

# Children and Gender Inequality: Evidence from Denmark\*

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## Abstract

Using Danish administrative data, we study the impacts of children on gender inequality in the labor market. The arrival of children creates a long-run gender gap in earnings of around 20% driven by hours worked, participation and wage rates. We identify mechanisms driving these “child penalties” in terms of occupation, sector, and firm choices. We find that the fraction of gender inequality caused by child penalties has featured a dramatic increase over the last three-four decades. Finally, we show that child penalties are transmitted through generations, from parents to daughters, suggesting an influence of childhood environment on gender identity.

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# 1 Introduction

Despite considerable gender convergence over the last century, gender inequality in earnings and wage rates continues to be substantial in all countries and the process of convergence has slowed down. The early literature on gender inequality in the labor market focused on the role of human capital and discrimination (Altonji & Blank 1999), but the disappearance of gender differences in education and the implementation of anti-discrimination policies suggest that the explanation for the remaining gender gap lies elsewhere. Based on administrative data from Denmark, we provide a simple explanation for the persistence of gender inequality: the effects of children on the careers of women relative to men are large and have not fallen over time. As a result, almost all of the remaining gender inequality can be attributed to children.

To estimate the impact of children on the labor market trajectories of women relative to men, we adopt a quasi-experimental approach based on event studies around the birth of the first child. For a range of labor market outcomes, we find large and sharp effects of children: women and men evolve in parallel until the birth of their first child, diverge sharply immediately after child birth, and do not converge again. Defining the “child penalty” as the percentage by which women fall behind men due to children, we find that the long-run child penalty in earnings equals about 20% over the period 1980-2013.<sup>1</sup> This should be interpreted as a total penalty including the costs of children born after the first one, and we show that the penalty is increasing in the number of children. The earnings penalty can come from three margins — labor force participation, hours of work, and the wage rate — and we find sharp effects on all three margins that are roughly equal in size.

We also use the event study approach to shed light on a range of underlying mechanisms that may drive the earnings and wage rate impacts. We show that children affect the job characteristics of women relative to men in a way that favor family amenities over pecuniary rewards. Specifically, just after the birth of the first child, women start falling behind men in terms of their occupational rank and their probability of becoming manager. Furthermore, women switch jobs to firms that are

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<sup>1</sup>We use the term “child penalty” throughout the paper, because it is a standard term in the literature. However, whether or not this should be viewed as a “penalty” on women depends on the underlying mechanisms driving it. For example, if the effect is driven by women voluntarily selecting into positions that have valuable family amenities (but lower wages), then the effect is not necessarily a penalty as such. We present evidence on such mechanisms as described below.

more “family friendly” proxied either by being in the public sector (which is known for its family friendliness) or by having women with young children in the management. The importance of the family friendliness of occupations and firms for gender equality has been much discussed in recent work (e.g. [Goldin 2014](#); [Goldin & Katz 2016](#)), but here we provide clean event study evidence that these qualitative dimensions directly respond to the arrival children.

Having estimated child penalties in the full population — allowing them to vary both across event time and across birth cohorts — we are able to decompose aggregate gender inequality over time into child-related inequality and residual inequality. We show that the fraction of total earnings inequality caused by child penalties has doubled over time, from about 40% in 1980 to about 80% in 2013. This dramatic change reflects a combination of two underlying changes: (i) child-related gender inequality in earnings has increased from about 18% to 20%, and (ii) total gender inequality in earnings has fallen from about 46% to 24%. To understand the first effect, note that although the percentage child penalty has fallen slightly over time, the penalty operates on a larger base due to the general increase in the earnings of women relative to men. While existing work points to the importance of parenthood for gender gaps, our decomposition analysis goes further by quantifying just how much of aggregate gender inequality is due to children and how this has evolved over time. The striking finding from this analysis is that over time gender inequality converges to the impact of children.

It is worth highlighting that our dynamic decomposition analysis represents a departure from standard gender gap decompositions (see [Blau & Kahn 2016](#) for a review) both in terms of the variation used for identification (within-person variation as opposed to cross-sectional variation) and in terms of the question asked. Standard decomposition approaches focus on the extent to which men and women receive unequal pay for equal work: the unexplained gender gap after controlling for human capital and labor market variables, but not children. By contrast, our goal is to study the impact of children on gender inequality, not controlling for labor market variables (such as occupation and firm choices) that are transmission mechanisms for children. This is a conceptually different question: Even with perfectly equal pay for equal work — a zero gap in standard decompositions — our analysis would still show large child-related gender inequality as equal work is in practice not an option for most women with children.<sup>2</sup>

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<sup>2</sup>Our finding that child penalties represent an increasing share of gender inequality over time is consistent with the observation from standard cross-sectional decomposition analyses that the unexplained part of the gender gap has been increasing over time (see e.g., [Goldin 2014](#); [Blau & Kahn 2016](#)). A contribution of our event study-based decomposition analysis is to show, in a causal sense, that the unequal effects of children are responsible for the increasing share of the unexplained gender gap.

Why are female child penalties so persistent after decades of effort to create gender equality through equal opportunity legislation, child care policies, and job-protected parental leave? While fully answering this question is beyond the scope of our paper, we provide evidence on one possible explanation: child penalties are transmitted through generations. We estimate the intergenerational transmission of child penalties by exploiting that our administrative measure of hours worked goes back to 1964, allowing us to relate the estimated child penalties between 1980-2013 to the within-family work history one generation before. We find that female child penalties are strongly related to the labor supply history of the maternal grandparents, but not the paternal grandparents, even after controlling for a rich set of family characteristics. For example, in traditional families where the mother works very little compared to the father, their daughter incurs a larger child penalty when she eventually becomes a mother herself. Our findings are consistent with an influence of nurture in the formation of women's preferences over family and career. This analysis is related to the work by [Fernandez \*et al.\* \(2004\)](#), but focusing on the intergenerational transmission of child penalties (as opposed to labor supply levels) between parents and their daughters (as opposed to daughters-in-law). The analysis is also related to recent work on the importance of gender identity norms for labor market outcomes ([Bertrand 2011](#); [Bertrand \*et al.\* 2013](#)). Our findings suggest that female gender identity is formed during a girl's childhood based on the gender roles of her parents.

Our paper contributes primarily to two literatures. First and foremost, we contribute to the enormous literature on gender inequality in the labor market as reviewed by for example [Altonji & Blank \(1999\)](#), [Bertrand \(2011\)](#), [Blau & Kahn \(2016\)](#) and [Olivetti & Petrongolo \(2016\)](#). Much of this literature has focused on the role of human capital, occupation and discrimination in explaining gender gaps, but there is also a sizeable amount of work on the role of parenthood. This includes papers by [Waldfogel \(1998\)](#), [Lundberg & Rose \(2000\)](#), [Sigle-Rushton & Waldfogel \(2007a,b\)](#), [Correll \*et al.\* \(2007\)](#), [Paull \(2008\)](#), [Bertrand \*et al.\* \(2010\)](#), [Wilde \*et al.\* \(2010\)](#), [Fernandez-Kranz \*et al.\* \(2013\)](#), [Fitzenberger \*et al.\* \(2013\)](#), [Goldin \(2014\)](#), [Adda \*et al.\* \(2015\)](#), [Angelov \*et al.\* \(2016\)](#), and [Goldin & Katz \(2016\)](#). Our paper is most closely related to the case study of MBA graduates from Chicago Booth School of Business by [Bertrand \*et al.\* \(2010\)](#), and to the paper by [Angelov \*et al.\* \(2016\)](#) who estimate child penalties in annual earnings and monthly wage rates using Swedish administrative data and an event study approach. Starting from related event studies, we contribute to the literature by providing evidence on mechanisms (the dynamics of occupation, sector, and firm choices), by investigating causal identification using different approaches, by developing a dynamic decomposition approach to estimating long-run trends in child-related gender inequality, and finally by

studying the intergenerational transmission of child penalties.

Second, we also contribute to the literature on children and family labor supply. This literature has focused on the potential endogeneity of children (e.g. [Browning 1992](#)), proposing instruments for the number of children such as twin births ([Rosenzweig & Wolpin 1980](#); [Bronars & Grogger 1994](#)) and sibling sex mix ([Angrist & Evans 1998](#)). While our primary objective is to study gender inequality, our analysis also yields estimates of the impact of children on hours worked and labor force participation, separately for males and females. Our event study approach relies on a different source of variation and captures a different impact than the existing IV-approaches. As we clarify, the event study approach has the potential to capture the global treatment effect of all children in the population, as opposed to only the local treatment effect of a second child or a third child obtained from the twin or sibling sex mix instruments.<sup>3</sup> We provide two identification checks of the event study approach. First, we compare the local treatment effect of a third child based on a sex-mix IV to the local treatment effect of a third child that can be obtained from our event study approach. We show that the IV estimates and the event study estimates are almost perfectly aligned. Second, we provide a difference-in-differences extension of our event study, using those who never have children as a control group. The difference-in-differences event study produces impacts of children that are very similar to our baseline results.

Finally, we note that children may have two conceptually different effects on labor market outcomes. One is a pre-child effect of anticipated fertility: women may invest less in education or select family friendly career paths in anticipation of motherhood. The other is a post-child effect of realized fertility: women changing their hours worked, occupation, sector, firm, etc., in response to actual motherhood. The event study approach cannot capture the pre-child effect; it is designed to identify only the post-child effect. If women invest less in education and career in anticipation of motherhood, then our estimated child penalties represent lower bounds on the total lifetime impacts of children. The fact that these child penalties can account for most of the current gender inequality leaves relatively little room for pre-child effects to operate today, but it is possible that such effects were more important at the beginning of the period we study.<sup>4</sup> We provide descriptive evidence that, while the effect of child penalties on gender inequality has been growing over time,

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<sup>3</sup>We focus on the twin and sex-mix instruments as these have been the most influential approaches, but other instruments have been proposed that might help uncover the effect of the first child as well. These instruments include miscarriages ([Hotz et al. 2005](#)), infertility shocks ([Aguero & Marks 2008](#)), and IVF treatment outcomes ([Lundborg et al. 2014](#)).

<sup>4</sup>In general, identifying anticipation effects of children is difficult without making strong structural assumptions and we make no such attempt here. Taking a structural approach, [Adda et al. \(2015\)](#) estimate that occupational choices due to anticipated fertility represent a very small fraction of the total earnings loss from children.

the effect of pre-child human capital investments has fallen. This is consistent with a shift from pre-effects to post-effects of children as a society develops: while women used to pay the career cost of children upfront, they now invest in education and careers almost at the level of men, but still end up paying the child penalty after motherhood.

The paper is organized as follows. Section 2 describes the institutional background and data, section 3 lays out the event study methodology and estimates the impacts of children, section 4 presents our dynamic decomposition of gender inequality, section 5 analyzes intergenerational transmission, and section 6 concludes.

## 2 Institutional Background and Data

### 2.1 Institutional Background

Scandinavian countries have been praised for offering better opportunities for women to balance career and family than most other countries. This view is based on the presence of generous family policies — job-protected parental leave and public provision of child care — and a perception that gender norms are comparatively egalitarian in Scandinavia. Consistent with this view, Denmark has one of the highest female labor force participation rates in the world, currently around 80% as opposed to around 70% in the United States, and it has almost no remaining gender gap in participation rates.

The perception of Scandinavia as being gender equal is not without merit, but two figures in the online appendix give rise to pause. Figure A.I shows that the gender gap in earnings is still substantial in Scandinavian countries, and that it is no longer very different from the gender gap observed in the United States. That is, while gender inequality in Denmark used to be dramatically lower than in the United States, the gender pay gap is currently between 15-20% in both countries and appears to have stabilized at that level. Despite the presence of very different public policies and labor markets in these countries, their gender gaps are converging over time.

Figure A.II probes the idea that gender norms are more egalitarian in Scandinavia than elsewhere. The evidence presented is based on questions from the International Social Survey Program (ISSP) on the attitudes that people have towards market work by women with and without children. Two insights emerge from the figure: one is that gender attitudes are still quite traditional — essentially that women should not be working full-time when they have children living at home — and the other is that different countries are very similar in holding this view. While the Scandina-

vian populations are somewhat more open to the idea that women with young children work part time (rather than staying at home entirely) compared to the U.S., the similarities in gender attitudes stand out much more than the differences. Overall, the evidence in Figures [A.I-A.II](#) raises doubts about the degree to which Scandinavian countries are strong outliers in terms of gender equality in the labor market.

The policy environment in Denmark is one which combines large tax-transfer distortions (which may affect the gender gap due to differential labor supply elasticities between men and women) and generous family policies intended to support female labor supply. As shown by [Kleven \(2014\)](#), the effective tax rate on labor earnings is exceptionally large in Denmark, but so are the implicit subsidies to labor supply through publicly provided child care and public spending on other goods that are complementary to working (transportation, elder care, education, etc.). Over the period we consider, public child care is universally provided at a heavily subsidized price from around 6-12 months after birth. Until the child reaches the age where public child care becomes available, there is job-protected and paid maternity and parental leave. Up until 2001, parents were offered 14 weeks of maternity leave followed by 10 weeks of parental leave to be shared between the mother and father. Since 2002 this has been extended to 18 weeks of maternity leave and 32 weeks of parental leave. Hence, throughout the period we consider, parents were covered first by paid leave and then by public child care, with no gap between the two.

## 2.2 Data

The analysis is based on administrative data for the full population in Denmark between 1980-2013. For the study of intergenerational transmission we exploit additional administrative data going back to 1964. The Danish data combine several administrative registers (linked at the individual level via personal identification numbers) and contains rich information on children, earnings, labor supply, occupation, firms, education, and many other variables. Crucially, the data allows us to link family members, generations, and workers with firms.

Our main event study analysis is based on *first* child births where the parents are observed every year between 5 years before having a child and 10 years after. We are thus focusing on first child births between 1985-2003 where the parents are known, alive and reside in Denmark throughout a 15-year window around the birth. We do not impose restrictions on the relationship status of the parents: we include all individuals who have a child in a given year and follow them through the 15-year window whether or not they are married, cohabiting, separated, divorced, or have not yet

formed a couple in any given year. This leaves us with a core estimation sample of around 470,000 births or 15,040,000 individual-year observations.

We estimate child penalties in earnings, labor force participation, hours worked, and wage rates (earnings/hours worked for those who are working). Our ability to estimate child penalties in hours worked and wage rates using sharp event studies relies on a unique administrative and third-party reported measure of hours worked that is available for the full population. This measure comes from a mandated pension scheme introduced in 1964 — *Arbejdsmarkedets Tillægspension* (ATP) — that require all employers to contribute on behalf of their employees based on individual hours worked.<sup>5</sup> The pension contribution is a function of hours worked in discrete steps, namely four bins of weekly hours (0-8, 9-17, 18-26, 27-) for someone paid weekly or four bins of monthly hours (0-38, 39-77, 78-116, 117-) for someone paid monthly, with the latter being much more common. Hence the annual pension contribution for someone paid monthly depends on  $\sum_{i=1}^{12} h_i$  where monthly hours  $h_i$  has 4 steps, which gives an annual hours measure in 37 steps ( $= 4 \times 12 - 12 + 1$ ). Our measure of the wage rate is defined as earnings divided by this ATP hours measure.<sup>6</sup>

Because the ATP hours measure is capped, it does not capture marginal hours adjustments for those working every month of the year in the highest hours bin. For a given child penalty in earnings, this will make us underestimate the penalty in hours worked and correspondingly overestimate the penalty in wage rates. The hours measure does capture larger labor supply adjustments such as switches to different levels of part-time work and work interruptions within the year, which are important adjustments for women with children. The key advantage of our measure is that it is precisely measured for the full population over a very long time period, unlike labor market surveys that have considerable measurement error and small samples.

### 3 Impacts of Children

#### 3.1 Event Study Methodology

The ideal experiment for studying the impact of children would be to randomize fertility. Absent such an experiment, researchers have proposed instruments for the number of children such as twin births (Rosenzweig & Wolpin 1980; Bronars & Grogger 1994) and sibling sex mix (Angrist & Evans 1998). Such instruments can provide insights on the local effect of a second child or a

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<sup>5</sup>The scheme also covers self-employed individuals who contribute on their own behalf.

<sup>6</sup>Note that we use mid-point within each bracket of annual hours to compute the wage rate.



third child, but they cannot provide estimates of the total effect of children — and in particular of the first child — in the population. This limits the usefulness of such approaches for our agenda, which is to understand the implications of parenthood for gender inequality. To investigate this question, we adopt an event study approach based on sharp changes around the birth of the first child for mothers relative to fathers. Although fertility choices are not exogenous, the event of having a first child generates sharp changes in labor market outcomes that are arguably orthogonal to unobserved determinants of those outcomes as they should evolve smoothly over time. The event study approach has the additional advantages of tracing out the full dynamic trajectory of the effects, and of being very precise as it exploits individual-level variation in the timing of first births. We spell out the identification assumptions of our approach in appendix B.

For each parent in the data we denote the year in which the individual has his/her first child by  $t = 0$  and index all years relative to that year. Our baseline specification considers a balanced panel of parents who we observe every year between 5 years before having their first child and 10 years after, and so event time  $t$  runs from  $-5$  to  $+10$ . To investigate the very long run, we also present results for a sample of parents who we observe up to 20 years after the birth of their first child. We study the evolution of wide set of labor market outcomes as a function of event time. Specifically, denoting by  $Y_{ist}^g$  the outcome of interest for individual  $i$  of gender  $g$  in year  $s$  and at event time  $t$ , we run the following regression separately for men and women

$$Y_{ist}^g = \sum_{j \neq -1} \alpha_j^g \cdot \mathbf{I}[j = t] + \sum_k \beta_k^g \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \gamma_y^g \cdot \mathbf{I}[y = s] + \nu_{ist}^g, \quad (1)$$

where we include a full set of event time dummies (first term on the right-hand side), age dummies (second term) and year dummies (third term). We omit the event time dummy at  $t = -1$ , implying that the event time coefficients measure the impact of children relative to the year just before the first child birth. If we did not include age and year dummies, the estimated event coefficients  $\hat{\alpha}_t^g$  would correspond simply to the mean value of the outcome at event time  $t$ , relative to the year before birth. By including a full set of age dummies we control non-parametrically for underlying life-cycle trends, and by including a full set of year dummies we control non-parametrically for time trends such as wage inflation and business cycles. We are able to identify the effects of all three sets of dummies because, conditional on age and year, there is variation in event time driven by variation in the age at which individuals have their first child. The inclusion of age dummies is important for comparing men and women, because women tend to be younger than men when

having their first child.

We specify equation (1) in levels rather than in logs to be able to keep the zeros in the data (due to non-participation). We convert the estimated level effects into percentages by calculating  $P_t^g \equiv \hat{\alpha}_t^g / E[\tilde{Y}_{ist}^g | t]$  where  $\tilde{Y}_{ist}^g$  is the predicted outcome when omitting the contribution of the event dummies, i.e.  $\tilde{Y}_{ist}^g \equiv \sum_k \hat{\beta}_k^g \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \hat{\gamma}_y^g \cdot \mathbf{I}[y = s]$ . Hence,  $P_t^g$  captures the year- $t$  effect of children as a percentage of the counterfactual outcome absent children. Estimating percentage effects based on a level-specification (rather than a log-specification) may raise the concern that the estimates are mainly driven by the effects at the top of the distribution, especially when considering earnings as the outcome. We will present quantile regressions of (1) that rule out this concern.

Having estimated the impacts of children on women and men separately, we define a *child penalty* on women relative to men at event time  $t$  as

$$P_t \equiv \frac{\hat{\alpha}_t^m - \hat{\alpha}_t^w}{E[\tilde{Y}_{ist}^w | t]}. \quad (2)$$

This child penalty measures the percentage by which women are falling behind men due to children at event time  $t$ . Two points are worth noting about this measure. First, while the identification of short-term child penalties (say  $P_0$  or  $P_1$ ) rely primarily on a smoothness assumption common to all event studies, the identification of long-term penalties (say  $P_{10}$  or  $P_{20}$ ) requires stronger assumptions and may call for the use of a control group (such as men and women who do not have children) or an instrument. We verify our event study estimates using these alternative strategies later. Second, while our approach is based on the event of having the first child, long-run child penalties will include the impact of children born after to the first one, unless of course we condition the sample on having only one child in total. Hence, long-run child penalties have the potential to capture the total effect of children on gender inequality.

### 3.2 Estimating the Impacts of Children

In this section we present estimates of the impacts of children on the trajectory of a wide range of labor market outcomes for men and women. We start by showing impacts on earnings, labor supply and wage rates, and then turn to the underlying anatomy of these impacts by showing how occupation, firm, and sector choices by men and women respond to children.

### 3.2.1 Impacts on Earnings, Labor Supply, and Wage Rates

Figure 1 plots the gender-specific impacts of children  $P_t^m, P_t^w$  across event time. As defined above, these are outcomes at event time  $t$  relative to the year before the first child birth ( $t = -1$ ), having controlled non-parametrically for age and time trends. The figure includes 95% confidence bands around the event coefficients, although these are not always clearly visible due to the high precision of our data. Panel A starts by showing total earnings before taxes and transfers. We see that, once life-cycle and time trends are taken out, the earnings of men and women evolve in an almost parallel fashion until parenthood. But at the precise moment the first child arrives, the earnings paths of men and women diverge: women experience an immediate drop in gross earnings of almost 30%, while men experience no visible change in their earnings. In the years following the initial drop, the earnings of women never converge back to their original level. Ten years after the birth of a first child, female earnings have stabilized at around 20% below its level just before child birth, whereas male earnings are essentially unaffected by children. As shown in the figure, the child penalty in the earnings of women relative to men is equal to 19.4% after ten years.

These earnings impacts can come from three margins: hours worked, labor force participation, and the wage rate. Panels B-D of Figure 1 show that all three margins are in play. For each of these outcomes, the trajectories of men and women are almost exactly parallel prior to having children and then diverge sharply immediately after the arrival of the first child. The gaps that open up in labor supply and wage rates are driven entirely by negative impacts on women, while men are unaffected.<sup>7</sup> For all outcomes, the gender gaps are very stable from around event time 3 and women show no sign of labor market recovery 10 years after the first child. Interestingly, the estimated long-run penalties in hours, participation and wage rates are similar in magnitude, implying that these margins are roughly equally important for the earnings impacts.<sup>8</sup> While our primary goal in this paper is to understand gender inequality, we note that the event studies of working hours and participation contribute to the large literature on family labor supply and fertility by providing clean estimates of the labor supply implications of the first child.

Our baseline specification shows that the impact of children is persistent over a 10-year horizon,

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<sup>7</sup>Our measures of hours worked and wage rates are conditional on labor force participation. The estimated effects in Panels B and D therefore include any selection effects into participation. If, as traditional selection models would predict, workers are positively selected on wage rates, then the true magnitude of the negative impact of children on female wage rates would be *larger* than implied by our estimates.

<sup>8</sup>The child penalties in panels B-D of Figure 1 are unconditional penalties: when estimating the effect of parenthood on one particular margin, we are not controlling for the other two margins in the regression. This explains why the long-run penalties on the three margins do not sum up to the overall earnings penalty.

but it is of course interesting to study the impact over an even longer horizon. Hence, Figure 2 considers an event study horizon that includes 20 years after the birth of the first child. For this exercise, we expand from the balanced panel of parents who have their first child between 1985-2003 to an unbalanced panel of parents who have their first child at any time between 1970-2013. In this sample, we estimate cohort-specific event coefficients and show average coefficients for the birth cohorts 1985-2003. The figure shows how strikingly persistent the effects of children are. In fact, the earnings impact 20 years after child birth is almost the same as the impact 10 years after. The only qualitative difference that emerges in the very long run is that hours worked do eventually begin to converge, while at the same time wage rates keep diverging. The combination of the narrowing hours gap and the widening wage rate gap produces a constant earnings gap.

We provide a number of extensions and robustness checks in the online appendix. First, while our event study approach uses the birth of the first child, the evidence presented so far is based on the full population of parents, irrespective of the total number of children they end up having. This implies that the dynamic patterns include the effects of children born after the first one, and the estimated long-run impacts should be interpreted as capturing the total impact of all children. To explore the implications of multiple children, Figure A.III replicates the event study in earnings in subsamples that condition on total fertility (1, 2, 3 or 4 children). The impact of children is very sharp in all four family types and the long-run child penalty increases by 7-10 percentage points per child. The short-run impact of the first child is about the same across families with 1, 2 or 3 children (a gender gap of 25-30% at event times 0 and 1), and only gradually do the different family sizes diverge as more children arrive in the larger families. In families with 4 children, on the other hand, the impact is larger from the outset, suggesting that these families anticipate that they will have many children and divide gender roles immediately after the first one.<sup>9</sup>

Second, in the event graphs presented so far, the drop in earnings and labor supply in year 0 is not much larger than the drop in subsequent years. This may seem surprising given the extra time taken off for maternity leave immediately following delivery. However, note that the use of calendar-year measures of earnings and labor supply create attenuation bias in the initial dip: as women give birth during year 0, some of the earnings and hours in this year were realized prior to child birth. To investigate this point, Figure A.IV reproduces the event study on a sample restricted to January births, in which case calendar time and event time coincide. Here we do see a

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<sup>9</sup>Note that the event study graph for families with two children in Panel B of Figure A.III looks very similar to the graph for all families in Figure 1. This is natural given that the average completed fertility, conditional on having children, is close to two in Denmark.

pronounced dip in event year 0 as one would expect.<sup>10</sup>

Finally, the fact that we estimate percentage effects based on a level-specification (rather than a log-specification) implies that the estimates put more weight at the top of the distribution. If there is lots of heterogeneity in the impacts of children across the distribution, the mean impacts can be very different from the impacts further down the distribution. To address this concern, Figure A.V shows median impacts of children on earnings and total hours worked. These quantile regressions are based on a 1/7 subsample, which makes the confidence bands somewhat larger. The results in the figure show that the median impacts are roughly similar to mean impacts, ruling out the concern that that our results are representative mainly of the top of the distribution.

### 3.2.2 Impacts on Occupation, Sector, and Firm

We have seen that motherhood is associated with large and persistent effects on earnings driven in roughly equal proportions by participation, hours of work, and wage rates. These empirical patterns, and especially the large wage rate effects, beg the question of what are the underlying mechanisms? One possibility is that women, once they become mothers, make career choices in qualitative dimensions (occupations, sectors, firms, etc.) that favor family amenities over pecuniary rewards. The importance of such effects has been much discussed (see e.g., Goldin 2014), and there is plenty of cross-sectional evidence showing that women with children work in different occupations and industries than women without children or men. Still, we are not aware of any evidence that these qualitative dimensions directly respond to children. We provide such evidence in this section.

The results are presented in Figure 3, which is based on the same specification as above. Panel A considers occupational rank in five levels: unskilled labor, skilled labor, white-collar work (low level), white-collar work (high level), and top manager. This ordering of occupations is consistent with an ordering based on average earnings or average wage rates in each occupation. This panel shows that men and women are on identical trends in terms of their occupational rank prior to becoming parents, but that women start falling behind men soon after parenthood.<sup>11</sup> Note that the

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<sup>10</sup>Focusing on January births also reveal a small drop in labor market outcomes in event year -1, which can be explained by sick leave and maternity leave during pregnancy. In Denmark, women are eligible for maternity leave during the last four to eight weeks of pregnancy.

<sup>11</sup>Notice that the impact of children is identified from the sharp changes in outcomes immediately following child birth (for women relative to men) rather than from the smooth trends in outcomes. In Panel A there is a smooth downward trend in occupational rank for both men and women. Because we control for age and year fixed effects, these trends reflect that individuals who have children earlier in life tend to have lower occupational rank. This is a cross-sectional correlation in the data, not an effect of having kids.

occupation graphs for men and women begin to diverge in event year 1 rather than in event year 0. This is natural given that women are giving birth during year 0, so that this year consists partly of a pre-birth period and partly of a period covered by job-protected parental leave. Hence, women do not have a strong incentive to change occupation within year 0, but can wait until year 1 when they return to work.

Panel B also explores occupational rank, but focuses specifically on the probability of becoming manager. We see a clear and persistent drop in the female probability of being manager after parenthood, while there is no change in the male probability of being manager around parenthood. In the long run, the probability of rising to the manager level is reduced by 26% for women relative to men as a result of having children.

The bottom panels turn to the choice of work environment and in particular its family friendliness.<sup>12</sup> We first consider the link between children and the decision to work in the Danish public sector, which has a long tradition of focusing on working conditions rather than on wages. This includes flexible working hours, leave days when having sick children, and a favorable view on long parental leaves (see [Nielsen \*et al.\* 2004](#) for a detailed description). It is therefore natural to expect that mothers would be induced to move into the public sector, a hypothesis that is confirmed in Panel C. The probabilities of working in the public sector for men and women begin to diverge soon after having a child. As with occupation, the divergence starts in year 1 rather than in year 0, i.e. when women return to work after their parental leave. We estimate that, ten years after child birth, women are 12% more likely than men to work in the public sector as a result of parenthood.

Finally, Panel D considers a proxy for the family friendliness of a work environment that also encompasses heterogeneity across firms in the private sector. Specifically, we proxy family friendliness based on whether the firm's management includes women with young children (below 15 years of age). The advantage of this measure is that it can capture many aspects of an otherwise complex, multidimensional concept. Having women with young children in the management may reflect generous maternity leave schemes, tolerance towards sick days, flexible working hours, the possibility of working from home, etc. Because firms that have women with young children in the management tend to offer lower wage rates, if women move into such firms following motherhood, this helps explain the wage rate penalties documented above.

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<sup>12</sup>The specification underlying these panels departs from the baseline specification used so far. When considering sector and firm choices, there is a difference in pre-trends between men and women even after controlling for age and year dummies. As these (smooth) pre-trends distract from the breaks around parenthood, Panels C and D control for linear pre-trends. That is, we estimate a linear trend separately for men and women using only pre-event data, and then we run the main event study specification residualizing the outcome variable with the estimated pre-trend.

The figure provides strong evidence that the family friendliness of women’s work environment responds to motherhood. The male and female probabilities of working in a family-friendly firm are almost perfectly parallel until the arrival of the first child, but they start diverging shortly after child birth. This effect is driven by mothers *switching* firms as opposed to within-firm increases in family friendliness: we find no effect on family friendliness when conditioning on staying in the same firm over time. The long-run effect of children on the probability of working in a family-friendly firm for women relative to men is equal to 8%.<sup>13</sup>

### 3.2.3 Identification

Based on event studies around the birth of the first child, we have argued that women’s career trajectories (but not men’s) are causally affected by children in a range of quantitative and qualitative dimensions. Before moving to the next step of the analysis, we pause to consider the causal identification of these impacts. Specifically, while the identification of *short-run* effects in an event study design relies only on smoothness and is therefore relatively compelling, the identification of *long-run* effects requires stronger assumptions. Implicitly, our estimates of long-run child penalties are based on using men as controls for women. The presence of parallel pre-trends after controlling non-parametrically for lifecycle and time trends lends support to this assumption, but as we move further away from the event those pre-trends become less informative. Furthermore, it may be problematic to use men as a control group given that they are also treated by the event, even if the smoothness of male outcomes around child birth suggests that in practice they are unaffected.

In appendix B we lay out a conceptual framework for identification and provide two identification checks that we briefly discuss here. The first identification check is a difference-in-differences event study design in which we compare those who have children to those who never have children. This design is based on assigning placebo births to individuals who never have children, drawing from the observed distribution of age at first child among those who do have children. The technical details are described in appendix B.2 and the results are shown in Figure A.VI. Panel A shows earnings impacts for women, while Panel B shows earnings impacts for men. Women with and without children are on identical pre-trends, diverge sharply at the time of the first child birth,

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<sup>13</sup>While we have thus shown that switching to more family-friendly firms in the private or public sectors is one of the mechanisms driving the child penalty in earnings, a related question is whether being in a family-friendly firm prior to child birth serves to moderate the child penalty. In other words, what is the pattern of heterogeneity in penalties with respect to pre-parenthood firm and sector decisions? An earlier version of the paper included a detailed heterogeneity analysis, which showed that having a more family-friendly employer by the time of child birth is indeed associated with a significantly smaller penalty. This holds even after including rich controls for other factors that are correlated with firm and sector choices.

and the impact is very stable over time. The long-run impact of children equals 20.6%, slightly larger than the baseline impact of 19.4% shown in Figure 1A. A new insight that emerges from the DD event study is that that men *are* affected by parenthood. The effect is tiny (3% after 10 years), but the high precision of our data makes it very clear. The small effect on men implies that the earning impact on women relative to men — as captured by the child penalties obtained from the baseline specification — is slightly smaller than what we reported in Figure 1.

The second identification check compares our event study approach to an IV approach using the sex mix of the first two children as an instrument for having a third child (as first proposed by Angrist & Evans 1998). Given the sibling sex mix instrument can be used only to estimate the local average treatment effect of a third child, we compare it to an event study around the birth of a third child. As a result, this analysis does not provide a direct validation of our child penalty estimates, but rather an indirect validation of the event study approach. The details of the design are described in appendix B.3 and the results are presented in Figure A.VII. The conclusion from this analysis is that the event study estimates and the IV estimates are almost perfectly aligned, providing strong support for our empirical approach.

These robustness checks strongly suggest that the impacts of children can be causally identified based on within-person variation in an event study design. This is useful for future research in this area.

## 4 Decomposing Gender Inequality Over Time

### 4.1 Dynamic Decomposition Framework

In this section we decompose gender inequality into what can be attributed to children and what can be attributed to other factors, showing how this composition has evolved over time. We take a standard Oaxaca-Blinder decomposition approach, but innovate on existing gender gap decompositions by leveraging the event study variation around child birth. In particular, while standard decomposition approaches in the gender literature (see e.g., Blau & Kahn 2016) have been based on cross-sectional variation in education and labor market variables *not including children*, our decomposition approach focuses precisely on children and exploits within-person variation over time. The goals of these two decomposition exercises are different: while the traditional goal has been to estimate the wage gap between observationally equivalent males and females — the unexplained gap sometimes interpreted as “discrimination”, although it could also reflect children — our goal is



to delve into the unexplained part of traditional decomposition analysis by estimating the impact of children. The impact we estimate could operate through both the unexplained and explained parts of standard cross-sectional decomposition analyses, because those analyses include variables such as occupation, industry and experience, which represent some of the mechanisms responsible for the impact of children as we have seen above.

Two points are worth highlighting from the outset. First, provided that the child penalties are correctly identified (as argued above), our decomposition into child-related gender inequality and residual gender inequality should be viewed as causal rather than purely correlational. Second, since child penalties by construction capture only the post-effects of actual fertility and not the pre-effects of anticipated fertility, residual gender inequality includes the potential pre-effects of children. For example, education choices made prior to having children may reflect anticipated fertility.

We focus on gender inequality in earnings. To capture changes over time in the impact of children on gender inequality, we extend the baseline specification (1) to allow for year-specific coefficients on event time. Specifically we consider the following specification:

$$Y_{ist}^g = \sum_y \sum_{j \neq -1} \alpha_{yj}^g \cdot \mathbf{I}[j = t] \cdot \mathbf{I}[y = s] + \sum_k \beta_k^g X_{kis}^g + \nu_{ist}^g, \quad (3)$$

where we interact the event time dummies with year dummies in order to estimate year-specific event coefficients  $\alpha_{yj}^g$ . Note that estimating event coefficients by calendar year  $s$  and event year  $t$  amounts to estimating event coefficients by birth cohort  $c = s - t$ . The second term on the right-hand side includes covariates indexed by  $k$  that may vary across individuals  $i$  and calendar time  $s$ . As in our baseline specification (1), we begin by including a full set of age dummies and year dummies in the set of covariates, but we will also consider an extended specification that includes a rich set of education dummies. Importantly, the  $X$ s should not include any earnings determinants that directly respond to the event of child birth as such covariates would bias the estimated event coefficients. That is, while it may be legitimate to control for education choices made prior to having children, controlling for factors such as occupation and firm choices (which we have seen respond to child birth) would lead to bias.

Defining the mean gender gap in year  $s$  as  $\Delta_s \equiv \{E[Y_{ist}^m|s] - E[Y_{ist}^w|s]\} / E[Y_{ist}^m|s]$  and using

specification (3), we can rearrange terms so as to obtain

$$\hat{\Delta}_s = \underbrace{\frac{\mathbb{E}[P_{st}\tilde{Y}_{ist}^w|s]}{\mathbb{E}[\hat{Y}_{ist}^m|s]}}_{\text{child penalties}} + \underbrace{\frac{\sum_k(\hat{\beta}_k^m - \hat{\beta}_k^w)\mathbb{E}[X_{kis}^m|s]}{\mathbb{E}[\hat{Y}_{ist}^m|s]}}_{\text{different returns to } X\text{s}} + \underbrace{\frac{\sum_k\hat{\beta}_k^w\{\mathbb{E}[X_{kis}^m|s] - \mathbb{E}[X_{kis}^w|s]\}}{\mathbb{E}[\hat{Y}_{ist}^m|s]}}_{\text{differences in } X\text{s}} \quad (4)$$

where  $P_{st} \equiv \frac{\hat{\alpha}_{st}^m - \hat{\alpha}_{st}^w}{\mathbb{E}[\hat{Y}_{ist}^w|s,t]}$  is the child penalty at event time  $t$  in calendar year  $s$ ,  $\tilde{Y}_{ist}^w$  is the predicted counterfactual earnings (i.e., absent children) for women, and  $\hat{Y}_{ist}^m$  is the predicted actual earnings for men. The first term on the right-hand side captures the impact of child penalties on gender inequality, the second term captures the impact of different coefficients on non-child covariates (such as different returns to education), while the last term captures the impact of differences in non-child covariates (such as different levels of education). In the standard language of decomposition analysis (see e.g., Fortin *et al.* 2011), the first two terms represent “unexplained” effects (different regression coefficients) while the last term represents “explained effects” (different observables).<sup>14</sup>

To decompose gender inequality over the full period 1980-2013, we expand from the previously balanced panel of parents who have their first child between 1985-2003 to the full population of parents who have their first child between 1970-2013. For first child births after 1980, we observe parents for at least one year prior to child birth and are therefore able to estimate child penalties relative to event time -1 as we have done so far. Moreover, because we keep parents in the sample for the longest possible number of years (for example, parents who have their first child in 1981 are observed until event time  $t = 32$ , conditional on still being alive and residing in Denmark), we are able to estimate child penalties at all event times for the post-1980 birth cohorts. On the other hand, for first child births up until 1980, we observe only post-event years and are therefore unable to directly estimate child penalties associated with these births. For these birth cohorts we therefore rely on an extrapolation of the post-1980 penalties that we describe below. The reason we include the earlier cohorts in the estimation sample when running specification (3) is that they help estimating the effect of the non-child covariates (such as education).

In Figure 4 we show earnings penalties by birth cohort obtained from specification (3). Panel A focuses on short-run penalties (event times 0-10) and Panel B focuses on long-run penalties (event

<sup>14</sup>For simplification purposes, equation (3) does not include an explained effect of differences in children between men and women. We leave out this term in the equation (but not in the analysis), because it is always very close to zero. In fact, in a balanced panel of men and women who have children together, the fractions of men and women at each event time in each calendar year are by construction the same. As we describe below, our decomposition analysis is based on an unbalanced panel, and so the explained effect of children will be non-zero resulting from differential attrition of men and women due to deaths and migration. However, the explained effect of children due to such differential attrition is in practice tiny.

times 11-20). Each panel includes a linear OLS fit in order to highlight the trend. We see that there is no trend in the short-run penalties, but a linear downward trend in the long-run penalties. We use these estimated trends to extrapolate child penalties to earlier birth cohorts: penalties between event times 0-10 are assumed to be constant at the average level of the later cohorts, while penalties between event times 11-20 are assumed to follow the linear trend estimated for the later cohorts.<sup>15</sup> With these extrapolations we obtain estimates of the child penalty  $P_{st}$  for every event time and every year between 1980-2013, allowing us to decompose gender inequality into child-related inequality and non-child inequality over the full period.

## 4.2 Decomposition Results

The results of our decomposition analysis are shown in Figure 5. Panel A is based on the specification without education controls, and it shows child-related gender inequality in blue and residual gender inequality in grey. We see that the fraction of gender inequality that can be attributed to children has increased dramatically over time, from about 40% in 1980 to about 80% in 2013. This increase reflects a combination of two underlying changes: (i) child-related gender inequality in earnings has increased from 18% to almost 20%, and (ii) total gender inequality in earnings has fallen from 46% to 24%. To understand the first effect, note that although the *percentage* child penalty has fallen slightly over time, the penalty operates on a larger base due to the general increase in the earnings of women relative to men coming from the second effect. As shown in equation (4), the impact of children on gender inequality depends both on the size of child penalties and on the counterfactual earnings of women relative to men. Hence, during a period where non-child gender inequality is falling and child penalties are constant or falling by less, there will be a tendency for child-related gender inequality to go up.<sup>16,17</sup>

<sup>15</sup>For event times beyond 20 for the earlier cohorts, we assume that the penalty stays constant at its level in event time 20. Such a steady state assumption can be justified by the results presented in Figure 2A, which showed that earnings penalties are quite stable from around event time 10.

<sup>16</sup>Besides these long-run changes in the composition of gender inequality, Figure 5 shows a short-run business cycle effect: during recessions (early 1980s, early 1990s, and 2008-09) overall gender inequality falls, but child-related inequality does not, and so the fraction of gender inequality due to children increases. In fact, during the global financial crisis in 2008-09 we estimate that child-related gender inequality was more than 90% of total inequality.

<sup>17</sup>To understand how the increasing importance of children plays out over the life-cycle, Figure A.VIII decomposes the age profiles of gender inequality in earnings for two specific years, 1985 and 2013. We see that there is relatively little earnings inequality in the tails, before having children and around the age of retirement. The largest earnings inequality occurs among those aged 30-55, around the time where most families have children living at home, and this is also where we estimate the impact of children — the difference between the dashed grey and solid grey lines in the figure — to be largest. While in 1985 there is a large difference between male earnings and counterfactual female earnings (i.e. absent children) throughout the life-cycle, in 2013 there is only a small difference between male earnings and counterfactual female earnings throughout the life-cycle.

Our findings imply that, to a first approximation, the gender inequality that remains today is all about children. The fact that our approach misses the potential pre-effects of anticipated fertility only reinforces the conclusion that gender inequality is *now* all about children, but it could change the conclusion that this was not the case 30-40 years ago. It is conceivable that, while the impact of child penalties (post-effects) has increased over time, the importance of pre-effects has fallen. Indeed, it would be natural if women invested less in education and careers in anticipation of motherhood in a more traditional era. In other words, the pattern documented in Panel A may reflect a gradual shift away from pre-effects towards post-effects of children.

To provide suggestive evidence on this idea, we include education choices made prior to child birth in the specification. Specifically, we include dummies for primary school, secondary school, vocational training, short post-secondary school, bachelor's degree, and master's/phd degrees. The results are presented Panel B, which shows child-related gender inequality in blue and education-related gender inequality in orange. The education-related part includes both the effect of different education levels (explained effect) and the effect of different education coefficients/returns (unexplained effect), with the latter being quantitatively more important. The explained effect of education is small from the start and turns negative in the early 2000s as women overtake men in terms of education levels. This is consistent with the disappearance of the college gap in the US (Goldin *et al.* 2006) and in most other high- and middle-income countries (Kleven & Landais 2017). As for the unexplained effect of education, this absorbs the effect of men and women choosing different education fields, conditional on level. In particular, women tend to choose "softer" fields than men (such as health care and teaching as opposed to construction and engineering) that are not as highly remunerated, but may offer better family amenities.

Two main insights emerge from Panel B. First, the inclusion of education controls has only a small impact on the estimation of child-related gender inequality. It is still the case that child-related inequality is close to 80% of total inequality at the end of the period. The robustness of the decomposition analysis to education controls results from the event study variation we use: the child effect is identified from within-person time variation around child birth, while the education effect is obtained from cross-sectional variation and therefore does not absorb the child effect. Of course, the fact that the education effect is based on cross-sectional variation implies that, unlike the child effect, it is only correlational. Second, while the child-related gender gap has been growing over time, the education-related gender gap has been shrinking dramatically. Education-related inequality was almost as large as child-related inequality at the beginning of the period, but

has almost disappeared over time. Our findings are consistent with a secular shift in the relative importance of post-effects of children (child penalties) and pre-effects of children (as proxied by education choices).

In this section we have tried to re-orient the traditional focus of gender gap decompositions by considering the impact of children and by exploiting sharp time variation instead of cross-sectional variation. Rather than studying the extent to which men and women receive unequal pay for equal work (the unexplained gap after controlling for human capital and job characteristics), we show that men and women receive unequal pay largely because of the unequal distribution of child care responsibilities. Even with perfectly equal pay for equal work, there would still be large gender inequality in earnings as equal work is not an option for the majority of women who are faced with the lion's share of child care responsibilities.

## 5 Intergenerational Transmission of Child Penalties

### 5.1 Background

Why are female child penalties so large and persistent? Traditional economic explanations appeal to comparative advantage in infant child care and the gains from specialization. However, the fact that the unequal effects of children persist over the entire career path of parents — and considering that women are now more educated than men on average — suggests that there is more going on than just comparative advantage.<sup>18</sup> A potential explanation is the presence of gendered preferences or norms regarding the appropriate roles of women and men who have children. Indeed, we saw in Figure A.II that the views on gender roles in families with children remain conservative in all countries. This raises the question of where these gendered preferences are coming from? In this section we present evidence that child penalties are transmitted through generations — from parents to their daughters — consistent with an influence of nurture in the formation of female preferences over family and career.

Our analysis relates to the literature studying the importance of gender identity norms in the labor market, as reviewed by [Bertrand \(2011\)](#). Many have argued that gender identity is formed during childhood, and some papers have documented the existence of intergenerational correla-

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<sup>18</sup>In fact, [Kleven \*et al.\* \(2018\)](#) show that there is essentially no heterogeneity in child penalties with respect to the relative education levels of the parents: women at the top of the relative education distribution incur about the same penalty as women at the bottom of the distribution. This is not the empirical pattern one would expect if the comparative advantage channel were very strong.

tions in gender identity norms and female labor supply. [Fernandez et al. \(2004\)](#) find that the labor force participation of married women is positively correlated with the labor force participation of their husbands' mothers, but *not* with the labor force participation of their own mothers, after controlling for various socio-economic characteristics. Their interpretation is that men growing up with working mothers develop more modern gender role attitudes and therefore have stronger preferences for working wives. Related, [Farre & Vella \(2013\)](#) show that mothers' gender role attitudes are correlated with their children's attitudes and labor force participation. They also find such correlations between mothers and daughters-in-law, similar to [Fernandez et al. \(2004\)](#).

Our analysis diverges from previous studies in two respects. First, we consider the intergenerational transmission of child penalties — i.e., labor supply changes around child birth for women relative to men — rather than the intergenerational transmission of labor supply levels. Working with child penalties takes care of some of the omitted variable concerns encountered when working with labor supply levels. Our analysis relies on our ability to link three generations — children, their parents, and their maternal and paternal grandparents — in the Danish administrative data. Second, we demonstrate the existence of a link between child penalties and the maternal grandparents, but not the paternal grandparents, in contrast to the earlier findings discussed above. Our findings are consistent with the idea that women's preferences over family and career is shaped by the gender roles she is exposed to during her childhood.

## 5.2 Specification

The analysis is based on our baseline event study sample of men and women who have their first child between 1985-2003 and are observed in a 15-year window around the first child birth. To link the child penalties for these parents to the past labor market behavior of the grandparents, we exploit that our administrative ATP measure of hours worked goes back to 1964. This allows us to investigate the relationship between child penalties for the 1985-2003 births and the relative labor supply of grandmothers and grandfathers during 1964-1979, distinguishing between maternal and paternal grandparents.

Denoting the cumulated labor supplies between 1964-1979 of the maternal grandmother and grandfather by  $h_i^{mm}$  and  $h_i^{mf}$ , we rank parents by quantiles of the distribution of  $h_i^{mm} - h_i^{mf}$ . For the paternal grandparents, we similarly rank parents by quantiles of the distribution of  $h_i^{pm} - h_i^{pf}$ . A higher rank in these distributions implies that the grandparents were more “modern” in terms of their gender division of labor.

We estimate how child penalties vary by these grandparental rank measures. Because here we are not primarily interested in the exact dynamic path of child penalties, we adopt a more parsimonious specification that replaces the full set of event time dummies for  $t = -5, \dots, 10$  by a single dummy for being at positive event times  $t \geq 0$ . In other words, we are considering average child penalties over the 10-year period following child birth. To estimate the effect of the maternal grandparents, we consider specifications of the following type:

$$\begin{aligned}
Y_{ist}^g &= \sum_q \alpha_q^g \cdot \mathbf{I}[\text{after}_t] \cdot \mathbf{I}[\text{grand}_{iq}^m] + \sum_k \beta_k^g \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \gamma_y^g \cdot \mathbf{I}[y = s] \\
&+ \sum_q \zeta_q^g \cdot \mathbf{I}[\text{grand}_{iq}^m] + \delta^g \cdot X_i^m + \eta^g \cdot \mathbf{I}[\text{after}_t] \cdot X_i^m + \nu_{ist}^g,
\end{aligned} \tag{5}$$

where  $\mathbf{I}[\text{after}_t]$  is an indicator for  $t \geq 0$ ,  $\mathbf{I}[\text{grand}_{iq}^m]$  is an indicator for the maternal grandparents being in quantile  $q$  of the distribution of relative labor supply, and  $X_i^m$  is a vector of controls for the maternal grandparents that we describe below. In the richest specification we allow the impacts of these controls to vary before and after child birth. As before, we run the specification in levels and calculate the percentage impact of children on women as  $P_q^w \equiv \hat{\alpha}_q^w / E[\tilde{Y}_{is}^w]$  in the  $q$ th quantile of the relative labor supply distribution of the maternal grandparents.<sup>19</sup> The specification for the paternal grandparents is the same, only replacing  $m$  by  $p$ .

To ensure that the intergenerational correlation in child penalties is not just a transmission of other variables that are correlated with child penalties, we include the following controls for the characteristics of the grandparents (maternal and paternal, respectively). First, we include education dummies for both the grandmother and the grandfather capturing the length and the field of education. The length of education is divided into primary school, secondary school, vocational training, short post-secondary school, bachelor's degree, and master's/phd degrees. Above secondary school, each level is divided into different fields such that we end up with 22 education dummies for each of the grandparents. These controls ensure that the intergenerational correlation in child penalties does not reflect a transmission of educational preferences or ability. Second, we control for the wealth level of the grandparents. We use the average net wealth of the grandfather over the years 1980-90 and control for quantiles of the within-cohort wealth rank of the grandfather.<sup>20</sup> This ensures that the child penalties are not driven by wealth effects that are transmitted

<sup>19</sup>Our analysis of intergenerational correlations focuses on the impact of children on women,  $P_q^w$ . We do not find any intergenerational correlations when considering the impact on men,  $P_q^m$ .

<sup>20</sup>This administrative wealth measure is available for the universe of Danish taxpayers as it was collected for the purpose of a wealth tax that existed until 1996.

through generations. Finally, we include dummies for the birth cohort of both the grandmother and the grandfather, as well as dummies for the region of residence of the grandparents.

### 5.3 Results

Our first results are presented in Figure 6. This figure plots the child penalty in earnings against quintiles of the relative labor supply distribution of the maternal grandparents (Panel A) and the paternal grandparents (Panel B). In this figure we are not including the grandparental controls described above (education levels, education fields, wealth levels, and region of residence).<sup>21</sup>

Panel A of the figure shows a clear downward-sloping relationship between the female child penalty and the relative labor supply of the maternal grandmother. That is, women incur smaller earnings penalties from children if they themselves grew up in a family where the mother worked more relative to the father. The effect of going from the bottom quintile to the top quintile of the distribution is quite large, 6 percentage points or almost one-third of the total penalty. In contrast, Panel B of the figure shows no effect of the paternal grandparents: the relationship between the female child penalty and the relative labor supply of the paternal grandparents is essentially flat, except for a small effect at the very top. The differential pattern between maternal and paternal grandparents suggests that child penalties are driven partly by female preferences formed during her childhood, rather than by male preferences formed during his childhood.

The obvious concern with interpreting these correlations is that they may reflect heterogeneity in other correlated dimensions. What appears to be a transmission of labor supply behavior from parents to daughters may be a transmission of other variables that are correlated with labor supply. To investigate this concern, Figure 7 shows intergenerational correlations when we control for other factors that are transmitted across generations and impact labor supply: dummies for the education level and field of each grandparent, dummies for the wealth quantile of the grandparents, and dummies for the region and cohort of the grandparents. In the figure, we allow the wealth, region and cohort variables — but not the education variables — to have differential effects before and after child birth. We see that the results are hardly affected by these controls. If anything, the intergenerational transmission from the maternal grandparents is slightly stronger in this specification. The downward-sloping relationship is smoother and the total effect is marginally larger.

In Figure A.IX in the online appendix, we extend the specification to also allow the education

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<sup>21</sup>However, we do include cohort-dummies for the grandparents as it seems more reasonable to consider the impact of grandparental labor supplies conditional on the norm for their cohort.



dummies to have differential impacts before and after the arrival of children. This specification is very rich, but its interpretation is unclear. Specifically, in the previous section we argued that there has been secular trend from pre-effects of children (anticipatory effects reflected in education choices) to post-effects of children (child penalties). As a result, the education choices of the grandparents may well proxy for the child penalties in their generation. In this case, by interacting the education choices of the grandparents with the after-child dummy of the next generation, we are most likely absorbing some the effect we are interested in. Consistent with this, Figure A.IX shows that such controls do reduce the intergenerational correlations quite substantially. There is still an effect from the maternal grandparents (combined with a zero effect from the paternal grandparents), but the magnitude is only half as large as in the specifications considered above.

The transmission of child penalties between generations can occur through two channels: a transmission of fertility preferences or a transmission of penalties conditional on fertility. That is, when women from “traditional” families incur larger child penalties, it may be because they have more children or it may be because they pay larger career costs for a given number of children. Figure A.X in appendix explores this question by adding fertility dummies for the parents among the controls in equation (5). The figure shows that neutralizing the fertility channel has no impact on the results. This implies that the transmission of child penalties does not occur through a transmission of fertility preferences, but through family-career preferences for a given number of children.

To conclude, the child penalties on women are strongly correlated with the division of labor in their own childhood homes, but not with the division of labor in their spouses’ childhood homes. That is, women who grew up in relatively traditional homes incur larger child penalties than women who grew up in relatively modern homes. The fact that these correlations survive the inclusion of rich non-parametric controls for the main confounders (such as education and wealth) suggests that they do indeed reflect a transmission of labor supply behavior as opposed to a transmission of other correlated factors.<sup>22</sup> Our findings are consistent with an influence of nurture in the formation of female preferences over children and career.

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<sup>22</sup>Table A.I in the online appendix provides further evidence on the robustness of these intergenerational correlations to different controls for the characteristics of the grandparents and parents. The table summarizes the results from the specifications we have presented graphically, and it shows estimates from alternative specifications.

## 6 Conclusion

Despite considerable gender convergence over time, substantial gender inequality persists in all countries. Using full-population administrative data from Denmark and a quasi-experimental event study approach, we show that most of the remaining gender inequality can be attributed to the dynamic effects of children. We have presented three main sets of results.

First, we have shown that the impact of children on women is large and persistent across a wide range of labor market outcomes, while at the same time men are virtually unaffected. The female child penalty in earnings is close to 20% in the long run. Underlying this earnings penalty, we find sharp impacts of children on labor force participation, hours worked, wage rates, occupation, sector, and firm choices. Together, these findings provide a quite complete picture of the behavioral margins that adjust in response to parenthood and how strongly gendered these margins are.

Second, we have decomposed gender inequality into what can be attributed to children and what can be attributed to other factors. We have shown that the fraction of child-related gender inequality has increased dramatically over time, from around 40% in 1980 to around 80% in 2013. Therefore, to a first approximation, the remaining gender inequality is all about children. Our decomposition analysis represents a re-orientation of traditional gender gap decompositions: instead of studying the extent to which men and women receive unequal pay for equal work (the unexplained gap after controlling for human capital and job characteristics, but not children), we study the extent to which they receive unequal pay as a result of children (but not necessarily for equal work). The unexplained gap in traditional decomposition analyses is often labeled “discrimination”, but our analysis highlights that the unexplained gap is largely due to children. This does not rule out discrimination, but implies that potential discrimination operates through the impacts of children.

Third, we have provided evidence in favor of environmental influences in the formation of preferences over family vs career. In particular, we have shown that the female child penalty is strongly related to the work history of the maternal grandparents: women who grow up in traditional families with a male breadwinner and a female homemaker incur larger child penalties when they themselves become mothers. At the same time, the female child penalty is unrelated to the work history of the paternal grandparents. Overall, these findings are consistent with the notion that child penalties are influenced by female gender identity formed during her childhood, as opposed to child penalties being driven by male gender identity formed during his childhood.

Our paper is agnostic about the potential welfare and policy implications of our findings. Al-

though the term “child penalty” may have normative connotations, we do not draw any normative conclusions here. The previous gender literature focusing on the unexplained gender gap had a very natural normative benchmark: equal pay for equal jobs. Our paper highlights that unequal pay is due to children, which may be good or bad depending on the perspective. A traditional economic view would focus on comparative advantage in child rearing (due to innate gender differences in abilities or preferences for child care vs market work) along with gains from specialization, in which case our findings do not necessarily call for policy intervention. Another view is that the unequal effects of children are driven by environmental factors such as culture, social norms or discrimination, producing potential inequities and inefficiencies. Our findings on intergenerational transmission are consistent with — but do not conclusively prove — the existence of such environmental factors. Future work should dig deeper into the underlying mechanisms and the implied welfare implications.

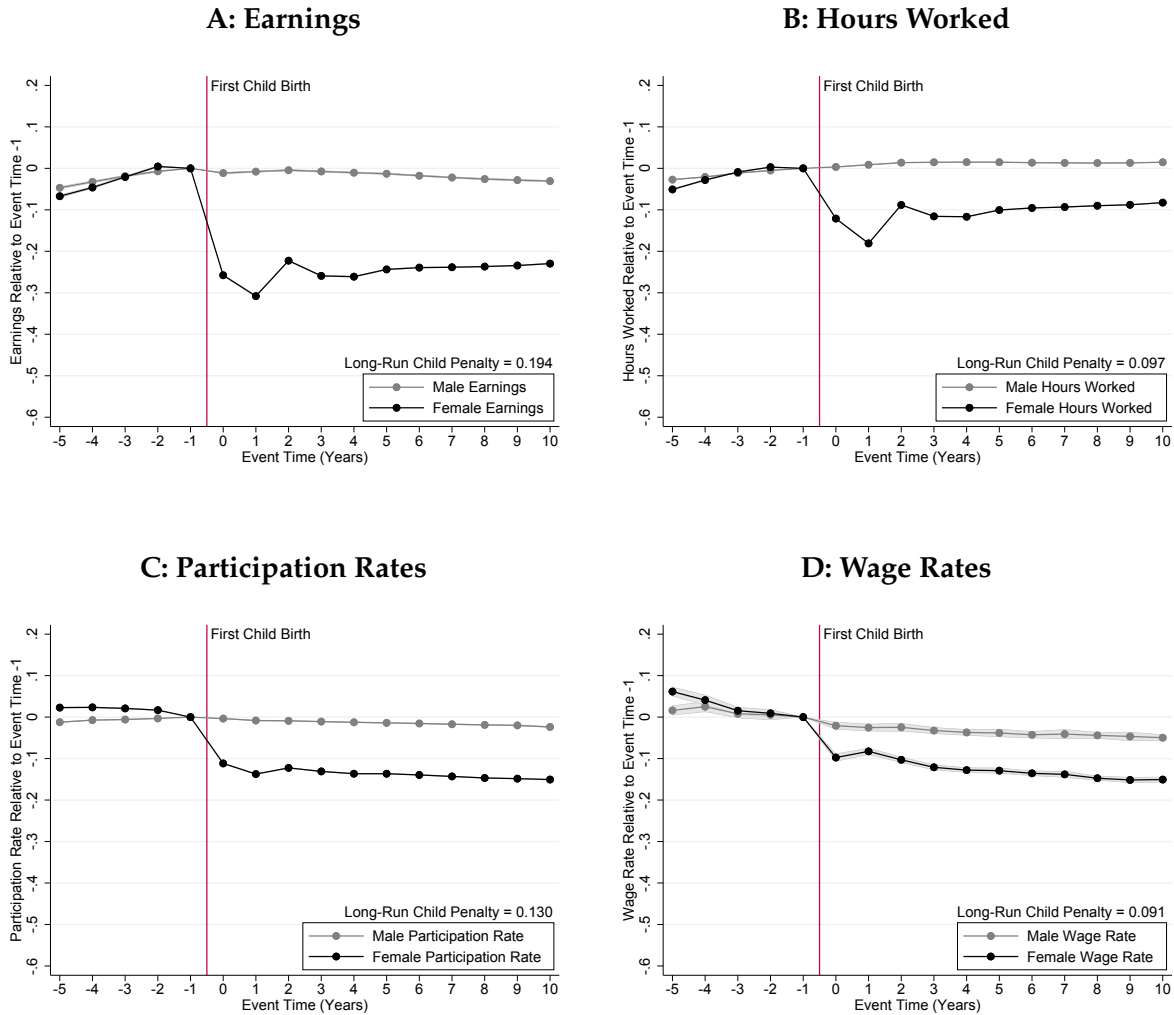
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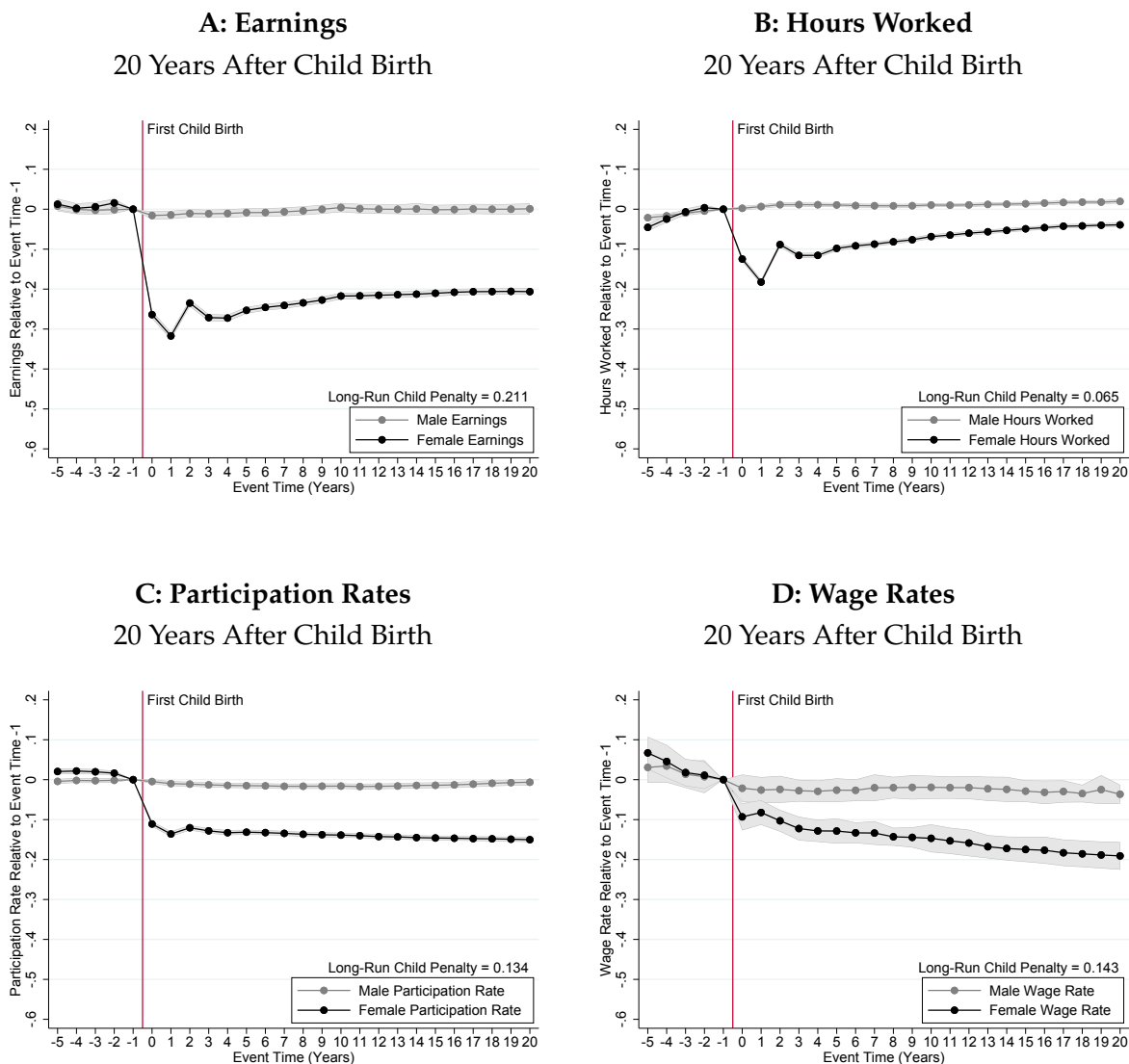
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Figure 1: Impacts of Children



Notes: The graphs show event time coefficients estimated from equation (1) as a percentage of the counterfactual outcome absent children (i.e.,  $P_t^g \equiv \hat{\alpha}_t^g / E[\tilde{Y}_{ist}^g | t]$  as defined in section 3.1) for men and women separately and for different outcomes. Each panel also reports a “child penalty” — the percentage by which women are falling behind men due to children — defined as  $P_t \equiv (\hat{\alpha}_t^m - \hat{\alpha}_t^w) / E[\tilde{Y}_{ist}^w | t]$ . The long-run child penalty is measured at event time 10. All of these statistics are estimated on a balanced sample of parents, who have their first child between 1985-2003 and who are observed in the data during the entire period between 5 years before and 10 years after child birth. The effects on earnings and participation are estimated unconditional on employment status, while the effects on hours worked and wage rates are estimated conditional on participation. The shaded 95 % confidence intervals are based on robust standard errors.

Figure 2: Impacts of Children in the Very Long Run

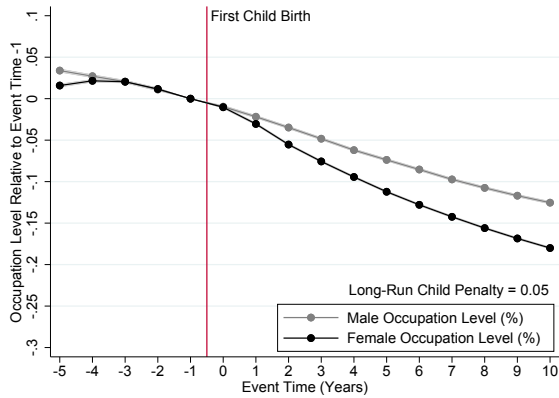


Notes: This figure is constructed in the same way as Figure 1, but expanding the event time window to include 20 years after the birth of the first child. In order to do this, we expand from the previously balanced panel of parents who have their first child between 1985-2003 to an unbalanced panel of parents who have their first child at any time after 1970, and who are observed in the data for the maximum years allowed by our data window. On this sample we estimate cohort-specific event dummies similar to the specification in equation (3) below and compute  $P_t^g \equiv \hat{\alpha}_t^g / E[\tilde{Y}_{ist}^g | t]$  similar to figure 1 using the average estimated event coefficients for the birth cohorts 1985-2003. The long-run child penalty is measured at event time 20. The shaded 95 % confidence intervals are based on robust standard errors.

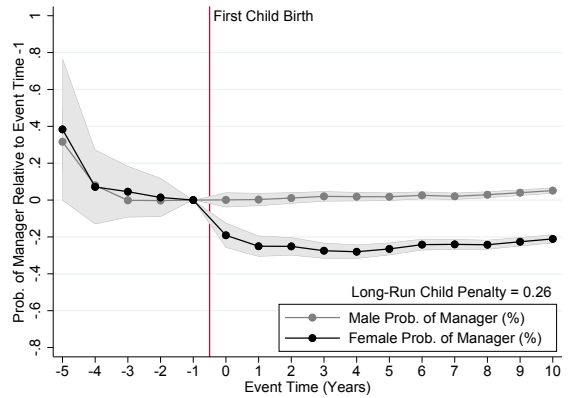


Figure 3: Anatomy of Child Impacts

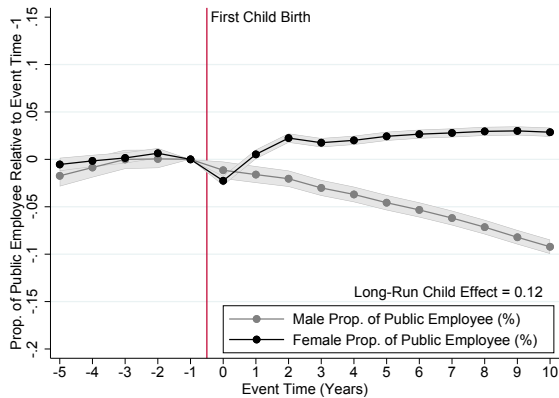
**A: Occupational Rank**  
Levels 1-5 from Unskilled Labor to Manager



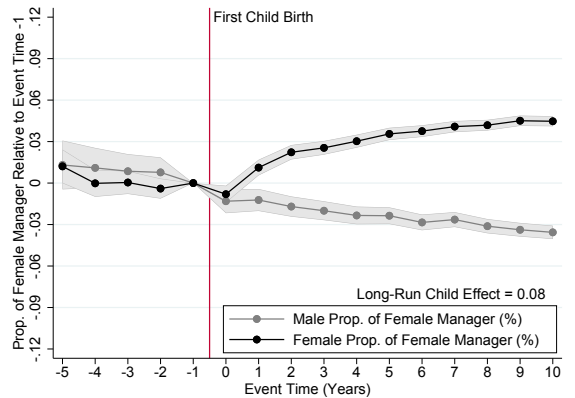
**B: Probability of Being Manager**  
Manager Dummy



**C: Probability of Public Sector Job**  
Public Sector Dummy



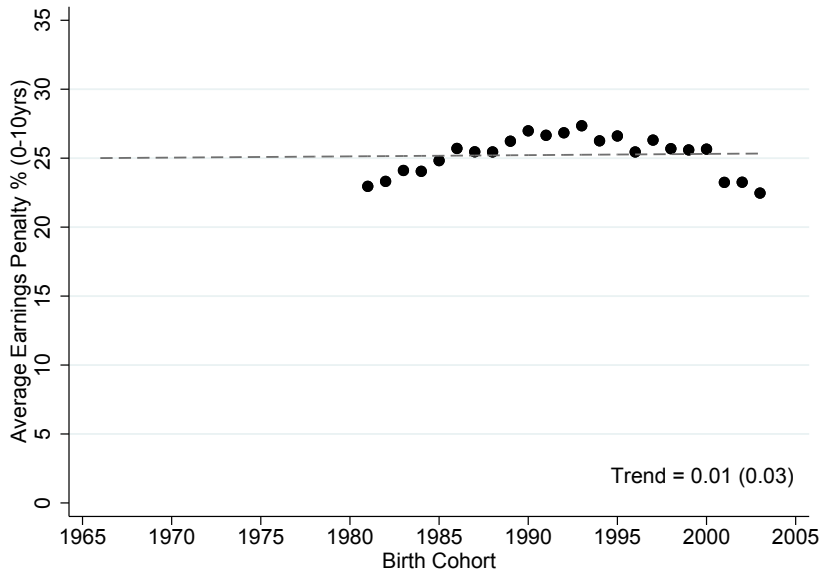
**D: Probability of Having a Female Manager with Children**  
Female Manager with Children Dummy



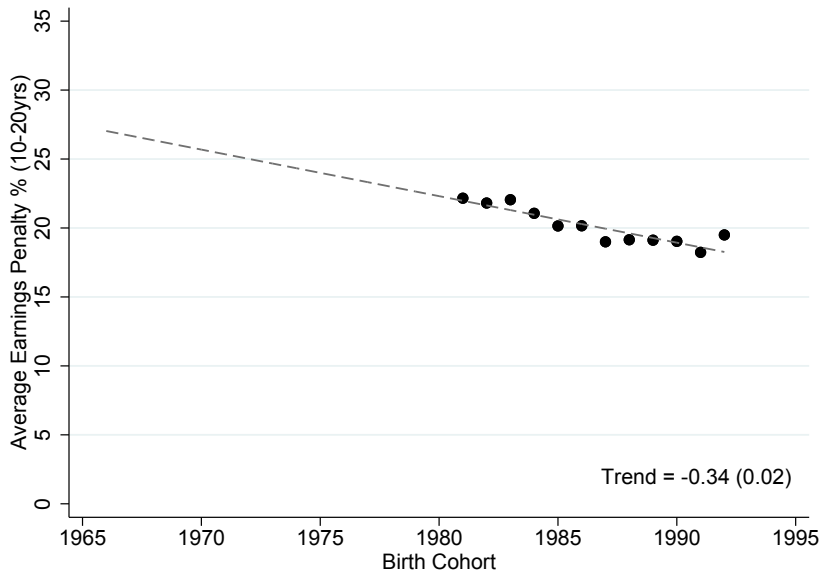
Notes: The figure shows impacts of children on men and women  $P_t^m$ ,  $P_t^w$  as well as the long-run child penalty  $P_t$  (at event time 10) as defined in Section 3.1. The effects on occupational rank are estimated conditional on not being self-employed or an assisting spouse. The effects on the probability of having a female manager with children below 15 is estimated conditional on being in a firm with at least 10 employees. The figures in panel C and D takes into account differential pre-trends between men and women. The shaded 95 % confidence intervals are based on robust standard errors.

Figure 4: Earnings Penalties by Birth Cohort and Extrapolation

**A: Average Earnings Penalty From Event Time 0-10**  
Zero Trend Across Cohorts



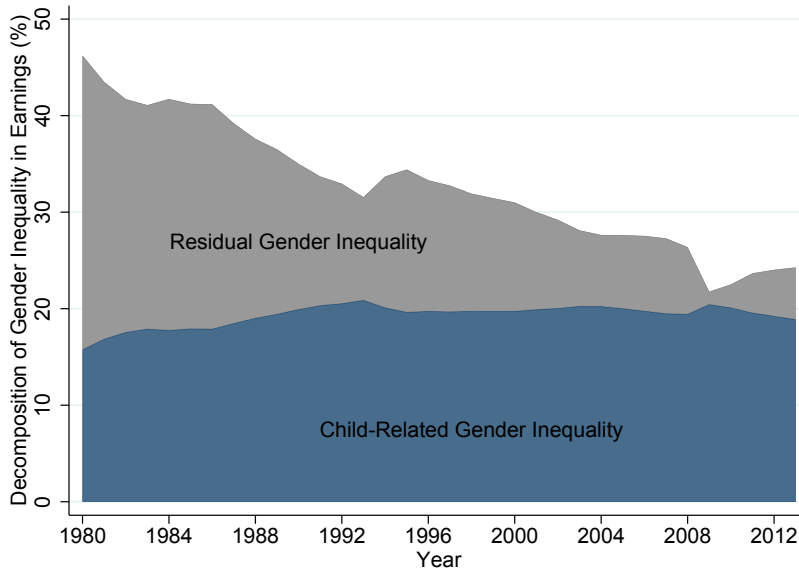
**B: Average Earnings Penalty From Event Time 11-20**  
Downward Trend Across Cohorts



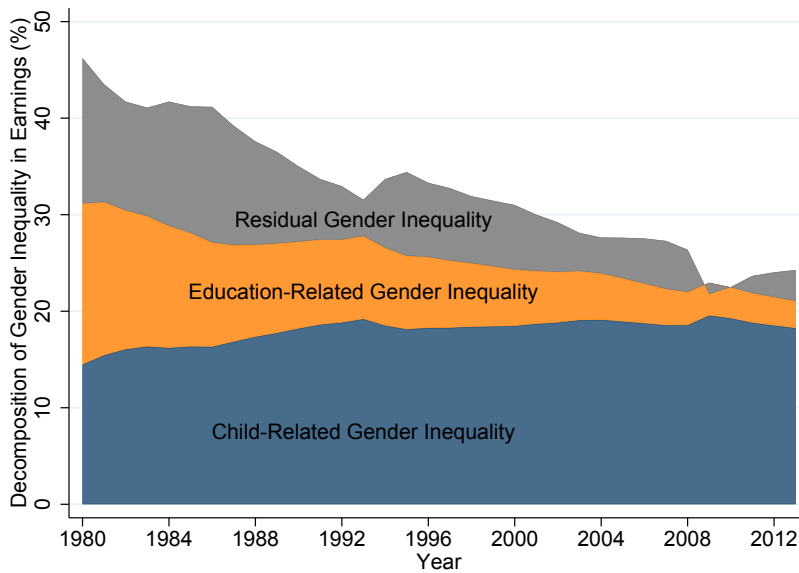
Notes: The figure shows earnings penalties by birth cohort obtained from (3). Panel A shows the average penalty across event times 0-10, while Panel B shows the average penalty across event times 11-20. Each panel also includes a linear OLS fit. There is a zero trend in the 0-10 year penalty, while there is a linear downward trend in the 11-20 year penalty. We use these linear trends to extrapolate child penalties to birth cohorts prior to 1981.

Figure 5: Decomposing Gender Inequality in Earnings

**A: Child-Related Inequality vs Non-Child Inequality**

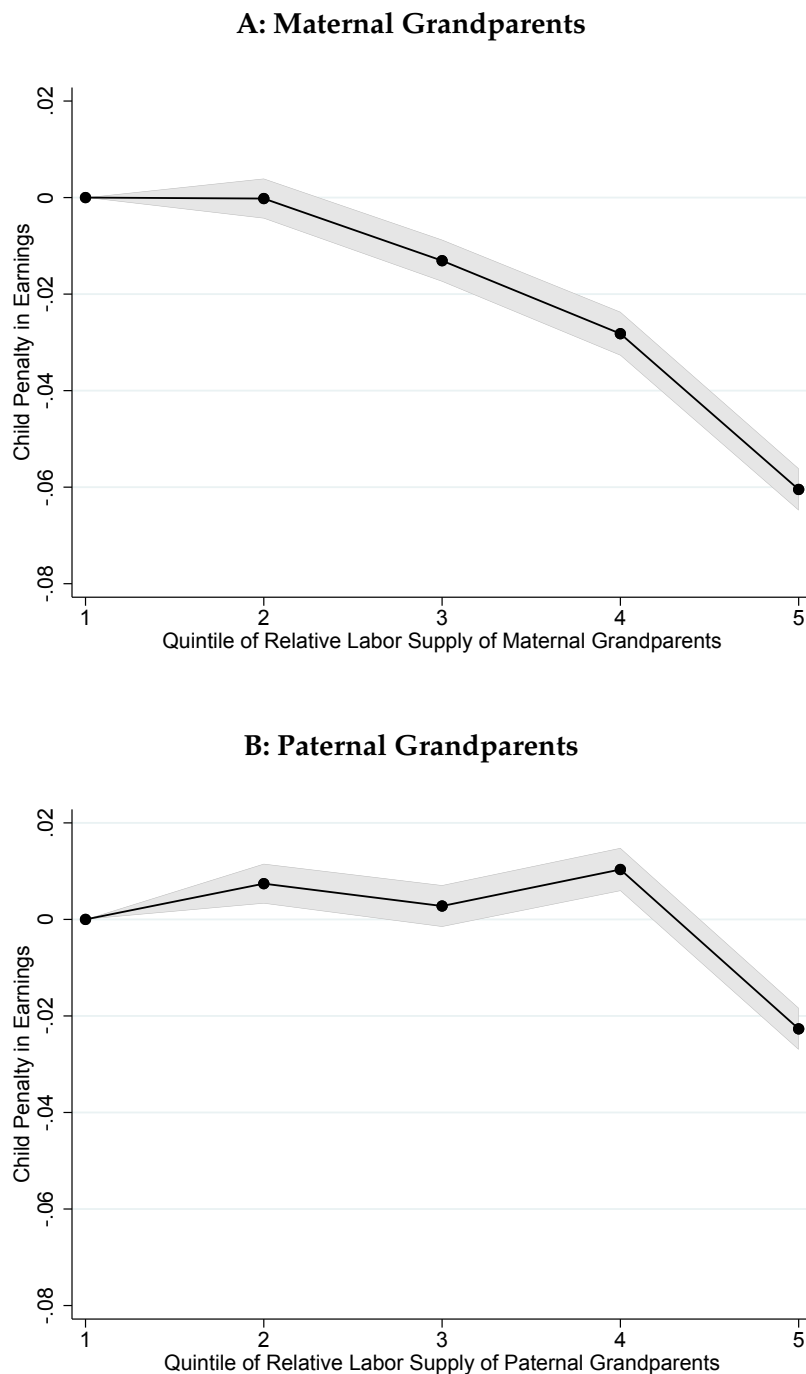


**B: Child-Related Inequality vs Education-Related Inequality**  
(Post-Child Effects vs Pre-Child Effects)



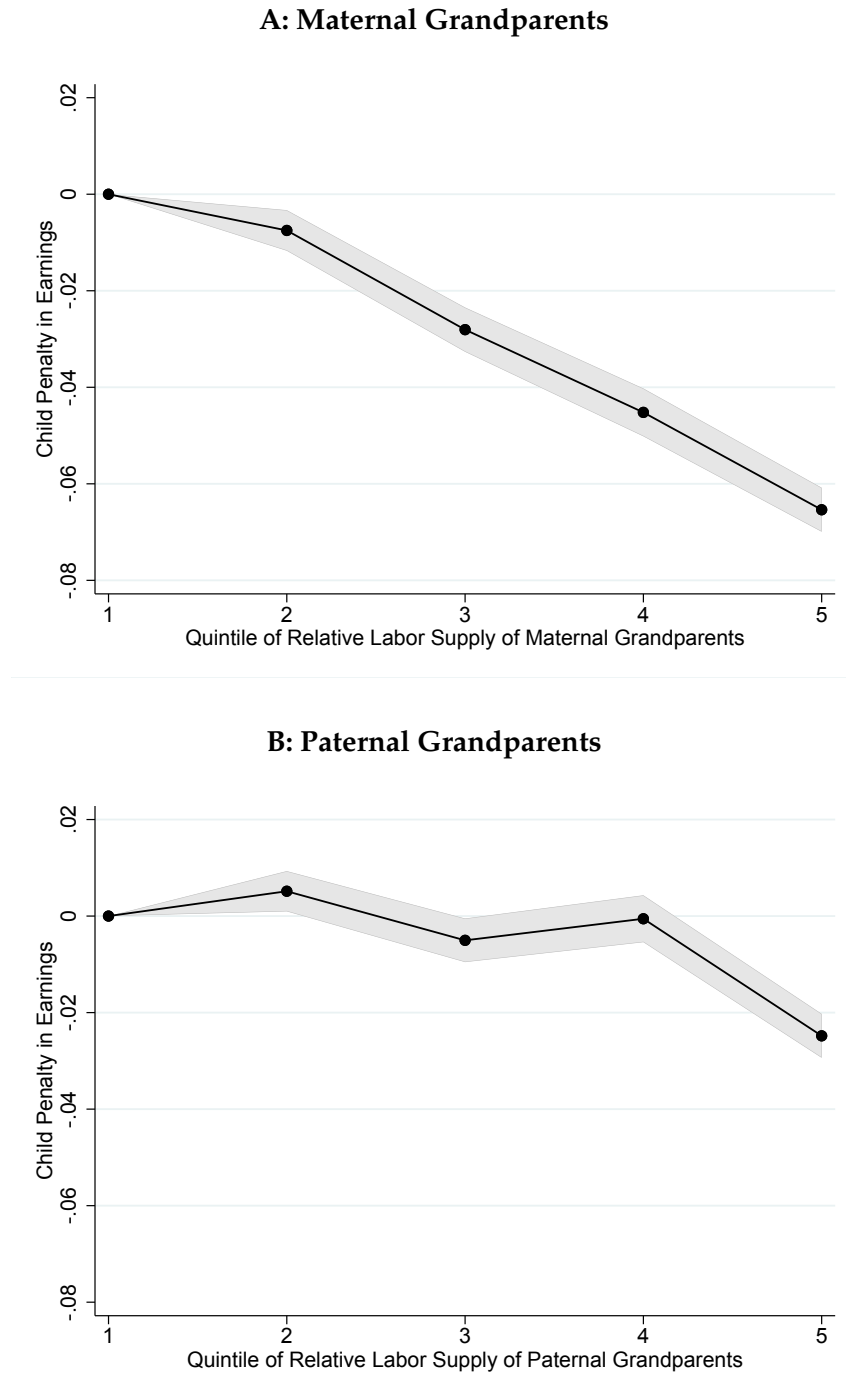
Notes: The figure shows dynamic Oaxaca-Blinder decompositions based on equations (3) and (4). The decomposition shown in Panel A allows for year-specific event-time coefficients and control for a full set of age and year dummies. The decomposition in Panel B augments the specification with education dummies: primary school, secondary school, vocational training, short post-secondary school, bachelor's degree, and master's/phd degrees. In both panels, the child-related gender gap captures the effect of child penalties  $P_{st}$  (the "unexplained" effect of children). In Panel B, the education-related gender gap includes both the effect of different education levels (explained effect) and the effect of different education coefficients (unexplained effect). The residual gender gap represents the effects (explained and unexplained) of age and year dummies.

Figure 6: Intergenerational Transmission of Child Penalties without Controls



Notes: The figure shows the child penalty in earnings against quintiles of the relative labor supply distribution of the grandparents. The relative labor supply of grandparents is based on cumulated hours worked over the period 1964-79 (obtained from ATP pension contributions). The child penalties are estimated using equation (5) and the statistic reported is  $P_q$  as defined in Section 5.2. The panels only include controls for the birth cohort of the grandparents. The shaded 95 % confidence intervals are based on robust standard errors.

Figure 7: Intergenerational Transmission of Child Penalties with Controls



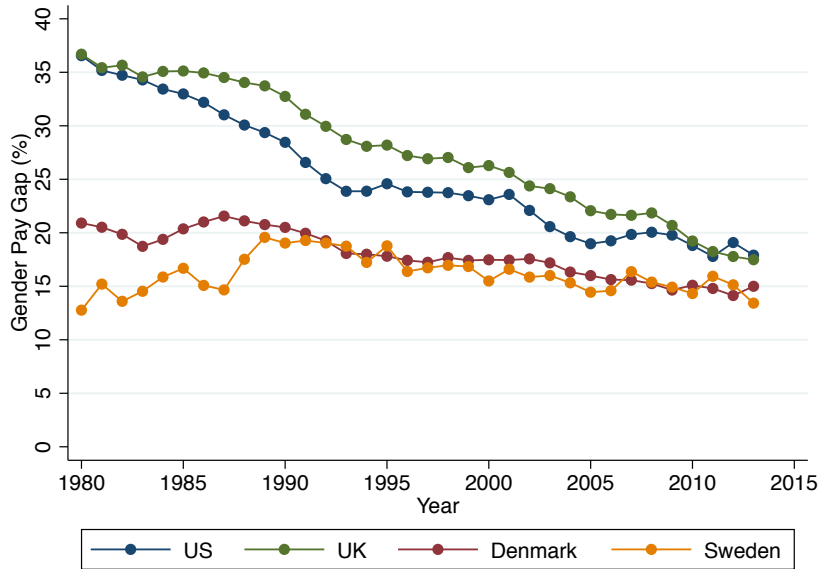
Notes: The figure shows the child penalty in earnings against quintiles of the relative labor supply distribution of the grandparents. The relative labor supply of grandparents is based on cumulated hours worked over the period 1964-79 (obtained from ATP pension contributions). The child penalties are estimated using equation (5) and the statistic reported is  $P_q$  as defined in Section 5.2. The panels allow for a set of non-parametric controls for within-generation wealth rank of the grandparents, grandparents region of residence as well as the birth cohort of the grandparents. The shaded 95 % confidence intervals are based on robust standard errors.

# Online Appendix

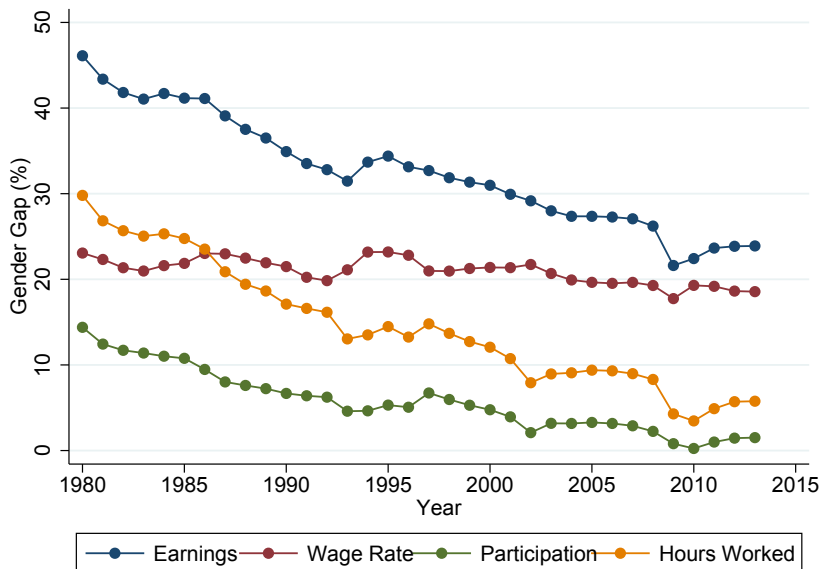
## A Supplementary Figures and Tables

Figure A.I: Gender Gaps Across Countries 1980-2013

**A: Convergence of the Gender Pay Gap Across Countries**  
 Median Earnings for Full-Time Workers



**B: Evolution of Gender Gaps in Denmark**  
 Means for All Workers

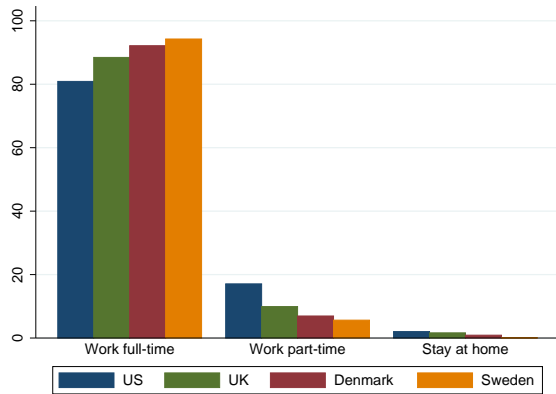


Notes: The time series in Panel A are drawn from OECD.org, except for Denmark where we use our own calculations of median earnings for full-time workers aged 16-64 (where full time is defined based on the ATP hours measure described in section 2.2). Our calculations for Denmark uses the same underlying data as the official OECD series, but is more consistent with the sample definitions used for the other countries. In Panel B the gaps in earnings and participation are calculated for the entire population aged 16-64 regardless of employment status, while the gaps in hours worked and wage rates are calculated conditional on participation.

Figure A.II: Gender Norms Across Countries

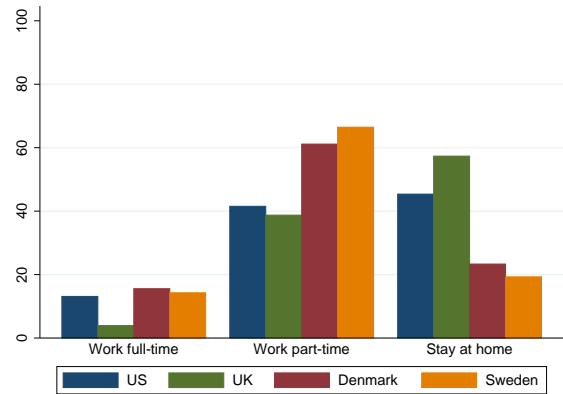
**A: Women Without Children**

*Do you think that women should work outside the home full-time, part-time or not at all when they are married but with no children?*



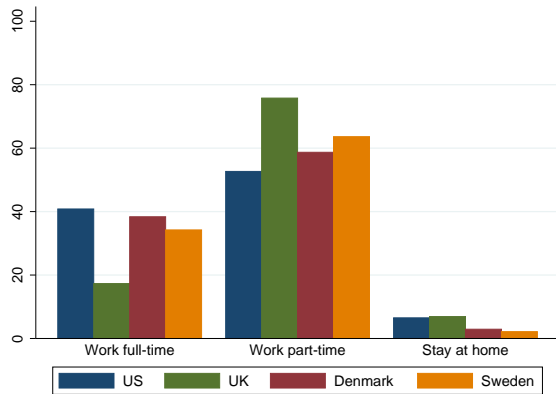
**B: Women With Children Under School Age**

*Do you think that women should work outside the home full-time, part-time or not at all when there is a child under school age?*



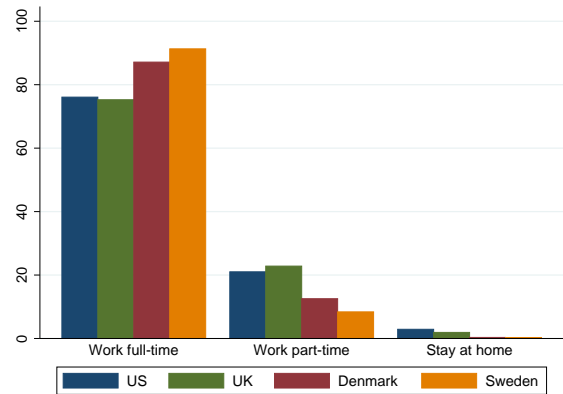
**C: Women With Children In School**

*Do you think that women should work outside the home full-time, part-time or not at all when the youngest child is still in school?*



**D: Women With Children Who Have Left Home**

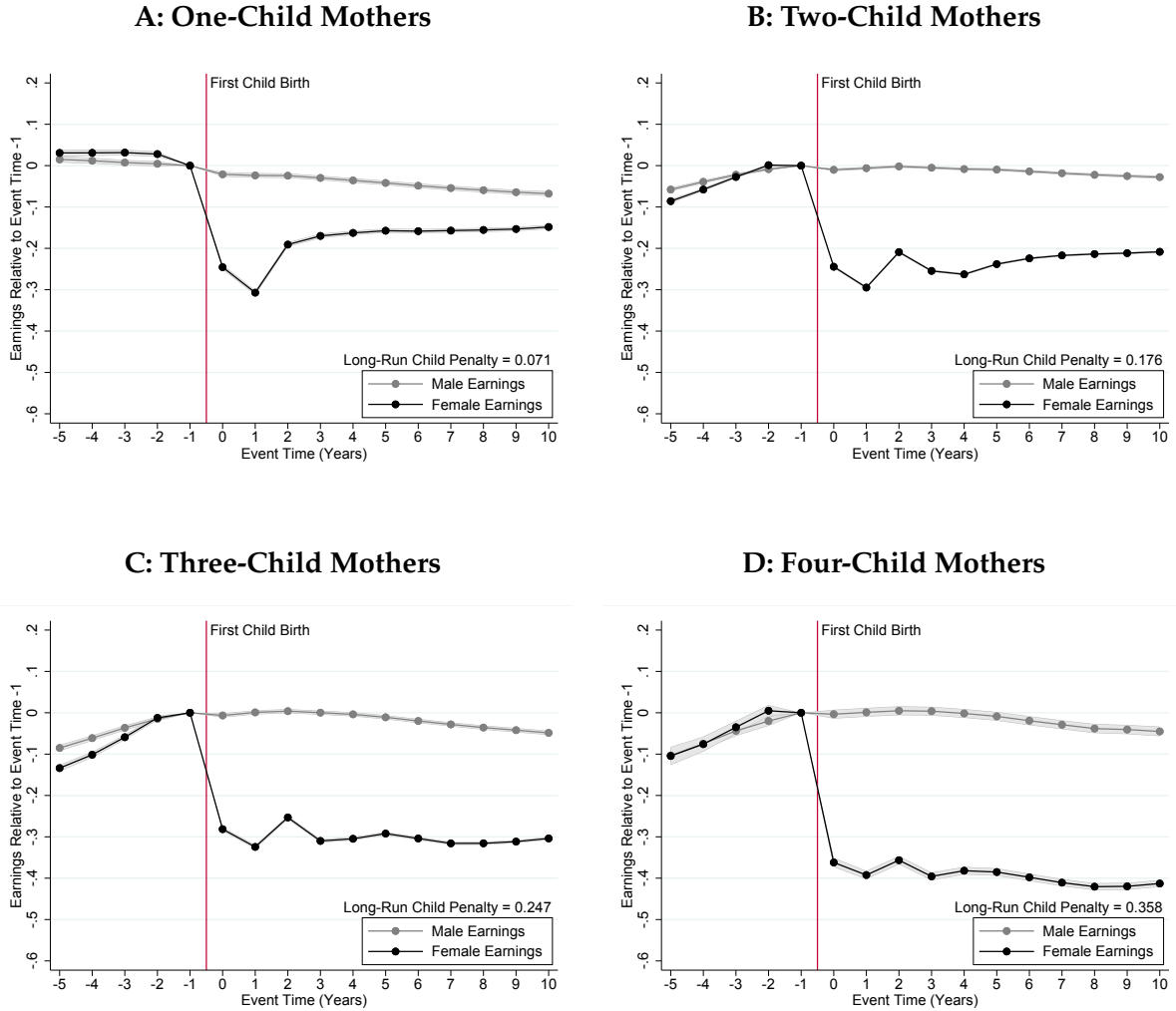
*Do you think that women should work outside the home full-time, part-time or not at all when the child has left the home?*



Notes: The figure is based on data from the International Social Survey Program (ISSP) in 2002. Each panel shows shares (in percent) choosing each of the 3 listed categories.

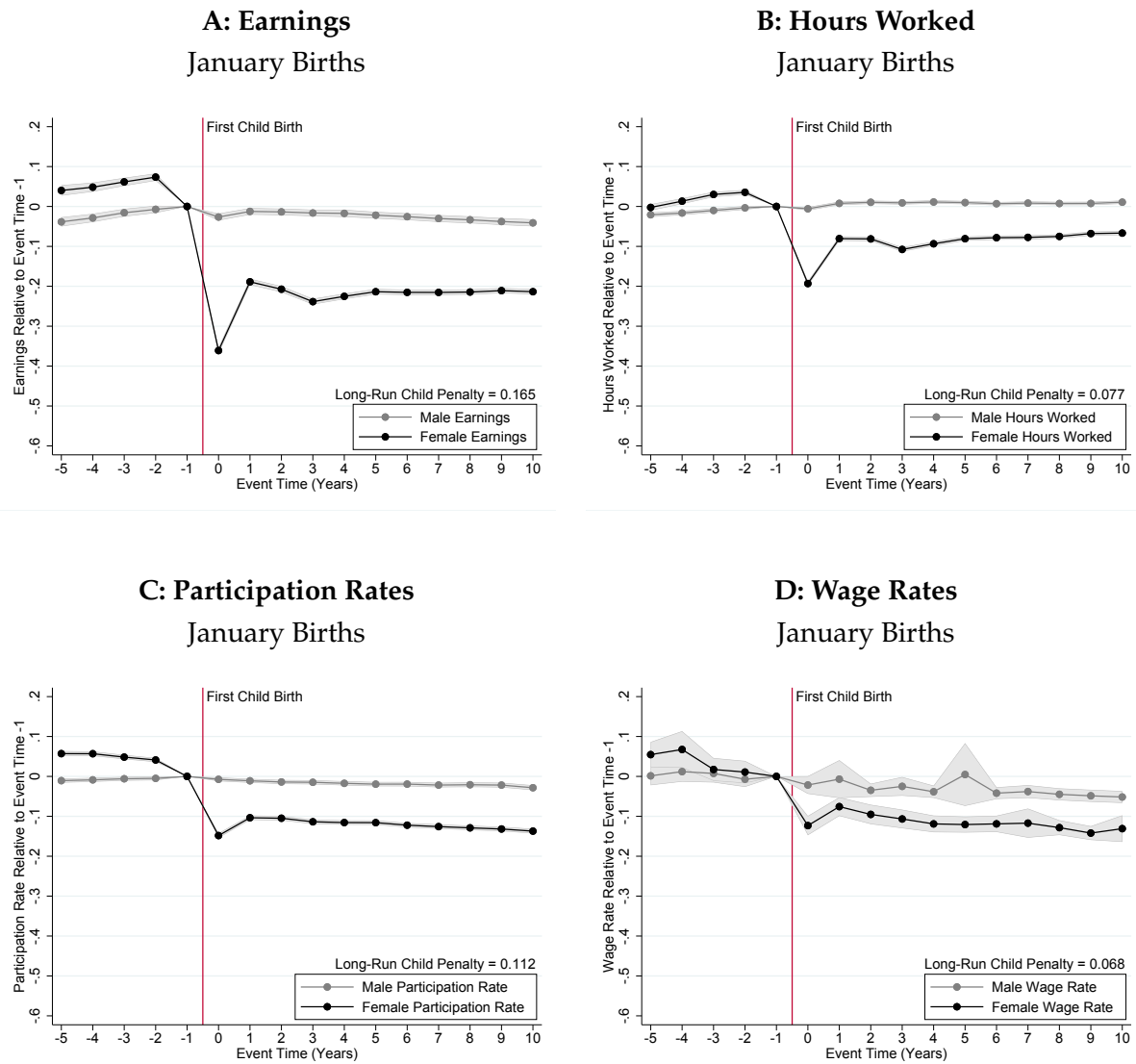


Figure A.III: Earnings Impacts by Number of Children



Notes: The figure shows the impact of children on earnings exactly as in Figure 1A, but splitting the sample by the woman's total number of children as of 2013 (1, 2, 3, or 4 children).

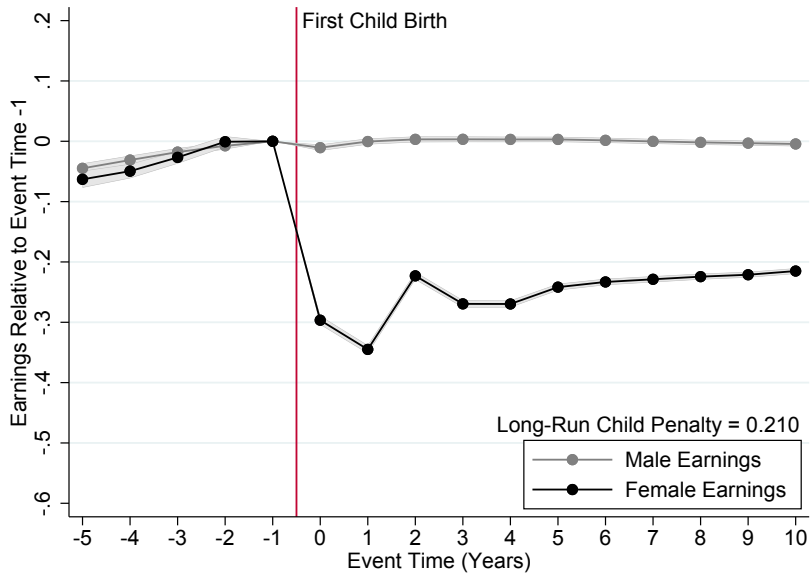
Figure A.IV: Impacts of Children Born in January



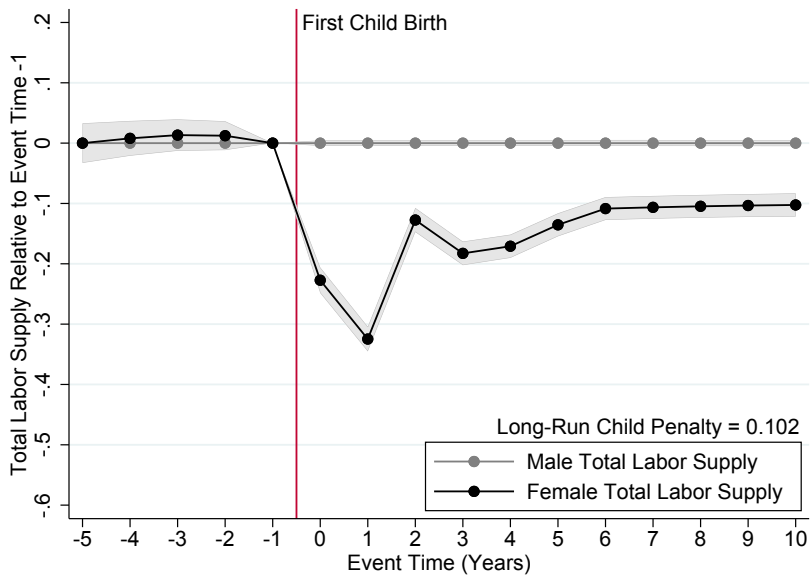
Notes: The figure is constructed as Figure 1, but estimated on the subsample of individuals who have their first child in January.

Figure A.V: Median Impacts of Children

**A: Earnings**



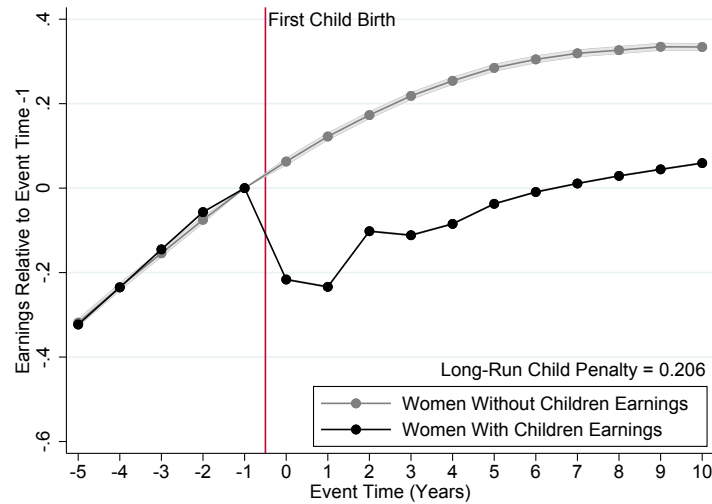
**B: Total Labor Supply**



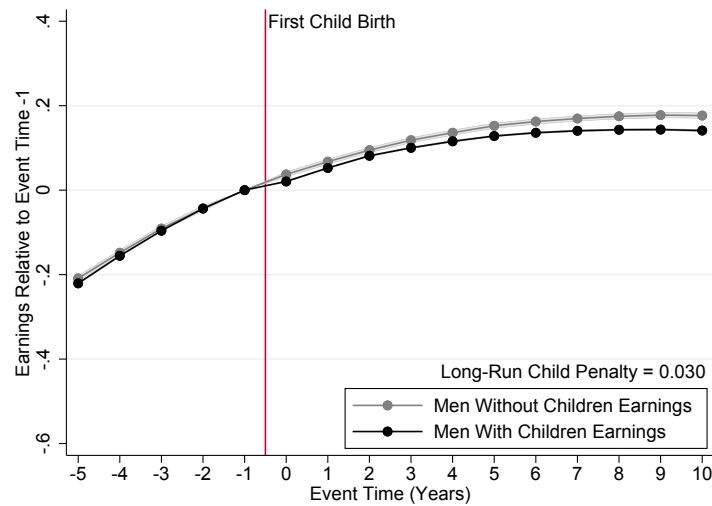
Notes: The figure is constructed in the same way as in Figure 1, but showing median impacts on earnings and total hours worked (including the zeros, thereby combining the intensive and extensive margins). These quantile regressions are based on a 1/7 subsample, which makes the confidence bands somewhat larger.

Figure A.VI: Earnings Impact of Children in a Difference-in-Differences Event Study Design

**A: Women Who Have Children vs Women Who Don't**

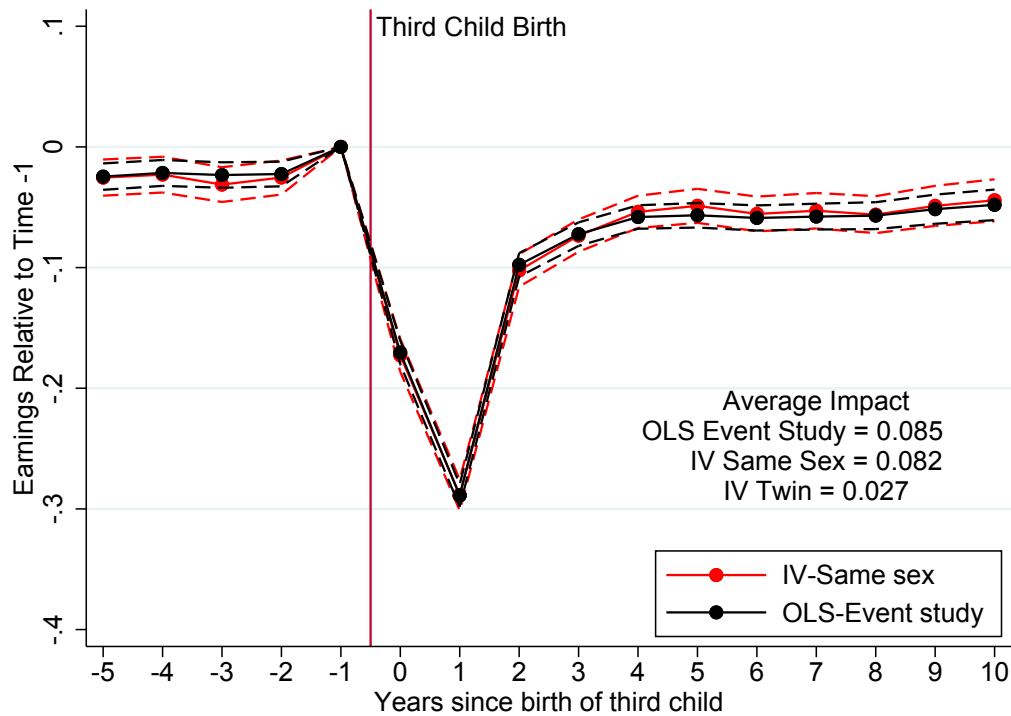


**B: Men Who Have Children vs Men Who Don't**



Notes: The figure show the evolution of earnings relative to the year before the birth of the first child for individuals with children compared to those who never have children (assigning placebo births based on the observed distribution of age at first child among those who have children). Panel A shows the evolution in earnings for women and Panel B the evolution for men. In the figure we control for year fixed effects. The details of how we construct the control groups of men and women who never have children are described in Appendix B.2. The figure reports long-run child penalties for men and women separately, estimated as a difference-in-differences between those who have children and those who never have children (as opposed to previous penalty measures based on comparing men and women, both of whom have children). The shaded 95 % confidence intervals are based on robust standard errors.

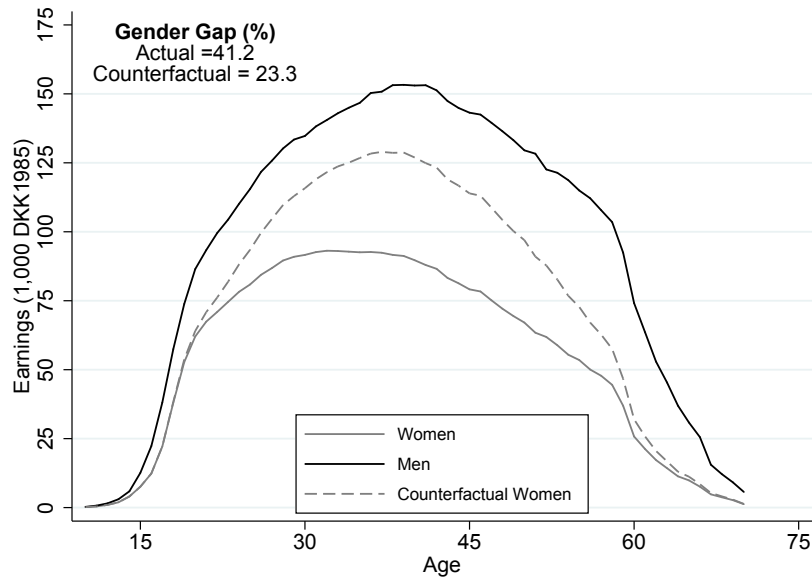
Figure A.VII: Earnings Impact of Third Child in Event Study vs IV Designs  
 Sibling Sex Mix or Twin Births as Instruments



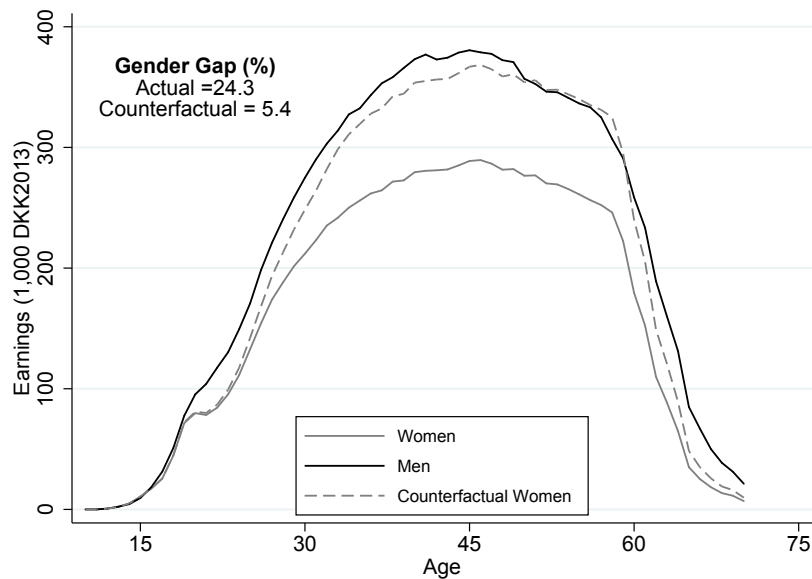
Notes: The figure shows the earnings impact of a third child on women ( $P_t^w$  defined in section B.2) obtained from the OLS event study specification (black series) and the IV same-sex specification (red series) as a function of years since the birth of the third child. The event study estimates are based on specification (11) in Appendix B.3. The IV-specification is based on the same specification using the sex mix of the first two children as instrument. The 95 % confidence intervals are based on robust standard errors. The figure also compares the average impact (i.e., across event times 0-10) obtained from the event study, the IV using sibling sex, and an IV using twins in the second birth.

Figure A.VIII: Decomposing the Age Profiles of Gender Inequality in Earnings

**A: 1985-Profile Decomposed**

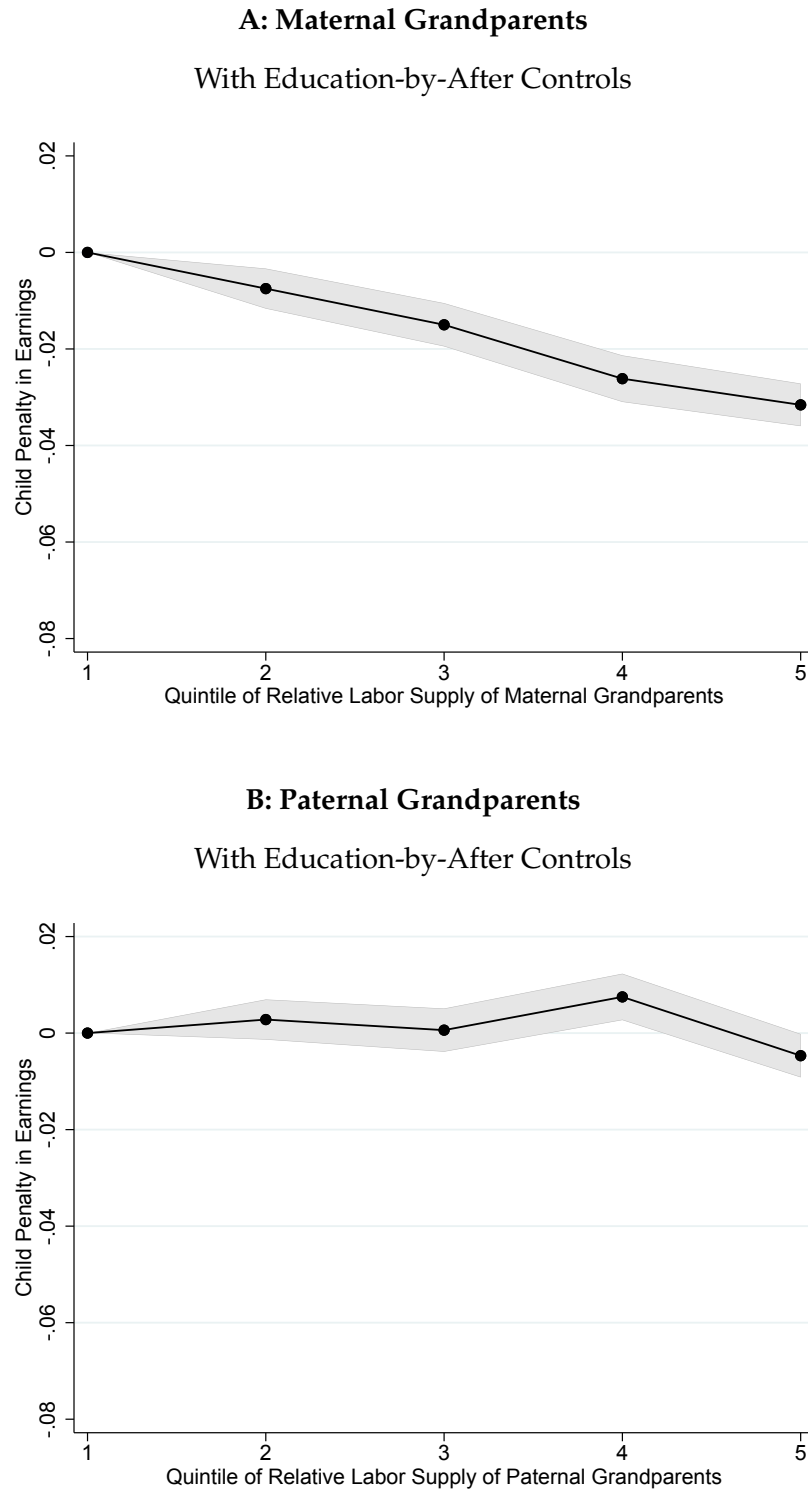


**B: 2013-Profile Decomposed**



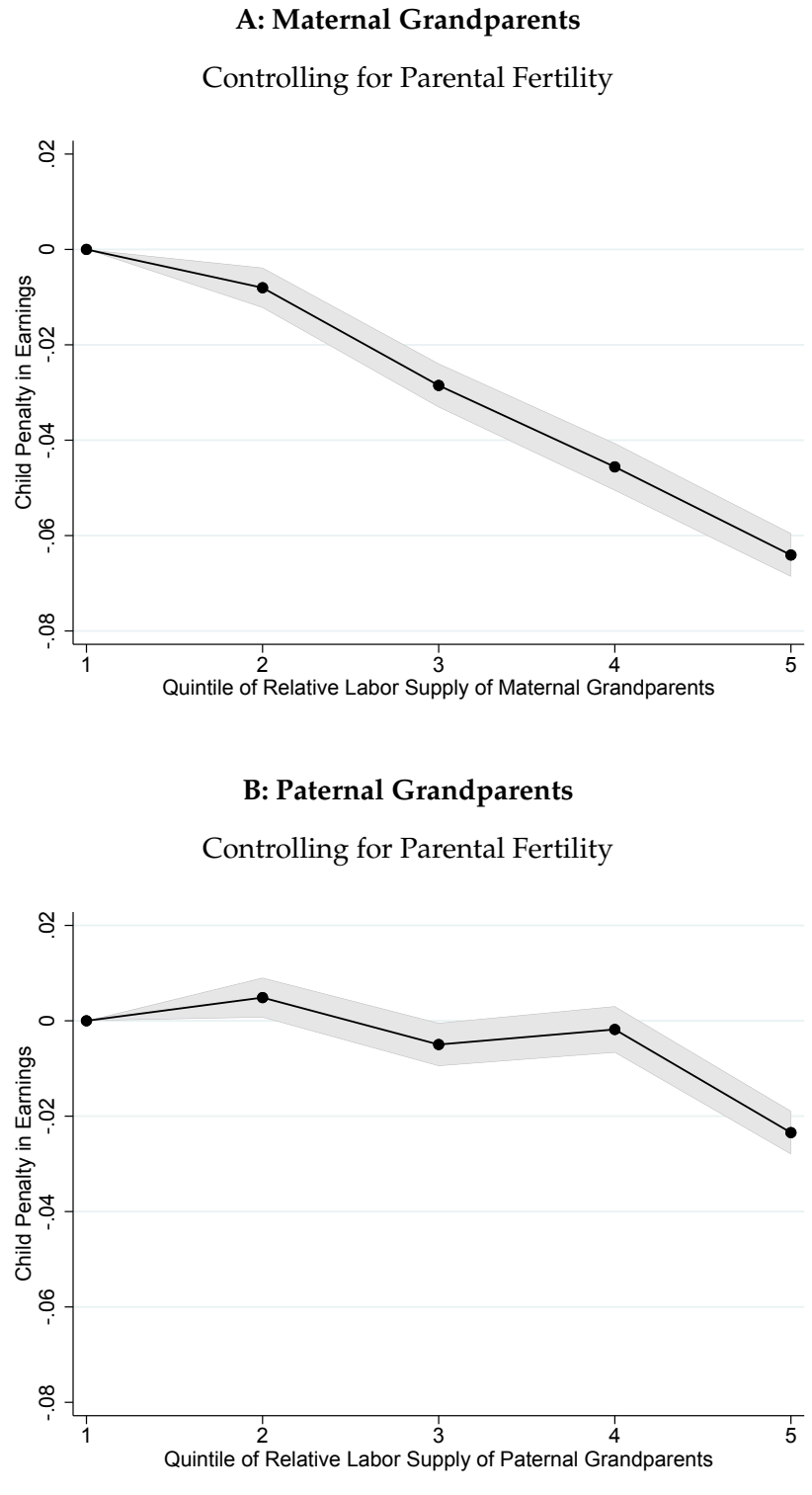
Notes: Based on the child penalties estimated from specification (3), this figure decomposes the within-year age profiles of gender inequality in earnings for 1985 and 2013. The difference between actual male earnings (solid black) and counterfactual female earnings (dashed grey) corresponds to non-child inequality, while the difference between counterfactual female earnings (dashed grey) and actual female earnings (solid grey) corresponds to child-related inequality. These within-year age profiles aggregate to the averages shown in Figure 5A for those two years.

Figure A.IX: Intergenerational Transmission of Child Penalties with Richer Controls



Notes: The figure shows the child penalty in earnings against quintiles of the relative labor supply distribution of the grandparents. The relative labor supply of grandparents is based on cumulated hours worked over the period 1964-79 (obtained from ATP pension contributions). The child penalties are estimated using equation (5) and the statistic reported is  $P_q$  as defined in Section 5.2 extended with a rich set of non-parametric controls for the education level/fields of the grandparents interacted with the after dummy. The shaded 95 % confidence intervals are based on robust standard errors.

Figure A.X: Intergenerational Transmission of Child Penalties Controlling for the Fertility Channel



Notes: The figure shows the child penalty in earnings against quintiles of the relative labor supply distribution of the grandparents. The relative labor supply of grandparents is based on cumulated hours worked over the period 1964-79 (obtained from ATP pension contributions). The child penalties are estimated using equation (5) and the statistic reported is  $P_q$  as defined in Section 5.2 extended with non-parametric controls for the complete fertility of the women. The shaded 95 % confidence intervals are based on robust standard errors.



Table A.I: Robustness Checks: Intergenerational Transmission of Child Penalties  
 Child Penalty Estimates by Quintiles of the Relative Labor Supply of the Grandparents

Specification:	(1)	(2)	(3)	(4)	(5)	(6)	(7)
<b>Panel A: Maternal Grandparents</b>							
2nd Quintile	0.000 (0.002)	-0.001 (0.002)	-0.009 (0.002)	-0.008 (0.002)	-0.008 (0.002)	-0.008 (0.002)	-0.008 (0.002)
3rd Quintile	-0.013 (0.002)	-0.014 (0.002)	-0.029 (0.002)	-0.029 (0.002)	-0.028 (0.002)	-0.015 (0.002)	-0.029 (0.002)
4th Quintile	-0.029 (0.002)	-0.027 (0.002)	-0.041 (0.003)	-0.044 (0.003)	-0.045 (0.003)	-0.026 (0.002)	-0.046 (0.003)
5th Quintile	-0.061 (0.002)	-0.061 (0.002)	-0.066 (0.002)	-0.065 (0.002)	-0.065 (0.002)	-0.032 (0.002)	-0.064 (0.002)
<b>Panel B: Paternal Grandparents</b>							
2nd Quintile	0.007 (0.002)	0.007 (0.002)	0.003 (0.002)	0.005 (0.002)	0.005 (0.002)	0.003 (0.002)	0.005 (0.002)
3rd Quintile	0.003 (0.002)	0.002 (0.002)	-0.007 (0.002)	-0.005 (0.002)	-0.005 (0.002)	0.001 (0.002)	-0.005 (0.002)
4th Quintile	0.01 (0.002)	0.011 (0.002)	0.005 (0.002)	0.001 (0.002)	-0.001 (0.002)	0.007 (0.002)	-0.002 (0.002)
5th Quintile	-0.023 (0.002)	-0.023 (0.002)	-0.025 (0.002)	-0.024 (0.002)	-0.025 (0.002)	-0.005 (0.002)	-0.024 (0.002)
<b>Grandparental Controls</b>							
Birth Cohort	X	X	X	X	X	X	X
Wealth Quintiles		X	X	X	X	X	X
Wealth Quintiles × After			X	X	X	X	X
Region				X	X	X	X
Region × After				X	X	X	X
Education					X	X	X
Education × After						X	
<b>Parental Controls</b>							
Fertility							X
Fertility × After							X

Notes: This table shows child penalty estimates by quintiles of the relative labor supply of the maternal grandparents (Panel A) and the paternal grandparents (Panel B) under different sets of controls in columns (1)-(7). Wealth controls consist of dummies for the within-generation wealth rank (quintiles) of the grandparents. Region controls add dummies for the region of residence of the grandparents. Education controls add dummies for both the the length and field of education (22 dummies) for both the grandmother and the grandfather. Fertility controls consist of dummies for the total number of children of the parents (measured in 2014). Specification (1) corresponds to the estimates shown in Figure 6, specification (5) to Figure 7, specification (6) to Figure A.IX, and specification (7) to Figure A.X.

## B Identification

### B.1 Conceptual Framework

In this section we set out a simple conceptual framework to clarify what is being estimated in the event studies. We denote by  $\mathbf{k}_i = (0, \dots, k_{it}, \dots, k_{iT})$  the anticipated lifetime path of fertility for individual  $i$ : the individual starts life with zero children, has  $k_{it}$  children at time  $t$ , and ends up with  $k_{iT}$  children over the lifetime. Earnings at time  $t$  are chosen based on the number of children present at time  $t$  as well as the anticipated lifetime path of fertility. Specifically,

$$\begin{aligned} Y_{it} &= F(k_{it}, \mathbf{x}_{it}, \mathbf{z}_{it}) \\ &= F(k_{it}, \mathbf{x}(k_{it}, \mathbf{k}_i, \mathbf{z}_{it}), \mathbf{z}_{it}), \end{aligned} \tag{6}$$

where  $Y_{it}$  is earnings,  $\mathbf{x}_{it} = \mathbf{x}(k_{it}, \mathbf{k}_i, \mathbf{z}_{it})$  is a set of earnings determinants that are chosen based on children, and  $\mathbf{z}_{it}$  is a set of earnings determinants that do not depend on children. Compared to the empirical specification (1), we simplify notation by leaving out indexation of gender and calendar time. The elements of  $\mathbf{x}_{it}$  include variables such as hours worked, occupation, sector and firm — variables which we have seen respond to children in the event studies — while the elements of  $\mathbf{z}_{it}$  include factors such as age, ability and preferences. Hence, in this framework earnings may respond directly to children conditional on choices (e.g. the impact of being tired or distracted at work) and indirectly through labor market choices  $\mathbf{x}_{it}$  (e.g. the impact of switching to a lower-paying, but more family-friendly firm). Furthermore, we allow labor market choices  $\mathbf{x}_{it}$  to respond both to the contemporaneous number of children  $k_{it}$  and to the entire path of past and future fertility. The latter effect captures for example that some women may take less education or opt for family-friendly career tracks knowing that they will eventually have many children.

While we do not specify the demand for children, we make the assumption that children  $k_{it}$  are exogenous to the outcome variable  $Y_{it}$  conditional on the set of underlying determinants  $\mathbf{z}_{it}$ . The assumption that “the event” (in our case, child birth) is not determined by the outcome variable is fundamental to any event study analysis. The graphical evidence presented above lends support to this assumption: there is no indication that outcomes respond *prior* to child birth (or prior to pregnancy as discussed above); the sharp breaks in career trajectories always occur *just after* having children.

This framework allows for two conceptually different effects of children on earnings. One is a

*pre-child effect* of future children, conditional on the current number of children  $k_{it}$ , which operates through the dependence of labor market choices  $\mathbf{x}_{it}$  on anticipated lifetime fertility  $\mathbf{k}_i$ . The other is a *post-child effect* of current children, conditional on anticipated lifetime fertility  $\mathbf{k}_i$ , which operates through both the direct effect of  $k_{it}$  and the effect of  $k_{it}$  on labor market choices  $\mathbf{x}_{it}$ . An obvious but important point is that the event studies cannot capture pre-child effects — these are incorporated in the pre-event levels that are differenced out — and is designed to identify only post-child effects. If women are investing less in education and career in anticipation of motherhood (as the child penalty sharply reduces the return to such investments), then the pre-child effect on female earnings is negative and the event study provides a lower bound on the total effect.<sup>23</sup>

Under what conditions do the event studies correctly identify the post-child impacts? It is important to distinguish between short-run and long-run impacts. The short-run impact is estimated by comparing event times *just* before and after time zero. Denoting these event times by  $t_-, t_+$  and using equation (6), the short-run event study estimates capture

$$E[Y_{it_+} - Y_{it_-}] = E[F(1, \mathbf{x}(1, \mathbf{k}_i, \mathbf{z}_{it_+}), \mathbf{z}_{it_+})] - E[F(0, \mathbf{x}(0, \mathbf{k}_i, \mathbf{z}_{it_-}), \mathbf{z}_{it_-})], \quad (7)$$

when we do not directly control for elements for  $\mathbf{z}_{it}$  through for example age and year dummies. Assuming smoothness of the average non-child earnings path, i.e.  $E[F(0, \mathbf{x}(0, \mathbf{k}_i, \mathbf{z}_{it_-}), \mathbf{z}_{it_-})] \approx E[F(0, \mathbf{x}(0, \mathbf{k}_i, \mathbf{z}_{it_+}), \mathbf{z}_{it_+})]$ , equation (7) identifies the short-run effect of the first child conditional on  $\mathbf{z}_{it_+}$ . With direct controls for  $\mathbf{z}_{it}$ , the smoothness assumption can be relaxed.

The long-run impact is obtained by considering an event time  $t_{++}$  long after time zero, i.e.

$$E[Y_{it_{++}} - Y_{it_-}] = E[F(k_{iT}, \mathbf{x}(k_{iT}, \mathbf{k}_i, \mathbf{z}_{it_{++}}), \mathbf{z}_{it_{++}})] - E[F(0, \mathbf{x}(0, \mathbf{k}_i, \mathbf{z}_{it_-}), \mathbf{z}_{it_-})]. \quad (8)$$

There are two differences between this impact measure and the previous one. The first difference is that the long-run impact captures the effect of total lifetime fertility  $k_{iT}$  as opposed to the effect of only the first child. The second difference is that the smoothness assumption is no longer sufficient for identification as we can still have large changes in non-child earnings components over a long event time window. Hence, if we are not fully controlling for  $\mathbf{z}_{it}$ , then the long-run child penalty may be a biased estimate of the true post-child impact. Allowing for non-parametric age and year controls as we do in specification (1) may go a long way in alleviating this problem, but we cannot

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<sup>23</sup>On the other hand, if women are engaging in intertemporal substitution of work effort around the event of having a child, then the pre-child effect could be positive. However, our event graphs feature very stable pre-trends that are identical for men and women, indicating that no significant intertemporal substitution is taking place.

be certain that there is no remaining bias. There are two potential solutions to the problem. One is to use a control group — naturally women and men who never have children — to account for the non-child earnings trend in a difference-in-differences design. The other is to leverage an instrument for child birth within our event study approach. In the following sections we consider both approaches.

## B.2 Identification Check: DD Event Study

In this section we lay out a difference-in-differences event study design that uses men and women who never have children as controls. The design is based on assigning placebo births to individuals who never have children, drawing from the observed distribution of age at first child among those who do have children (within cells of cohort and education).

Two technical issues arise when defining the control group and assigning placebo births. First, individuals observed without children include two types: those who will never have children and those who have not had children yet. The first group is the cleanest possible control group, but we face a truncation issue in identifying them. Taking age 40 as the latest age at which people have their first child (as only small fractions of both men and women have their first child after that age), the fertility of cohorts born after 1973 is truncated as they are younger than 40 when we last see them in 2013. Hence, for individuals observed without children in the later cohorts, we select those who are most likely never to have children based on a linear probability model of zero lifetime fertility ( $k_{iT} = 0$ ) as a function of observables, estimated on the (non-truncated) cohorts born between 1955–1973.<sup>24</sup> In each of the later cohorts, the number of individuals we select as having zero lifetime fertility is such that the probability of never having children is the same after 1973 as the average between 1955–1973 (as this probability has been quite stable during this time).<sup>25</sup>

<sup>24</sup>Specifically, we estimate the following model separately for men and women:

$$P[k_{iT} = 0] = X'\beta, \tag{9}$$

where  $k_{iT} = 0$  is a dummy for zero lifetime fertility and  $X$  includes the following dummy controls: quartiles of the income distribution of the individual's cohort, quartiles of the wealth distribution of the individual's cohort, quartiles of the wealth distribution of the spouse's cohort, education length/degree (8 categories), decade of generation of the maternal grandmother, decade of generation of maternal grandfather, and region of residence. The model is estimated on the non-truncated cohorts from 1955-1973. We predict the probability of zero lifetime fertility for the truncated cohorts after 1973 as  $\hat{P} = X'\hat{\beta}$ .

<sup>25</sup>That is, for each cohort  $c$  born after 1973, we rank individuals based on their  $\hat{P}$ s. Among those from cohort  $c$  who have no children by 2013, we pick the  $n_c$  individuals with the highest  $\hat{P}$ s such that  $\frac{n_c}{N_c} = P_{1955-1973}$ , where  $N_c$  is the total number of individuals (men and women, respectively) in the cohort and  $P_{1955-1973}$  is the average fraction of individuals with  $k_{iT} = 0$  for cohorts 1955-1973. These  $n_c$  individuals are assumed to have zero lifetime fertility. The rest of cohort  $c$  observed with no children are assumed to have children later, and are therefore not included among the controls.

Our control group consists of these selected individuals from the post-1973 cohorts along with all individuals without children from earlier cohorts.

Second, we need to allocate placebo births to those in our control group. Here we also have to distinguish between truncated cohorts born after 1973 and non-truncated cohorts born before that time. For the older cohorts, the distribution of age at first child  $A$  is approximated by a log-normal distribution within cells of birth cohort  $c$  and education  $e$ . That is, we assume  $A_{c,e} \sim \mathcal{LN}(\hat{\mu}_{c,e}, \hat{\sigma}_{c,e}^2)$  where the mean  $\hat{\mu}_{c,e}$  and variance  $\hat{\sigma}_{c,e}^2$  are obtained from the actual distributions within each cohort-education cell. Individuals in older cohorts without children get a random draw from this distribution. For the younger cohorts, we draw a random age at first child from  $\mathcal{LN}(\tilde{\mu}_{c,e}, \hat{\sigma}_{c,e}^2)$  where the mean  $\tilde{\mu}_{c,e}$  is the predicted average age at first child obtained by estimating a linear trend on the older cohorts. That is, consistent with the stylized pattern observed for the older cohorts, we allow for an upward linear drift in the age at first child while keeping the variance constant.

With this setup, we are able to implement event studies that compare our treatment group (a balanced panel of those who have their first child between 1985–2003 and are observed in a 15-year window around the first child birth) to a control group (a balanced panel of those who never have children, but have been assigned a placebo birth between 1985–2003 and are observed in a 15-year window around the placebo). The impact of children can be estimated as a difference-in-differences, i.e.

$$\mathbb{E}[Y_{i,t>0} - Y_{i,t<0} \mid k_{iT} > 0] - \mathbb{E}[Y_{i,t>0} - Y_{i,t<0} \mid k_{iT} = 0]. \quad (10)$$

The identification assumption is a standard parallel trends assumption, which in the notation established above implies  $\mathbb{E}[\Delta F(0, \mathbf{x}(0, \mathbf{k}_i, \mathbf{z}_{it}), \mathbf{z}_{it}) \mid k_{iT} > 0] = \mathbb{E}[\Delta F(0, \mathbf{x}(0, \mathbf{0}, \mathbf{z}_{it}), \mathbf{z}_{it}) \mid k_{iT} = 0]$ . Given the parallel trends assumption — the validity of which we can verify from the pre-trends — it is not necessary to introduce controls for  $\mathbf{z}_{it}$ . Therefore, we drop the age dummies in the specification discussed below.<sup>26</sup>

Figure A.VI shows the earnings impacts of children in this difference-in-differences design. Panel A shows women while Panel B shows men. The event studies are very sharp and confirm the key qualitative findings from the baseline specification. Women with children and women without children are on identical pre-trends, diverge sharply at the time of the first child birth, and the impact is very stable over time. The impact of children 10 years after equals 20.6%, slightly larger

<sup>26</sup>We do keep the year dummies (in order to show changes in real terms), but the child penalty estimates are virtually unchanged when dropping the year dummies as well.

than the baseline impact of 19.4% shown in Figure 1A. The baseline graph already suggested that we were slightly underestimating the career cost of children on women: it showed a weak upward pre-trend for women (compared to men) that the child penalty estimate did not take into account. Using women without children as a control group accounts for this trend.

A new insight that emerges from the DD event study design is that men *are* affected by parenthood, although the effect is very small. Men who have children and men who don't are on identical pre-trends and then diverge at event time 0. Even though the effect is tiny, the perfect pre-trends and precision of our data makes the effect very clear. The long-run child penalty in male earnings is equal to 3%.

### B.3 Identification Check: IV Event Study

As another identification check, we compare our event study approach to an IV approach using the sex mix of the first two children as an instrument for having a third child (Angrist & Evans 1998). The idea is that parental preferences for variety make it more likely to have a third child when the first two have the same sex, while the children's sex should have no independent impact on labor market outcomes and thus satisfy the exclusion restriction.<sup>27</sup> As the sibling sex mix instrument gives the local average treatment effect of a third child, we have to modify our event study approach to also provide the local impact of the third child in order to compare the two approaches.

We consider the following event study specification for estimating the effect of a third child:

$$Y_{istt'} = \sum_{j \neq -1} \alpha_j \cdot \mathbf{I}[j = t] + \sum_k \beta_k \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \gamma_y \cdot \mathbf{I}[y = s] + \sum_{n \neq -1} \delta_n \cdot \mathbf{I}[n = t'] + \nu_{istt'} \quad (11)$$

where the index  $t$  still denotes event time with respect to the first child, while the new index  $t'$  denotes event time with respect to the third child. The first three terms on the right-hand side corresponds to our baseline specification: it gives the effect of the first child, controlling for a full set of age and year dummies. The fourth term on the right-hand side is new and it includes event time dummies around the birth of the third child (omitting  $t' = -1$ ). Even though the objective is to estimate the effect of the third child, we keep the event time dummies around the birth of the

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<sup>27</sup>The validity of this exclusion restriction can be verified based on our event study approach: we find no differences in the impacts of the first child depending on whether it is a boy or a girl, suggesting that child gender is not important for parental labor market outcomes. While the exclusion restriction is thus compelling, there might be a problem with the assumption of no defiers underlying LATE. Some parents may have preferences for a specific sex — typically boys — and are therefore less likely to have a third child if they start out with two boys. The presence of defiers due to boy-bias is arguably less of an issue in Denmark than in more traditional societies.

first child in the specification. This is because past child dynamics may matter for the impact of the third child.<sup>28</sup>

We run the specification on a sample of women who had their first child between 1985-2003 (as before) and who have completed fertility of two or three when we last observe them in 2013.<sup>29</sup> In order to capture the long-run effect of a third child, we expand from the previously balanced panel that included a 10-year window after the first child birth to an unbalanced panel that includes the longest possible window for each individual. For example, a woman who had her first child in 1985 and is observed until 2013 is included in the sample with event times up to  $t = 28$  with respect to that child, allowing for the longest possible period after the third child.

The IV-specification is the same as equation (11), but there we instrument the event time dummies around the third child birth using the sex mix of the first two children. Specifically, we instrument each dummy  $\mathbf{I}[n = t']$  by the interaction  $\mathbf{I}[n = t'] \times \mathbf{I}[\text{same sex siblings}]$ , which takes the value of one when the woman is at event time  $t'$  with respect to the third child and her first two children have the same sex. In contrast to previous implementations of such IV approaches, this specification traces out the full dynamic pattern of the effects of the third child.

The results are presented in Figure A.VII, which shows the earnings impacts of a third child obtained from the event study specification (black series) and the IV specification (red series) as a function of years since the birth of the third child. Analogous to the previous graphs, the statistic shown here is  $P_{t'}^w \equiv \hat{\delta}_{t'}^w / \mathbb{E}[\tilde{Y}_{istt'}^w | t']$  where  $\tilde{Y}_{istt'}^w$  is the counterfactual outcome absent the effect of the third child, but not the other children, i.e.  $\tilde{Y}_{istt'}^w \equiv \sum_j \hat{\alpha}_j \cdot \mathbf{I}[j = t] + \sum_k \hat{\beta}_k \cdot \mathbf{I}[k = \text{age}_{is}] + \sum_y \hat{\gamma}_y \cdot \mathbf{I}[y = s] + \hat{\eta}_i$ .

The following key insights emerge from the figure. First and foremost, the event study estimates and the IV estimates are almost perfectly aligned through event time, providing strong support for our empirical approach. The fact that the pre-event coefficients are very similar (and close to zero) for the two approaches suggests that anticipation effects of children are not important. The fact that the post-event coefficients are very similar shows that the impacts on women who have a third child because of the sex mix of the first two (the IV-compliers) are the same as the impacts on all treated women. Second, the short-run effect of a third child is similar to the short-run effect of the first child, an earnings reduction of 20-30%. Third, the long-run effect of a third child is about 5%.

<sup>28</sup>The reason why we can separately identify event time dummies for the first child and the third child, while simultaneously controlling for a full set of age and year dummies, is that there is enough independent variation in the ages at which women have their first and third child, respectively.

<sup>29</sup>As we do not include men here, we have dropped the superscript  $g$  in specification (11).

This is less than the long-run effect of the first child among those who only have one child (equal to 7.1% as shown in Figure A.III), suggesting that the marginal effect of children is declining in the number of children.

Besides sibling sex mix, a number of studies have considered the occurrence of twins at first birth (e.g. Rosenzweig & Wolpin 1980; Bronars & Grogger 1994) or at second birth (e.g. Angrist & Evans 1998) as instruments for children. As an alternative to the strategies discussed above, we may consider twins at second birth as an instrument for the third child. Figure A.VII therefore compares the average impacts obtained from the event study and same-sex IV to the average impact obtained from a twin IV. We see that the twin estimate is considerably smaller than the event study and same-sex estimates. A natural interpretation of this difference is that twins represent a more efficient child production technology (e.g. due to economies of scale) and therefore impose smaller penalties on women. This implies that even though the occurrence of twin births is an exogenous event, it is not a valid instrument for having an extra child in the standard sequential way: it does not satisfy the exclusion restriction if it changes aspects of the child care technology that have their own direct impact on earnings.<sup>30</sup> Our results are consistent with those of Angrist & Evans (1998), who also find smaller effects when using the twin instrument than when using the same-sex instrument.<sup>31</sup>

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<sup>30</sup>To be clear, the exogeneity of twins implies that it does give the causal effect of a twin birth relative to a singular birth (the reduced-form impact of a twin birth dummy), but it does not give the effect of increasing the number of children through the standard sequential birth technology.

<sup>31</sup>Angrist & Evans (1998) argue that, besides economies of scale, the smaller size of the twin estimates could be driven by differences in the age of the third child: in cross-sectional comparisons, a third child triggered by twins will be older than a third child triggered by same sex siblings. Our dynamic IV approach allows us to separate the age and economies-of-scale hypotheses by estimating the impact of the third child at each event time after birth. Such an exercise reveals that the twin estimates are smaller conditional on event time, which cannot be explained by age and suggests that economies of scale are important.