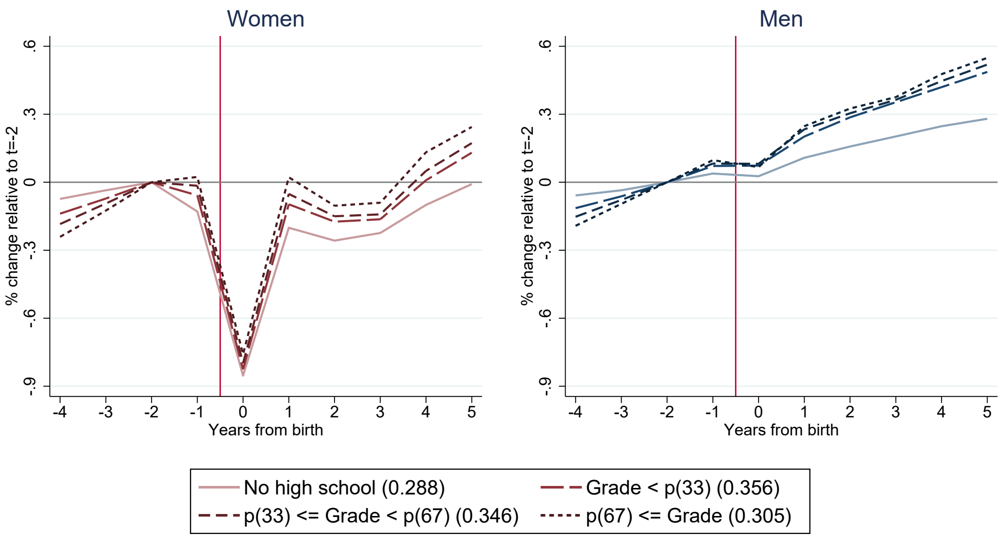
Figure B.4: Share of first births by age at first birth



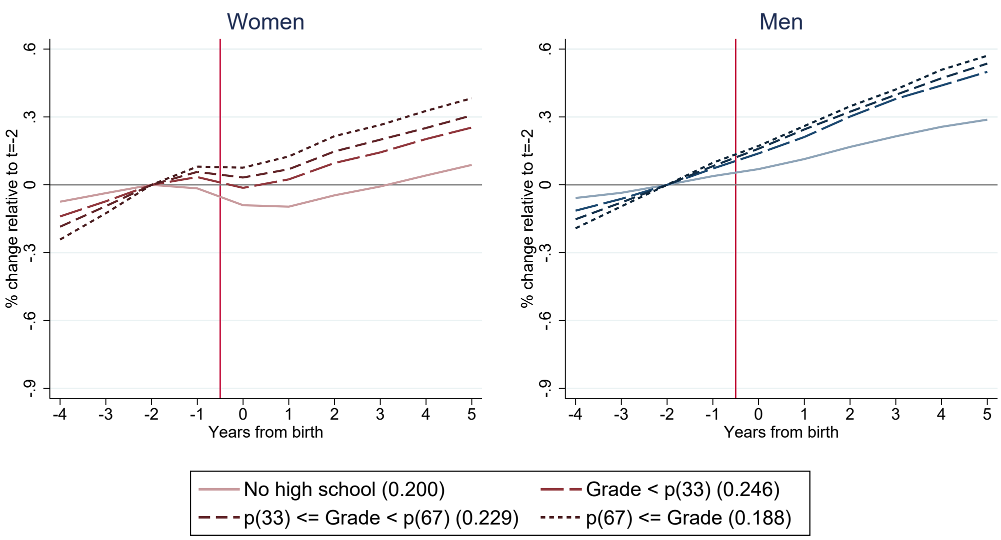
Notes: This figure shows the share of first births by mothers' and fathers' age at first birth. All analyses are undertaken separately by skill level as defined by high school completion and grade rank. See Table B.1 for details on the sample.

Figure B.5: Earnings around first birth

(a) Earnings (set to 0 during PL)



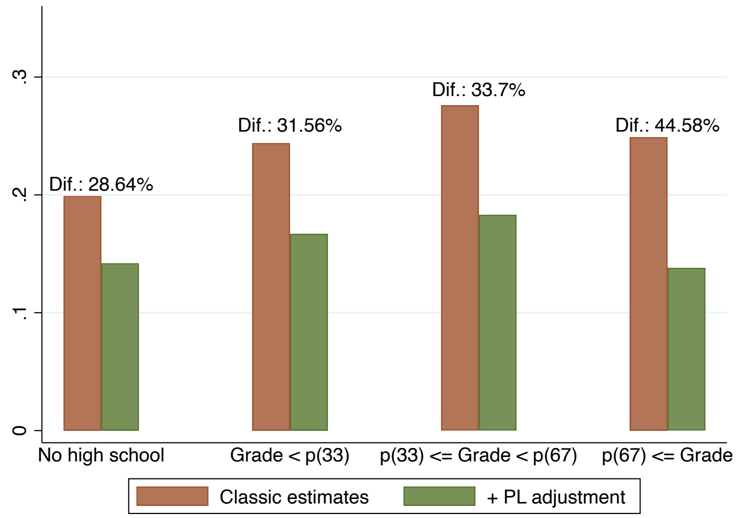
(b) Earnings (set at pre-PL levels during PL)



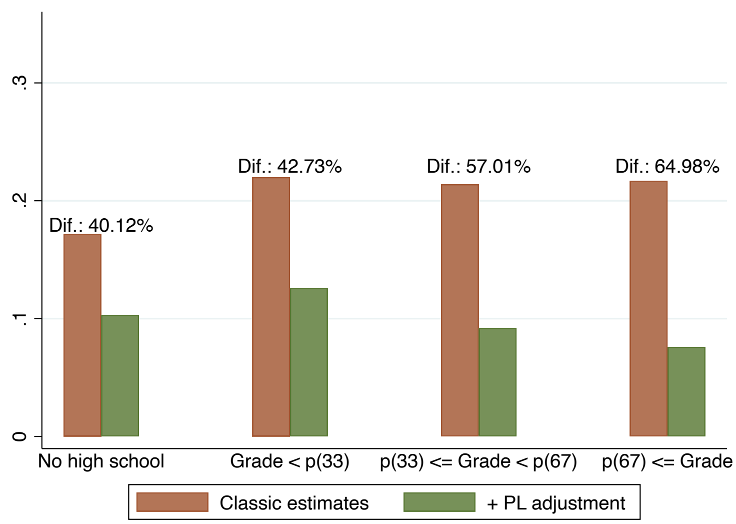
Notes: Panel (a) shows earnings relative to two years before first birth when any earnings during spells of parental leave are set equal to zero. Panel (b) shows earnings relative to two years before first birth when earnings during spells of parental leave are set equal to the level of earnings immediately preceding a spell of parental leave. Percentage point differences at year 5 between women and men with the same skill 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: The 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 Results by number of children

Table C.1: Summary statistics by number of children (as of end of panel)

Women

	1 child	2 children	3 children	All
Age	29.14	27.26	26.46	27.65
Married	0.21	0.20	0.23	0.21
Years from graduation	4.06	1.97	1.54	2.45
Below high school	0.16	0.08	0.09	0.10
High school/vocational	0.44	0.37	0.34	0.39
BA	0.25	0.32	0.33	0.30
MA & above	0.15	0.23	0.24	0.21
Employment	0.75	0.84	0.81	0.81
Share of month on benefit, ex. PL	0.32	0.27	0.30	0.28
Hours per month, inc. zeros	104.00	115.14	109.76	111.80
Hours per month, ex. zeros	138.58	137.78	135.25	137.71
Part-time	0.24	0.24	0.26	0.24
Earnings per month	18914.92	20649.75	19450.45	20093.57
ln(earnings)	3.03	3.01	2.96	3.01
Hourly wage	188.02	182.54	178.89	183.44
ln(hourly wage)	5.14	5.13	5.11	5.13
High school grade rank	44.76	50.38	52.52	49.54
Parental leave, year 5	0.04	0.20	0.63	0.21
N	15795	40872	6399	63066

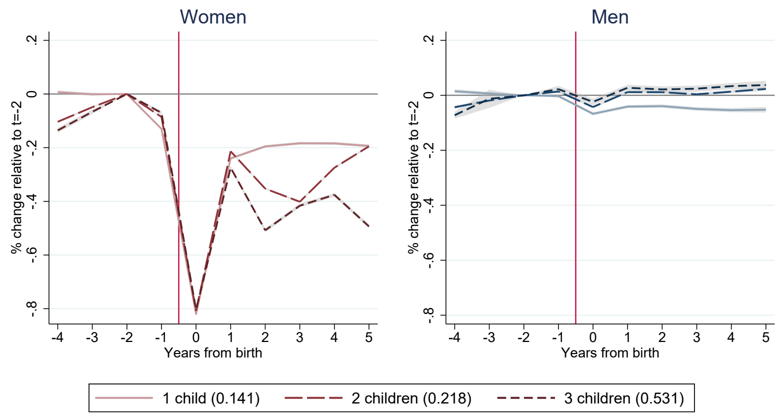
Men

	1 child	2 children	3 children	All
Age	30.91	29.18	28.30	29.54
Married	0.21	0.21	0.22	0.21
Years from graduation	7.17	4.28	3.50	4.95
Below high school	0.22	0.10	0.12	0.14
High school/vocational	0.54	0.51	0.48	0.52
BA	0.13	0.18	0.18	0.17
MA & above	0.11	0.20	0.23	0.18
Employment	0.79	0.87	0.84	0.85
Share of month on benefit, ex. PL	0.23	0.18	0.21	0.19
Hours per month, inc. zeros	116.04	127.67	122.03	124.07
Hours per month, ex. zeros	146.88	146.95	145.57	146.80
Part-time	0.19	0.18	0.20	0.19
Earnings per month	23894.85	26657.19	25351.11	25805.79
ln(earnings)	3.24	3.26	3.22	3.25
Hourly wage	210.15	211.39	207.97	210.75
ln(hourly wage)	5.25	5.27	5.25	5.26
High school grade rank	44.54	48.88	52.50	48.54
Parental leave, year 5	0.01	0.10	0.40	0.11
N	15456	37907	6023	59386

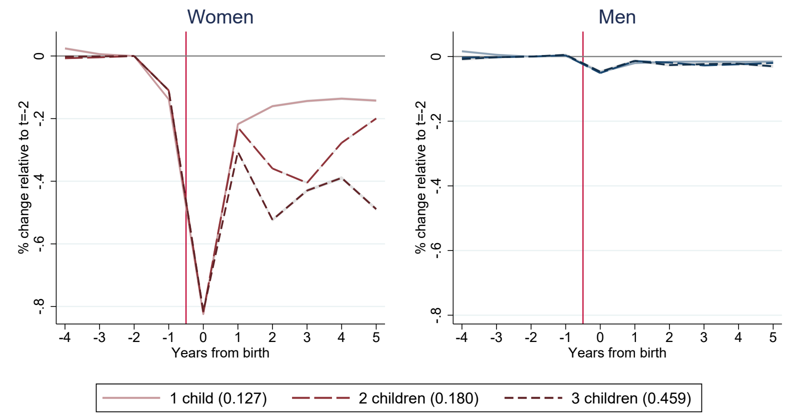
Notes: All characteristics are measured 12 months before first birth. Employment is defined as strictly positive hours of work. Share of month on benefits include all benefits except parental leave and are defined as days registered on benefits divided by total number of days in a given month. Part-time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

Figure C.1: Child penalties by number of children by the end of the panel:
Without controls for years from graduation; Earnings, participation and hours = 0 during PL

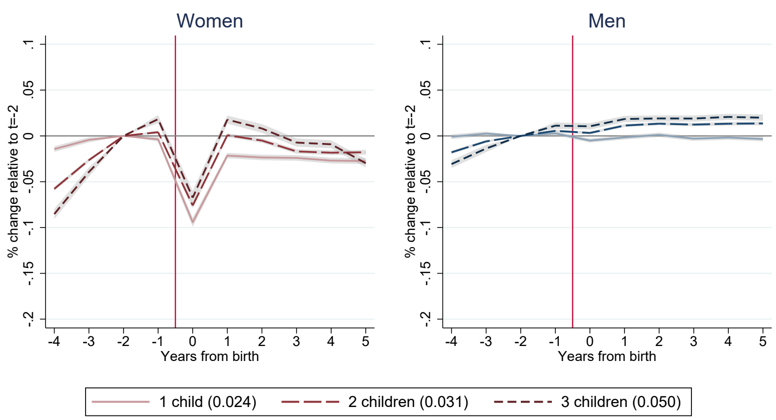
(a) Earnings



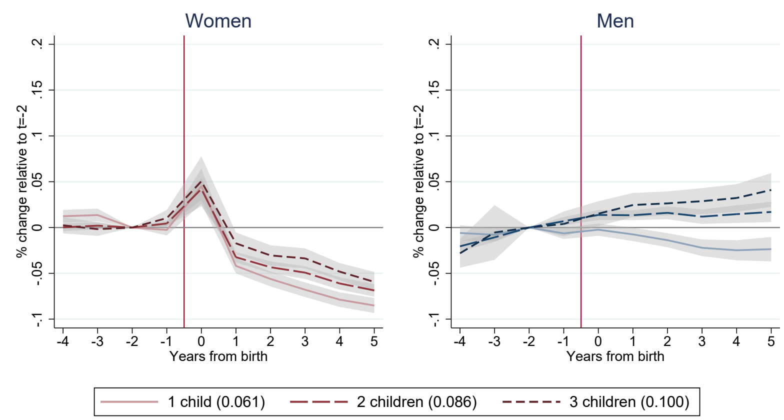
(b) Participation



(c) Hours worked

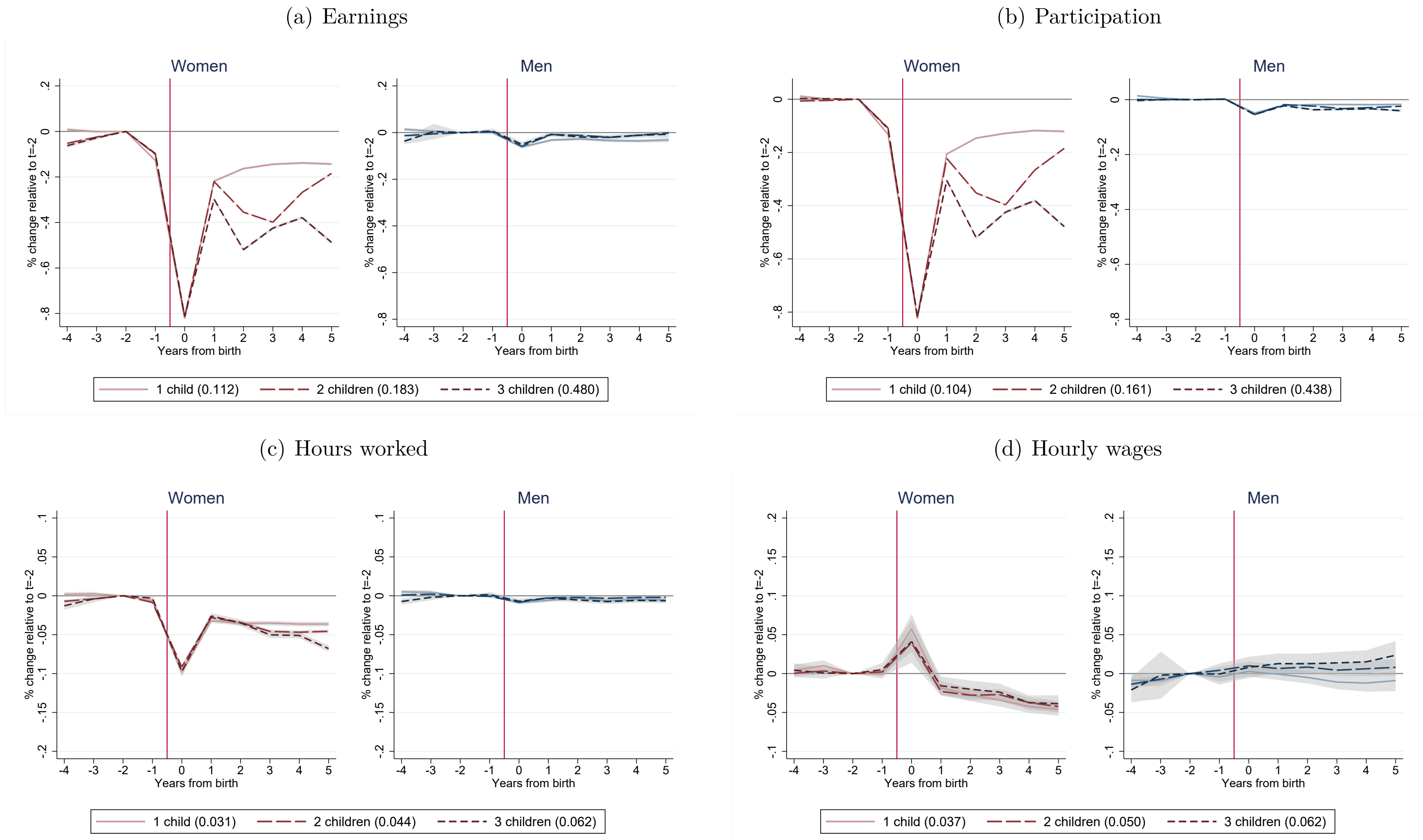


(d) Hourly wages



Notes: The figure illustrates estimates of child penalties by number of children by the end of the panel 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 number of children at the end of the panel. See Table C.1 for details on the sample. 95% confidence intervals indicated.

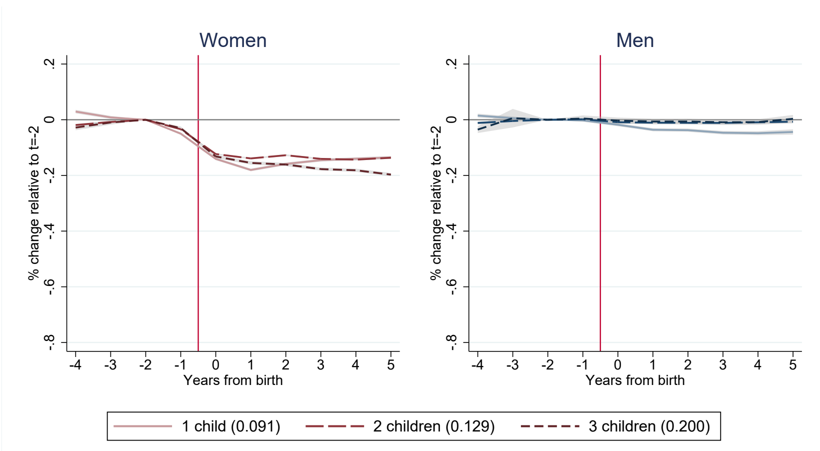
Figure C.2: Child penalties by number of children by the end of the panel:
With controls for years from graduation; Earnings, participation and hours = 0 during PL



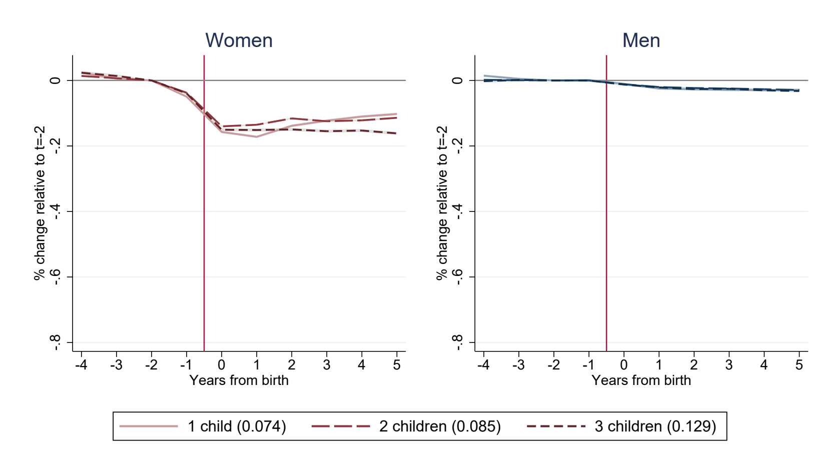
Notes: The figure illustrates estimates of child penalties by number of children by the end of the panel with controls for years from graduation and treating outcomes as zeros during spells of parental leave. Panel (a) shows results for earnings, Panel (b) for participation, Panel (c) for hours worked, 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 number of children at the end of the panel. See Table C.1 for details on the sample. 95% confidence intervals indicated.

Figure C.3: Child penalties by number of children by the end of the panel:
With controls for years from graduation; Earnings, participation and hours during PL set at pre-PL levels

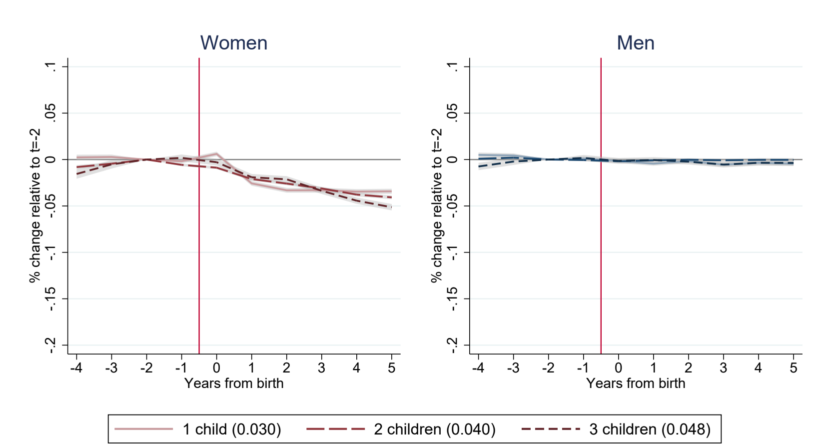
(a) Earnings



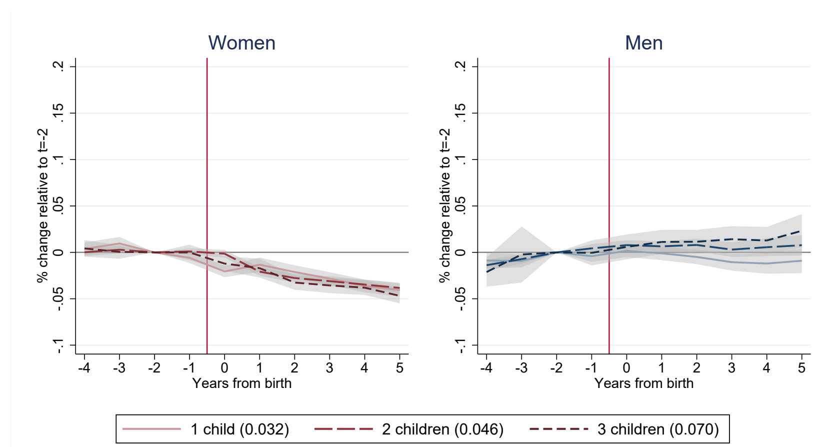
(b) Participation



(c) Hours worked



(d) Hourly wages



Notes: The figure illustrates estimates of child penalties by number of children by the end of the panel 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 number of children at the end of the panel. See Table C.1 for details on the sample.

D Robustness: Parental cohorts born 1970-1975

Table D.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
Mean	1.86		1.86		1.99		1.85		1.87	
Mean (1+ child)	2.33		2.14		2.24		2.13		2.15	
Total	28306		102120		60398		33233		224057	

Men

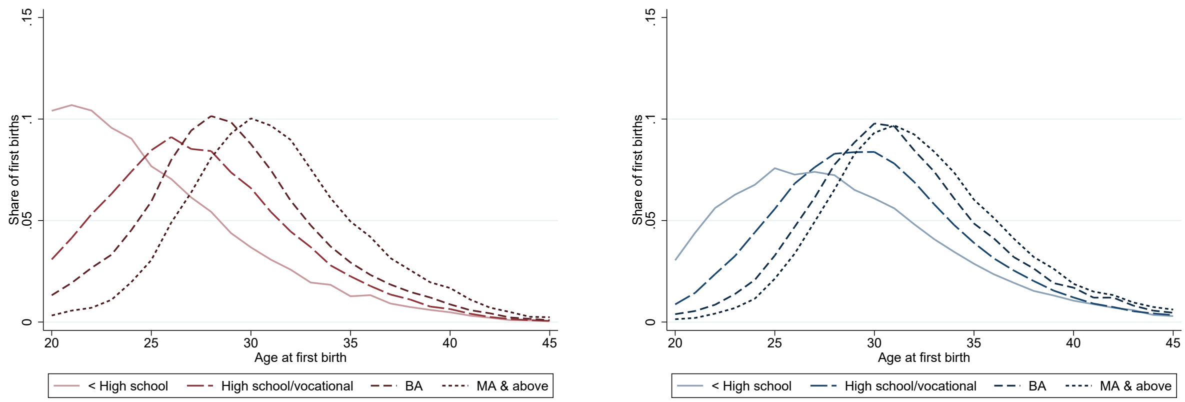
	< HS		HS/voca.		BA		MA & above		All	
No. of children	Count	Share	Count	Share	Count	Share	Count	Share	Count	Share
0	12958	0.31	25771	0.21	5423	0.17	5258	0.16	49410	0.22
1	7135	0.17	19361	0.16	4262	0.13	3800	0.12	34558	0.15
2	12012	0.29	50665	0.41	14011	0.44	14239	0.44	90927	0.40
3	6335	0.15	21484	0.17	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
Mean	1.56		1.69		1.84		1.85		1.73	
Mean (1+ child)	1.93		2.06		2.39		2.34		2.16	
Total	42073		123570		31976		32054		229673	

Notes: The statistics reported refer to 1970-1975 birth cohorts observed at age 45 in the BEF Register. The number of children by age 45 should approximate completed fertility. Education levels are observed at age 45.

Figure D.1: Age at first birth

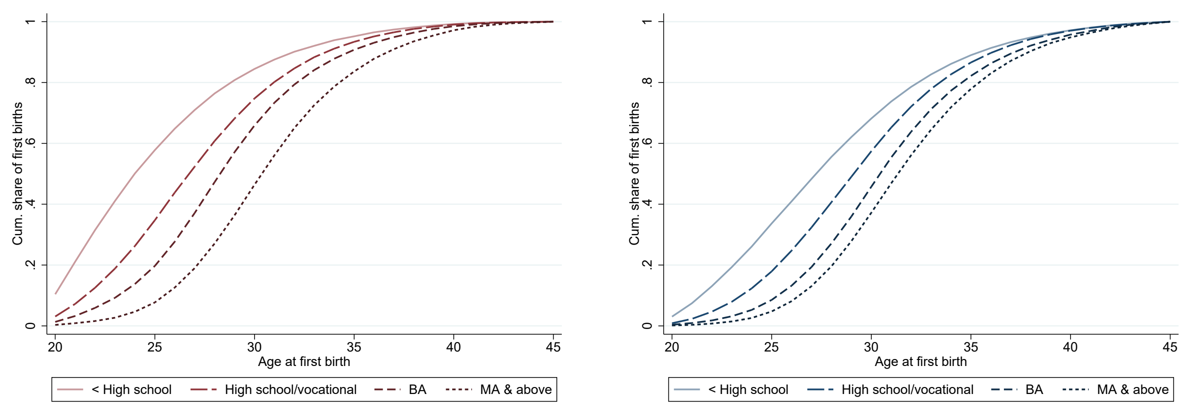
(a) Share of 1st births

Women Men



(b) Cumulative share of parenthood

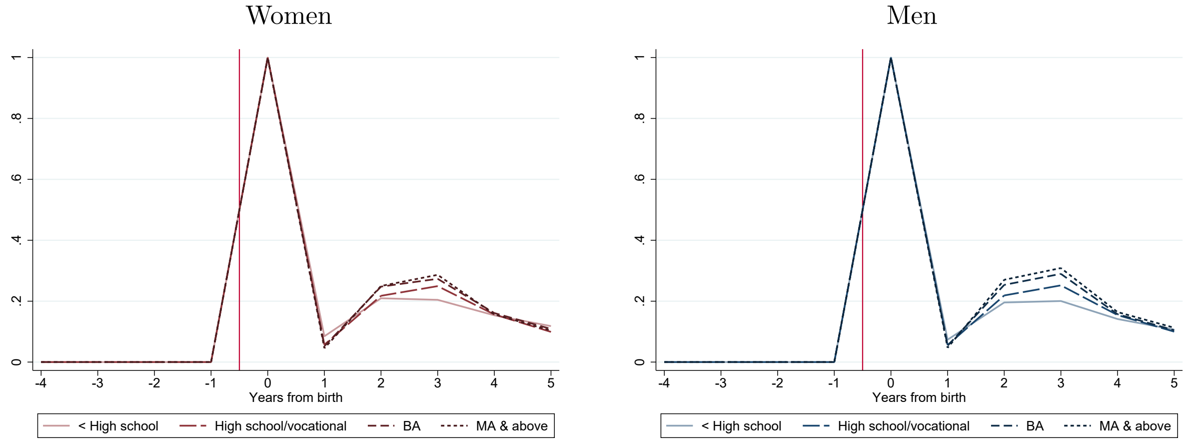
Women Men



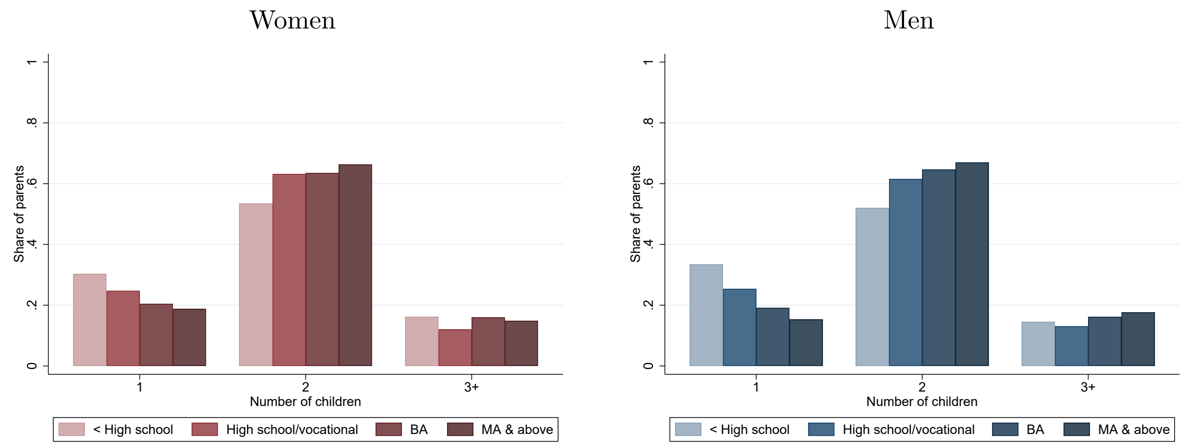
Notes: Panel (a) shows the share of first births by mothers' and fathers' age at first birth. Panel (b) shows the cumulative share of first births by mothers' and fathers' age. All analyses are undertaken separately by observed education levels at age 45. See Table D.1 for details on the sample.

Figure D.2: Fertility within 5 years of first birth

(a) Probability of birth



(b) Number of children 5 years from first birth



Notes: Panel (a) shows the probability of birth/having a child around first birth. Panel (b) shows the distribution of number of children 5 years after year of first birth. All analyses are undertaken separately by observed education levels at age 45. See Table D.1 for details on the sample.