Parenthood and the Gender Gap: Evidence from Denmark*

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Abstract

Despite considerable gender convergence over time, substantial gender inequality persists in all countries. Using Danish administrative data from 1980-2011, we show that most of the remaining gender gap can be attributed to the dynamic effects of having children. The arrival of children leads to a long-run penalty in female earnings of 21% driven in roughly equal proportions by labor force participation, hours of work, and wage rates. Underlying this child penalty, we find clear dynamic effects of child birth on occupation, promotion to manager, and the family friendliness of the firm for women relative to men. The fraction of the aggregate gender gap that can be explained by children is strongly increasing over time—from 30% in 1980 to 80% in 2011—showing that non-child reasons for gender inequality have largely disappeared. Conditional on rich observables, the female child penalty in earnings is *increasing* in the relative skill of the female in the family, suggesting that mechanisms other than comparative advantage are at play. We probe into the potential role of "gender identity" effects by showing that the female child penalty is strongly related to the relative labor supply history of her parents. This is consistent with the notion that gender attitudes surrounding family and career are shaped in part by the environment in which individuals grow up.

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1 Introduction

Despite considerable gender convergence over the last century, substantial gender inequality persists 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 education 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 for the full population in Denmark since 1980, we provide a simple explanation for the persistence of the gender gap: the effects of parenthood on the careers of women relative to men are large and have not fallen over time. Hence, most of the remaining gender gap can be attributed to children. Our findings are surprising given that Scandinavian countries have been leaders in the implementation of legislation and policies that are supposed to allow women to balance career and family.

To provide context, Figure 1A shows the evolution of the gender gap in earnings for full-time workers in different countries. It is striking that the cross-country differences in gender inequality have largely disappeared over time. For example, while gender inequality in Denmark was dramatically lower than in the United States around 1980, today the gender pay gap is between 15-20% in both countries and appears to have plateaued at that level. That is, gender convergence happened earlier in Scandinavia than elsewhere, but the process also slowed down earlier in Scandinavia allowing other countries to catch up. So even though these countries feature different public policies and labor markets, they are no longer very different in terms of overall gender inequality.

To estimate the effect of parenthood on the careers of women relative to men, we adopt a quasi-experimental approach based 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. The long-run child penalty in female earnings equals 21% over the period 1980-2011. This should be interpreted as a total penalty that includes 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. Our ability to precisely estimate hours and wage rate effects relies on a unique administrative and third-party reported measure of working hours that is available for the full population.

Based on the event study approach, we find effects on several other margins that can shed light on some of the underlying mechanisms. Just after the birth of the first child, women start falling behind men in terms of their occupational rank (ordered by earnings level) and their probability of being manager. Furthermore, women switch jobs to firms that are more "family friendly" as measured by the share of women with young children in the firm's workforce, or by an indicator for being in the public sector which is known to provide very flexible conditions for working women (see Nielsen *et al.* 2004). Since family friendly firms are associated with lower earnings and wage rates, this response explains part of the gender gaps described above.

We proceed to decompose the gender gap in the full population (with and without children) into what can be attributed to children and the residual. We show that the fraction of the total earnings gap that can be explained by children has risen from 30% in 1980 to 80% in 2011. This dramatic change reflects a combination of two effects: (i) the child-related earnings gap has increased from about 14% to 18%, and (ii) the total earnings gap has fallen from about 45% to 22%.[1] To understand the first effect, note that although the female child penalty in percentage terms 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. Our findings have implications for future work on gender inequality, which should focus on understanding what drives gender roles and gender outcomes in relation to parenting. This is consistent with the views expressed by Goldin (2014) on what the "last chapter" of gender convergence must contain, but the persistence of child penalties in a country with generous family policies suggests that the last chapter may not be written any time soon.

Further insight can be obtained by analyzing the heterogeneity of child penalties across families. This analysis shows that relative skill within families—as measured by relative wage rates in the years prior to child birth—do not affect child penalties in the direction one might expect. Both the earnings penalty and the wage rate penalty are increasing in the skill of the mother relative to the father, conditional on a rich set of covariates. Even in families where the woman is the primary earner before having children she takes the major hit when children arrives. These findings are

[1] These gender gaps are larger than those reported in Figure 1A discussed above. This is because the cross-country evidence in Figure 1A is based on *median* earnings for *full-time* workers, whereas we are now considering *mean* earnings for *all* workers as shown in Figure 1B.

interesting in relation to the evidence on the disappearance of the gender gap in education (Goldin *et al.* 2006; Goldin & Katz 2008). While the closing of the education gap has a direct and positive effect on gender equality in earnings (consistent with the narrowing of the non-child gender gap that we document), the potential gain will not be fully realized if child penalties are borne to a larger degree by highly skilled women. The large child penalties for high-skill women that we estimate are consistent with evidence for the US by Wilde *et al.* (2010) and Bertrand *et al.* (2010).

The size and persistence of female child penalties, along with their heterogeneity across skill, are difficult to reconcile with comparative advantage alone. A recent literature discusses the importance of social norms and gender identity for explaining the different labor market outcomes of men and women, although causal testing of these ideas is difficult (Bertrand 2011; Bertrand *et al.* 2013). We explore the potential role of such effects by showing that the female child penalty is strongly related to the labor supply history of her parents, conditional on the socio-economic characteristics of the family. For example, in traditional families where the mother works very little compared to the father, their daughter pays a much larger child penalty when she eventually becomes a mother herself. 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 1980-2011 to the within-family work history one generation before. 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).

Our paper speaks to the large literature on gender inequality in the labor market (surveyed by Altonji & Blank 1999; Bertrand 2011), and it is closely related to a body of work emphasizing the importance of parenthood for gender differences (e.g. Waldfogel 1998; Paull 2008; Bertrand *et al.* 2010; Wilde *et al.* 2010; Adda *et al.* 2011; Goldin 2014). We push this literature based on an event study methodology that take advantage of the quality and comprehensiveness of the Danish administrative data. Although we find that the dynamic effects of children on the gender gap are very large, one could argue that the event study approach represents a *lower* bound due to a potential effect of children that it misses. If women select family-friendly career paths (offering flexible hours and generous maternity leave, but lower wages) based on their *planned* fertility prior to child birth, then the pre-child gender gap partly reflects a child penalty. This idea is consistent with our finding that women are working in relatively family-friendly firms and sectors prior to

child birth, and that this by itself reduces the child penalty following child birth (see also Nielsen *et al.* 2004). From this perspective our estimates may be viewed as conservative.

Our paper is also related to the literature on family labor supply and fertility. This literature has tried to estimate the causal effect of children on female labor supply using potentially exogenous variation in family size coming from twin births, sibling sex mix, miscarriage and infertility (see e.g., Browning 1992; Bronars & Grogger 1994; Angrist & Evans 1998; Hotz *et al.* 2005; Aguero & Marks 2008). The empirical approach in our paper is different from this body of work, and the objective is different too. Our identification is based on sharp breaks in career trajectories that occur *just after* having children for women, but not for men. These sharp dynamic patterns are unlikely to be driven by omitted variables or selection on unobservables as these factors should be smooth around the precise moment of child birth.

While we are thus estimating a causal effect of children on labor market outcomes, it is important to keep in mind that having children is a choice and this affects the interpretation. In particular, by estimating the relationship between children and career choices, our results are most naturally interpreted as measuring complementarity in preferences. We show that this complementarity is very strong for women but not for men, and that these gendered preferences can account for most of the remaining gender inequality. The key question is why preferences are so strongly gendered. Is it biology or is there a role for environmental influences in the formation of preferences? We start probing into these questions by considering patterns of heterogeneity and intergenerational transmission, but future work should go further in the investigation of the underlying mechanisms as this will ultimately determine the welfare and policy implications of the patterns we uncover here.

The paper is organized as follows. Section 2 describes the context and data, section 3 lays out the event study methodology, section 4 presents the empirical results, and section 6 concludes.

2 Context and Data

2.1 Context

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. However, Figure 1 shows that this is far from the full story. The cross-country comparisons in Panel A were discussed above and shows that Denmark is no longer a strong outlier in terms of the gender gap in earnings for full-time workers. Panel B focuses on Denmark alone and shows gender gaps in different labor market outcomes for all workers. We see that the gender gap in participation has gradually disappeared over the last three decades and that the gender gap in hours worked has fallen substantially, but that large gaps persist in earnings and wage rates (defined as earnings/hours worked among those who are working). The earnings gap is now around 22% and is created mostly by differences in wage rates and to a smaller degree by differences in hours worked.[2]

Figure 2 probes the idea that gender norms are more egalitarian in Scandinavia than elsewhere. The evidence in the figure is based on questions from the International Social Survey Program (ISSP) regarding the attitudes that people have towards market work by women with and without children. Specifically the survey asks participants whether they think women should work outside the home full-time, part-time or not at all when they have no children (Panel A), have children under school age (Panel B), have children in school (Panel C), and have children who have left home (Panel D). Two striking insights emerge from the figure: one is that gender attitudes are still quite traditional—essentially that women should work full-time before having children and after the children have left home, while they should work only part-time or not at all when they have children living at home—and the other is that different countries are very similar in holding this view. The only noticeable cross-country difference is that the Scandinavian 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 US and UK populations, but overall the similiarities in gender attitudes stand out much more than the differences. The figure is based on samples that include both men and women, but interestingly there is very little difference in these gender attitudes between men and women. Overall and in contrast to common wisdom, the evidence presented in Figures 1 and 2 raises doubts about the degree to which Scandinavian countries are positive outliers in terms of gender equality in the labor market.

The policy environment in Denmark is one which combines large tax-transfer distortions (which

[2] As we describe below, the way we measure hours worked means that we understate somewhat the gender hours gap and by implication overstate the gender wage rate gap. That is, the decomposition in Figure 1B of the earnings gap into the underlying margins is tilted somewhat from hours worked to wage rates.

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 1980-2011. For the study of intergenerational transmission we exploit additional administrative data going back to 1964. The Danish data combines several different administrative registers (linked at the individual level via personal identification numbers) and therefore contains rich information on children, earnings, labor supply, occupation, firm, education, and many other outcomes. Furthermore, the data allows us to link family members, generations, and workers to firms.

The Danish population is currently 5.5 million people and there were around 2 million child births during the period 1980-2011. For our main event study analysis we focus on *first* child births where both 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 1985-2001 where both parents are alive and reside in Denmark throughout a 15-year window around the birth. We condition on both parents being at least 20 years of age when having their first child (teenage births constitute only 2.3% of all births during the 1985-2001 period). We do not impose restrictions on the relationship status of the parents: we include all individuals who have a child together in a given year and follow them through the 15-year window whether or not they are married, cohabiting, separated, divorced, or have not formed a couple yet in any given year. This leaves us with a core estimation sample of around 350,000 births or 11,200,000 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.[3] 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.

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. Moreover, we are able to validate our results for the discrete hours measure against a continuous hours measure reported by all firms on behalf of their employees, but only since 1997.

3 Event Study Approach

The idea of the event study approach is to estimate female child penalties based on (sharp) changes that occur just after child birth for mothers relative to fathers. For each parent in the data we denote by $t = 0$ the year in which the individual has his/her first child and index all years relative to that year. As described above, we consider 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. We then study the evolution of different outcomes (earnings, labor supply, wage rates, etc.) as a function of event time.

[3] The scheme also covers self-employed individuals who contribute on their own behalf.

Specifically, denoting by Y_{ist} the outcome of interest for individual i in year s at event time t, we run the following regression separately for men and women

$$Y_{ist} = \sum_{t \neq -1} \alpha_t \cdot EVENT_{it} + \sum_j \beta_j \cdot AGE_{is}^j + \sum_s \gamma_s \cdot YEAR_s + \nu_{ist}, \tag{1}$$

where $EVENT_{it}$ is an event time dummy, AGE_{is}^j is an age dummy for being j years old, and $YEAR_s$ is a year dummy. We omit the event time dummy at $t = -1$, implying that the event coefficients α_t measure the impact of children relative to the year just before the first child birth. If we did not include the age and year dummies in the specification, the estimated event coefficients $\hat{\alpha}_t$ would correspond simply to the mean value of the outcome at event time t, measured relative to the year before birth. By including a full set of age dummies we control non-parametrically for non-child related career progression, and by including year dummies we control non-parametrically for non-child related time changes such as wage inflation and business cycles. In other words, the age and year dummies remove any underlying life-cycle and time trends in the outcomes we consider.

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 $\hat{\alpha}_t / E\left[\hat{Y}_{ist} \mid t\right]$ where $\hat{Y}_{ist}$ is the predicted outcome when omitting the contribution of the event dummies, i.e. $\hat{Y}_{ist} \equiv \sum_j \hat{\beta}_j \cdot AGE_{is}^j + \sum_s \hat{\gamma}_s \cdot YEAR_s$. This captures the year-t effect of having a child as percentage of the counterfactual outcome absent the child. We estimate this separately for men and women and denote the gender-specific effects by $P_t^k \equiv \hat{\alpha}_t^k / E\left[\hat{Y}_{ist}^k \mid t\right]$ where $k = m, w$. We then define the *long-run child penalty* on women as the average effect of children over a 10-year horizon for women relative to men, i.e.

$$\Delta P \equiv P^m - P^w \quad where \quad P^k \equiv E\left[P_t^k \mid 0 < t \leq 10\right]. \tag{2}$$

Hence, the child penalty ΔP is the percentage by which women are falling behind men due to children over a 10-year period following the first child birth. The choice of a 10-year window is based on the empirical analysis below, which shows that the effect is roughly at a steady state by that time.

To gain insight into the potential determinants of child penalties, we present a detailed study of heterogeneity using the rich observational data. Here we consider penalties at the family level

and regress these on a range of non-parametric controls. The long-run child penalty on the female in family i is defined as

$$\Delta p_i \equiv p_i^m - p_i^w \quad where \quad p_i^k \equiv \frac{E\left[Y_{it}^k \mid 0 < t \leq 10\right] - E\left[Y_{it}^k \mid -5 \leq t < 0\right]}{E\left[Y_{it}^k \mid -5 \leq t < 0\right]}, \tag{3}$$

that is, the percentage change in a given outcome between the 5-year period before birth and the 10-year period after birth for the man relative to the woman within a family. The family-level penalty in equation (3) is conceptually similar to the aggregate-level penalty in equation (2), but in general it does not aggregate to the same number: the average over family-level penalties in percentages ($E\left[\Delta p_i\right]$) is not the same as the percentage penalty in the average levels (ΔP) due to a potential correlation between penalties and the outcome level. Furthermore, when focusing on family-level penalties, we have to drop families in which one or both of the parents have zeros (non-participation) in all five years preceding the arrival of the child. The key advantage of defining family-level penalties is that it allows us to study heterogeneity in a given dimension while controlling for the (correlated) variation in other relevant determinants of penalties.

We regress the penalty Δp_i on a range of variables that capture the socio-demographics, work environment, and relative skill of the two parents in the family. The richest specification we consider is the following

$$\begin{aligned} \Delta p_i \quad = \quad & \sum_c \beta_{1c} \cdot COHORT_i^c + \sum_k \sum_j \beta_{2jk} \cdot AGE_i^{jk} + \sum_j \beta_{3j} \cdot KIDS_i^j + \sum_j \beta_{4j} \cdot INCOME_i^j \\ & + \sum_j \beta_{5j} \cdot EDUCATION_i^j + \sum_j \beta_{6j} \cdot SKILL_i^j + \sum_k \sum_j \beta_{7j} \cdot EXPERIENCE_i^{jk} \\ & + \beta_8 \cdot PUBLIC_i + \sum_j \beta_{9j} \cdot FRIENDLY_i^j + \sum_k \sum_j \beta_{10j} \cdot PROFESSION_i^{jk} + \mu_{ic}. \end{aligned} \tag{4}$$

The explanatory variables in (4) are dummies defined as follows: $COHORT_i^c$ is a dummy for the first child being born in year c, AGE_i^{jk} is a dummy for parent k being j years old when having the first child, $KIDS_i^j$ is a dummy for having j children in total (1, 2, 3, 4+), $INCOME_i^j$ is a dummy for being in the jth decile of the household income distribution just before having the first child, $EDUCATION_i^j$ is a dummy for being in the jth quartile of the distribution of relative years of education between parents (based on completed education before having a child), $SKILL_i^j$ is a dummy for being in the jth decile of the distribution of relative wage rates between parents (based on the five years prior to having the first child), $EXPERIENCE_i^{jk}$ is a dummy for parent k having j years of experience between completing education and the arrival of the first child (bottom coded

at zero if education is completed after child birth), $PUBLIC_i$ is a dummy for the woman working in the public sector at the time of having her first child, $FRIENDLY_i^j$ is a dummy for the woman working in a firm belonging to the jth quartile of the distribution of family friendliness when she has her first child, and $PROFESSION_i^{jk}$ is a dummy for parent k being in profession j (based on 22 categories education fields). Family friendliness $FRIENDLY_i^j$ is based on the share of women with young children in the firm's workforce (or by the presence of women with young children in the firm's management).

4 Empirical Results I: Child Penalties and the Gender Gap

4.1 Estimating Child Penalties

We start by documenting a set of stark changes in the labor market outcomes of women relative to men around the event of having the first child. Figure 3 plots P_t^m and P_t^w as defined in section 3: these are outcomes for men and women as a function of event time t, measured relative to the year just before the birth of the first child ($t = -1$) and controlling 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 enormous amount of precision in the administrative data. Panel A starts by considering 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 a strikingly parallel way until they become parents. But at the precise moment that the first child arrives, the earnings paths of men and women begin to diverge: women experience an immediate drop in gross earnings of almost 30%, while men experience no significant variation in their earnings. Importantly, 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 plateaued around 20% below its level just before child birth, whereas male earnings are essentially unaffected by children. As shown in the figure, these estimates imply a long-run child penalty in the earnings of women relative to men (ΔP defined in equation (2)) equal to 20.8%.

While we take an event study approach using the birth of the first child, the evidence presented in Figure 3 is based on the full population of women with children, irrespective of the total number of children a woman ends up having. This implies that the dynamic patterns we document include the effects of children born after the first one, and the estimated long-run child penalty is naturally interpreted as the aggregate penalty of all children. In appendix Figure A.I we replicate

the earnings event study from Figure 3A for different numbers of children (1, 2, 3, 4+). The impact of children is sharp for all four family sizes and the magnitude of the long-run child penalty varies with the number of children in the expected direction. The child penalty increases by roughly 10 percentage points per child. Note that the event study graph for families with two children in Panel B of Figure A.I looks very similar to the graph for all families in Figure 3, which is natural given that the average completed fertility per woman in Denmark is close to two conditional on having children.

In general, earnings responses can be driven by three margins: labor force participation, hours worked conditional on participation, and the wage rate. In panels B-D of Figure 3 we analyze how parenthood affects each of these three margins separately using the same methodology as above. Panel B plots the evolution of hours worked for men and women relative to the year before the first child birth. Hours worked follow the same qualitative pattern as earnings, with a sharp and persistent drop after child birth for women relative to men. Three years after birth, hours worked by women are 10% lower than before birth, while hours worked by men are almost unchanged. Ten years after birth, there is no sign of convergence; a persistent 10% gender gap in hours worked has been created due to children. Panel C displays the evolution of the labor force participation rates of men and women. Again, the labor force participation trends of men and women are perfectly similar until the birth of a first child, at which point they sharply diverge with a 10% relative drop for women that fully persists over time. Finally, Panel D shows that wage rates feature a similar dynamic pattern: men and women are on very similar trends prior to the birth of the first child, diverge immediately after birth, creating a 10% gap between men and women that does not fade over time.

These results show that the female child penalty in earnings is in part a direct consequence of intensive and extensive labor supply adjustments made by the family after the birth of the first child. At the same time, the wage rate patterns suggest that there is more going on than these quantitative labor supply adjustments. A possibility is that women, once they become mothers, make active career decisons in other more qualitative dimensions (choice of occupations, tasks, firms) that have immediate and persistent effects on their wage rates. We provide direct evidence on such margins of adjustment in the next section. Interestingly, the estimated long-run penalties at the intensive, extensive, and wage rate margins are roughly similar in magnitude, suggesting that these margins are equally important for understanding the long-run effect of children on the

earnings paths of women relative to men.[4]

In the event graphs presented so far, the drop in earnings and labor supply in event year 0 is not much larger than the drop in subsequent event years. While this may seem surprising, note that the use of calendar-year measures of earnings and labor supply tend to create attenuation bias in the first-year dip compared to a continuous time representation: as women give birth some time during year 0, some of the earnings and hours in calendar-year 0 were realized prior to child birth. To investigate this point, we reproduce Figure 3 on a sample restricted to child births in January for which the definition of calendar years and event years coincide. The results are shown in appendix Figure A.II from which the following insights emerge. First, when focusing on January births alone we do see a pronounced dip in event year 0 as one would expect. This dip reflects the extra time taken out of the labor market immediately following delivery. Second, focusing on January births also reveal a slight drop in labor market outcomes in event year -1, which can be explained by sick leave and parental leave (for which women are eligible during the last four weeks of pregnancy) taken just before giving birth. Third and most important, the long-run child penalties over a 10-year horizon are very similar for January births and all births, which implies that the calendar-year presentation in Figure 3 does not introduce any bias in the long run.

We have presented estimates on the career cost of children using child births between 1985-2001 and an event study horizon that includes 10 post-birth years. It is of course interesting to study how these career patterns evolve in the very long run, which is feasible to explore with our data. In appendix Figure A.III we consider child births between 1985-1991 and a 20-year post-birth horizon. The figure is otherwise similar to Figure 3 and shows results for earnings, hours worked, participation, and wage rates. The long-run child penalty estimates shown in each panel is based on an average over event years 10-20 (ΔP in equation (2) for $10 < t \leq 20$). The figure shows how strikingly persistent the effects of children are. In fact, the earnings penalty 20 years after child birth is almost the same as the penalty 10 years after. The only qualitative difference that emerges from considering 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.

It is useful to take a step back in order to discuss identification and how to interpret the event study estimates we have presented. Consider first the relationship between our approach and

[4]The child penalties in panels B-D of Figure 3 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.

the vast literature on family labor supply and fertility (e.g., Browning 1992). This literature has discussed the difficulties of interpreting the observed negative correlation between children and female labor supply, noting that causal inference is difficult due to omitted variables and reverse causation. A series of papers try to estimate the causal effect of children on female labor supply using potentially exogenous instruments for family size such as twin births, sibling sex mix, miscarriage, and infertility (e.g., Bronars & Grogger 1994; Angrist & Evans 1998; Hotz *et al.* 2005; Aguero & Marks 2008). While this literature has been constrained by data limitations—having to rely on cross-sectional variation in survey data—we leverage full-population administrative panel data in order to pursue an event study strategy that exploits sharp breaks in career trajectories occurring *just after* having children for women relative to men. The sharp dynamic patterns that we document are unlikely to be driven by omitted variables (such as unobserved heterogeneity in career preferences) as these should be smooth around the moment of child birth, nor are they driven by reverse causality as the labor market changes occur after child birth. Broadly speaking, our identification is related to the fundamental insights of Sims (1972) and the concept of Granger causality: we exploit the fact that the arrival of children is sharply related to future career trajectories, but not to past career trajectories. Examples of papers that come close to our event study strategy include Paull (2008) on the impact of children on hours worked in the UK and Wilde *et al.* (2010) on the impact of children on female wages in the US, although they do not push the analysis of anatomy, mechanisms, and secular composition changes as we do in this paper.

While the preceding arguments suggest that we are uncovering a causal relationship between children and labor market outcomes, it is important to keep in mind that having children is a choice and this affects the interpretation. Three points are worth noting. First, we are estimating the effect of children on the sample of individuals who have selected parenthood as opposed to the effect of an exogenous change in children on the full population. As in the IV approaches discussed above, what we obtain is a treatment effect on the treated. Second, since we are estimating the relationship between choice variables—having children and various labor market choices—the results are most naturally interpreted as measuring complementarity in the utility function. The decision to have children and a less ambitious career is strongly complementary for women, but not for men. The deeper question is why this complementarity is so strongly gendered, a question to which we return in section 5. Third, because parenthood is a *planned* choice, some of the labor supply and career decisions that are complementary to parenthood could be made prior to the birth of the first child. Although the striking similarity of pre-child trends for men and women suggests that such

anticipatory responses are relatively limited, we cannot rule out that some women make child-related career choices far in advance, before our event study window starts. For example, this would be the case if a woman decides to never enter the labor force in anticipation of becoming a mother. For such a woman, the estimated child penalty using our event study methodology would be zero, although the true child penalty is positive and already incorporated in the pre-child gender gap in participation rates. Hence the large child penalties that we find are, if anything, *lower bounds* on the total career effects of children.

4.2 Anatomy of Child Penalties

We have seen that motherhood is associated with large and persistent penalties in earnings driven in roughly equal proportions by participation, hours of work, and wage rates. These empirical patterns, and especially the large effects of children on wage rates, beg the question of what are the underlying mechanisms that drive the effects. This section focuses on this question, leveraging the rich administrative data to explore if child birth changes women's careers in qualitative dimensions that affect their productivity in the labor market. The results are presented in Figure 4, which is based on the same event study methodology used above.

Panels 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 (controlling non-parametrically for age effects), but that women start falling behind men soon after parenthood. Note that the occupation graphs begin to diverge in event year 1 rather than in event year 0 as for earnings and labor supply. This makes sense 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 come back to work. Panel B also explores occupational rank, but focuses specifically on the probability of being top manager. It is in general harder to uncover effects of children on this margin, because relatively few individuals have risen to the managerial level prior to becoming parents. Nevertheless, the graph suggests that parenthood has a negative effect on women's prospects of becoming managers. While the male and female trends are not perfectly similar prior to child birth, they do begin to diverge at a much faster pace following child birth.

The bottom panels turn to the choice of work environment and in particular its family friendliness. We first consider the link between parenthood 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 for those with 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 clearly confirmed in Panel C. Women and men are on very similar pre-child trends in terms of their probabilities of working in the public sector, but begin to diverge strongly soon after having a child. Ten years after child birth, women have a 10pp higher probability of public sector employment than men, relative to the year before child birth. As with occupation, the divergence mainly starts in year 1 rather than in year 0, i.e., when women return to work after their parental leave.

Finally, Panel D considers a proxy for the family friendliness of a work environment that also encompasses heterogeneity across firms in the private sector. Here we take advantage of our employer-employee matched data by defining a firm's "family friendliness" as the share of women with children below 15 years of age in the firm's workforce (excluding the considered woman's own first child when it arrive). Having a larger share of female employees with young children may reflect that the firm offers more family-friendly policies, or that the firm is more family friendly in the broader sense of employing people that women with children see as like-minded. Since our measure of firm family friendliness is negatively related to wage rates, if women move into more family-friendly firms following parenthood, this helps explain the wage rate penalties documented above. The outcome shown in Panel D is the percentile rank in the distribution of firm family friendliness for men and women, respectively, relative to the year before child birth. Although men and women are not on identical trends prior to birth (the female trend is steeper), there is a very clear break in the relative trends around the moment of having a child. Women begin to move into family-friendly firms at a much higher pace in the years following child birth, whereas the male trend is completely unaffected by child birth. The female trend is increasing somewhat already in event year -1, consistent with an anticipation effect of motherhood. Taking the differential pre-trend into account we estimate a long-run effect of parenthood on the percentile rank in the distribution of firm family friendliness for women relative to men equal to 4.36.

Overall, the results in Figures 3-4 show that women's career trajectories change sharply due to motherhood, creating substantial gender inequality in a range of quantitative and qualitative dimensions. The results demonstrate the difficulties that women continue to face in trying to

balance career and family, and are broadly consistent with the arguments by Goldin (2014) on the "last chapter" of gender convergence. As discussed above, our large effects are, if anything, lower bounds as they do not include the potential anticipatory responses to planned parenthood. For example, while we find sharp effects on women's decision to work in the public sector or in a family friendly firm just after child birth, it is entirely conceivable that some women have made decisions to be in such sectors and firms far in advance in anticipation of eventual motherhood. Consistent with this, women are more likely than men to work in the public sector or in a more family friendly firm already prior to birth. Such lifetime effects are difficult to estimate without making strong parametric assumptions.

4.3 Decomposing Gender Inequality Over Time

In this section we study the long-run evolution in the composition of gender inequality into what is driven by children and what is driven by other factors (such as human capital or discrimination). For the reasons just discussed, our decomposition into child-related gender inequality and residual gender inequality will, if anything, be biased towards the latter, because there may be lifetime effects of anticipated parenthood that are not captured by our event study methodology. As we shall see, this potential bias makes our findings all the more striking.

Our decomposition analysis is implemented as follows. The first step is to estimate cohort-specific child penalties, which we do by extending the baseline event study specification (1) in the following way

$$Y_{ist} = \sum_{s} \sum_{t \neq -1} \alpha_{st} \cdot EVENT_{it} \cdot YEAR_s + \sum_{j} \beta_j \cdot AGE_{is}^{j} + \sum_{s} \gamma_s \cdot YEAR_s + \nu_{ist}, \tag{5}$$

with the only innovation being that we interact the event time dummies by year dummies in order to estimate year-specific event coefficients α_{st}. Note that estimating event coefficients by year s and event time t amounts to estimating event coefficients by birth cohort $c = s - t$. As in the baseline specification we consider an event time window running from -5 to +10, but we expand from the previously balanced panel of individuals who have their first child between 1985-2001 to a semi-balanced panel of individuals who have their first child at any point during the data period 1980-2011. The sample is *semi*-balanced in the sense that early cohorts are not observed all the way back to event time -5 (for example, birth cohort 1981 is not observed before event time -1) and that late cohorts are not observed all the way up to event time +10 (for example, birth cohort 2009 is

not observed after event time +2), but within each cohort we require both parents to be present in the maximum number of years possible within our data period. Expanding the sample in this way have no major impact on any of our conclusions, but it is helpful for separately identifying event×year coefficients and year coefficients by creating more independent variation in event time and calendar time towards the end points of the data period.

The earnings penalties for birth cohorts 1985-2001 obtained from specification (5) are shown in Figure A.IV. We show short-run earnings penalties in Panel A (an average over event time 0-4) and long-run earnings penalties in Panel B (an average over event time 5-10). We see that there is some cyclicality in the short-run child penalty faced by women, but no statistically significant long-run trend. On the other hand, the long-run child penalty features no cyclicality, but a statistically significant negative time trend.

The second step of the analysis requires us to take a stand on the child penalties faced by women who have their first child outside our event study period, but are in the labor market at some point during 1980-2001. For example, women who have their first child in 1978 will be at event time +7 in 1985, and we have to assign a child penalty associated with this event time and year for our historical decomposition analysis. The results presented in Figure A.IV give guidance on how to do this. The child penalties for event time 5-10 are on an almost perfectly linear trend between 1985-2001, and so we extrapolate linearly to obtain penalties for these event times outside our event study period. On the other hand, the child penalties for event time 0-4 are not trending between 1985-2001, and so we simply assume that they were constant at their 1985 level prior to that year and that they were constant at their 2001 level following that year.

The third step of the analysis requires us to take a stand on the child penalties faced by women after the end of our event time window, i.e. from event time +11 onwards. We already analyzed longer-run penalties in Figure A.III, which showed clearly that earnings penalties are extremely stable from event year +10 onwards. Hence we assume uncontroversially that each women is at a steady state from 10 years after birth.

The fourth and final step is to decompose the gender gap using the estimates and assumptions described above. Building on the notation from section 3, the percentage child effect in event year t and calendar year s is denoted by P_{st}^k for $k = m, w$, and so the female child penalty associated with this event and calendar time is given by $\Delta P_{st} \equiv P_{st}^m - P_{st}^w$. Given the previous three steps, we have an estimate of ΔP_{st} for any event time and any year during 1980-2011, which we can use to decompose the gender gap. If the actual earnings of a woman with children are Y_{ist}, then we

construct the earnings she would have had absent children as $\hat{Y}_{ist} \equiv Y_{ist}/(1-\Delta P_{st})$. We do not adjust the earnings of men (as the adjustment for women is already based on an estimate relative to men), nor do we adjust the earnings of women before they become mothers or women who never have children. Using the adjusted earnings $\hat{Y}_{ist}$, we construct a new gender gap—this is the residual gap not related to children. The difference between the residual gender gap and the actual gender gap is the child-related gender gap.

The results of our decomposition exercise are shown in Figure 5. We see that the fraction of gender inequality in earnings that can be attributed to children has increased dramatically over time, from about 30% in 1980 to about 80% in 2011. This secular change reflects a combination of two effects: (i) the child-related gender gap in earnings has increased from about 14% to 18%, and (ii) the total gender gap in earnings has fallen from about 45% to 22%. To understand the first effect, note that although the *percentage* child penalty on women has fallen slightly over time (as shown in Figure A.IV), 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. That is, at a time where non-child gender inequality is falling (for example, due to changes in education or discrimination) while child penalties are roughly constant or falling by less, there will be a tendency for child-related gender inequality to go up.

Our findings imply that, to a first approximation, the remaining gender inequality is all about children. This has important implications for future work on gender inequality, which should focus on understanding what drives gender outcomes in relation to parenthood. There is likely to be some biological element to these gender differences (innate differences in childrearing abilities or preferences), but the more interesting question for economists is whether the strongly gendered parental outcomes are also driven by environmental influences, labor markets, and policy. These are the aspects that the gender inequality literature has already been focusing on (see Bertrand 2011), but the findings in Figure 5 highlight that those topics must be studied specifically in the context of having children in order to shed light on the remaining gender inequality.

5 Empirical Results II: Heterogeneity and Mechanisms

5.1 Heterogeneity in Child Penalties

We have documented the existence of large child penalties on women's careers in a number of dimensions, and we have shown that these penalties can account for almost all of the remaining

gender inequality in earnings. The key question is why the labor market effects of parenthood are so strongly gendered after decades of legislation and policies that are supposed to foster gender equality (and in a country often seen as a leader on this front). Are these differences mostly due to biology or is there an important role for environmental influences? A first step in exploring this question is to document the degree and patterns of heterogeneity. The presence of large heterogeneity by itself speaks against the idea that child-related inequality is *only* about biology and lend support to the role of environmental influences. Moreover, the specific patterns of heterogeneity provide suggestive evidence of mechanisms, although our descriptive analysis of heterogeneity does not identify causal effects as such.

One way of analyzing heterogeneity would be to consider each dimension of interest separately. The problem with such a strategy is that many of the interesting variables are highly correlated, making it hard to draw meaningful conclusions when considering them one at a time. As an example take the one dimension of heterogeneity we have already considered, namely heterogeneity in the child penalty by the number of children shown in Figure A.I. We saw there that the child penalty in earnings is strongly increasing in the number of children, with a roughly 10pp increase in the earnings penalty per child. Yet, having more children is correlated with many other parental variables that could influence the size of child penalties such as the age at first birth, education, occupation, income level, and so on. To properly analyze the patterns of heterogeneity in child penalties and shed light on potential mechanisms, we leverage the granularity and comprehensiveness of the Danish administrative data to document how the child penalty correlates with different dimensions of interest—for example, the number of children, the relative skills of the parents, and the work environment of the mother at the time of birth—holding constant all other potential determinants of child penalties in the data.[5]

The analysis is executed by regressing family-level penalties Δp_i defined in equation (3) on a rich set of non-parametric controls as specified in equation (4). Although conceptually similar to the aggregate-level penalties estimated in the previous section, the family-level penalties differ slightly in their definition. Instead of computing the penalty by comparing post-birth outcomes to the outcome at event time -1, the family-level penalties compare post-birth outcomes to the

[5]Of course, the family characteristics we consider are not randomly allocated, but may be partly driven by selection on unobserved preferences over careers and children. While this serves as a warning against causal interpretation, it is worth noting that the sharp event studies presented above suggest that the full effect of children come in part "as a surprise" to parents. Anticipation effects appear to be relatively limited, while immediately after child birth we see sharp changes in a multitude of dimensions. If the full implications of children come as a surprise, the dimensions that correlate with child penalties may well come as a surprise too. This would mitigate the concern that the heterogeneity we document is mainly driven by selection.

average outcome between event time -5 and -1. This minimizes the noise in family-level penalties introduced by potential mean-reversion in labor market outcomes at the individual level. Note also that if the mother or the father have zero earnings in all of the five years prior to child birth, then the family-level penalty cannot be computed. Finally, our measure of the family-level penalty is unbounded, which may create problems with extreme outliers. We address this concern in two ways. First, in our baseline specification we simply exclude penalties over 400% and under -400%. Second, we also consider results from robust regressions using a Huber M-estimator, which imposes weights on observations so as to reduce the influence of outliers.[6]

The full set of results on heterogeneity in earnings penalties, hours penalties, and wage rate penalties are presented in appendix tables A.I-A.III, while a subset of these results are presented in Figure 6. The four panels in the figure display coefficient estimates from the specification in column (2) of the appendix tables, which corresponds to the specification shown in equation (4).

In panel A of Figure 6, we plot the coefficient estimates of the effect on child penalties of the total number of children that the woman has. The reference is having one child only. Consistent with the evidence in Figure A.I, earnings penalties increase steadily with the number of children and in fact the size of the effect is roughly unaffected (slightly smaller) by the rich set of covariates. Interestingly, the earnings effect is driven by both hours penalties and wage rate penalties, and in roughly equal proportions. These results confirm that larger families go hand-in-hand with lower career trajectories for women relative to men.

In panel B, we investigate how child penalties correlate with the relative wage rates that the mother and father had prior to child birth. Specifically, we compute for each family the average wage rate of the woman and the man w_i^w, w_i^m between event years -5 and -1, and rank families by deciles of the distribution of relative wage rates w_i^w / w_i^m.[7] Because we are including a rich set of controls for relative years of education, profession, and experience, the relative wage rate is meant to capture relative earnings abilities within the family. The reference category in the regression is the first decile of the relative wage rate distribution, i.e. families in which the earnings ability of the woman relative to the man is the smallest. The results show that the earnings penalty is strongly *increasing* in the earnings ability of the mother relative to the father. The 30% of women who are "primary earners" (according to their wage rate) prior to giving birth face an earnings penalty up

[6] Quantile regressions would have been a natural alternative, but they proved too computationally demanding with so many indicator variables and such a large sample.

[7] By averaging individual wage rates over five years, we mitigate the potential concern about short-term mean reversion in wage rates at the individual level.

to 10pp larger than women who are secondary earners. The larger earnings penalties on high-skill women are driven by much larger wage rate penalties, while there is an offsetting effect from smaller hours penalties. The appendix tables A.I-A.III show that these qualitative results are very robust to different sample definitions and specifications.[8]

These descriptive findings show that women at the top of the distribution face the hardest trade-offs between career and family, broadly consistent with evidence from the US by Wilde *et al.* (2010) and Bertrand *et al.* (2010). The fact that the *hours* penalty is declining in the relative skill of the woman is consistent with the presence of a comparative advantage channel, but this effect is swamped by other factors that create larger wage rate and earnings penalties. The evidence in section 4.2 on the effect of motherhood on occupational rank, sector, and firm provides insight into how high-skill women who face increased demands at home may reduce the intensity of their careers in a way that creates large earnings penalties.

Panel C shows that child penalties correlate strongly with the work environment of the woman at the time of having her first child (at event time -1). Working in the public sector is associated with a 10pp smaller penalty in earnings, driven mostly by a lower penalty in total hours. Furthermore, working in a relatively family-friendly firm—proxied as above by the fraction of women with young children in the firm—is also associated with substantially smaller earnings and hours penalties. Note here that selection on unobservables is most likely to go against the effects we find. In particular, women with relatively strong preferences for family over career (those who would face larger child penalties other things equal) are more likely to select into family-friendly work environments ex ante, which by itself would increase observed child penalties in such environments.

Finally, panel D shows the evolution of the child penalty in earnings across birth cohorts, relative to the 1985 birth cohort and controlling for our rich set of covariates. While the conditional earnings penalty is overall declining with birth cohort, it also exhibits significant cyclicality. The recession years following the 1987 financial crisis were associated with larger child penalties than during boom periods. This evidence of cyclicality is consistent with the raw evidence on cohort-specific penalties presented in Figure A.IV.

[8] In tables A.I-A.III, we also investigate how child penalties correlate with relative years of education (pre-birth) within the family. Conditional on all the other controls, relative years education within the family have only a modest and marginally significant effect on child penalties.

5.2 Intergenerational Transmission of Child Penalties

The size and persistence of female child penalties, along with their heterogeneity across skill, are difficult to reconcile with comparative advantage alone. This suggests that the effects are partly driven by preferences over what men and women should do when becoming parents. Indeed, we have shown in Figure 2 that views on the appropriate gender roles in families with children are very conservative in all countries. The vast majority of both men and women hold the view that women should not work full-time as long as there are children living at home. This raises the question of where these gendered preferences are coming from? Are they biologically determined or is there a role for environmenal influences? In this section we present a set of findings that speak in favor of environmentally determined gender preferences.

A recent literature discusses the importance of social norms and gender identity in explaining the different labor market outcomes of men and women, although causal testing of these ideas has proved difficult (see Bertrand 2011). We explore the role of such influences by showing that the female child penalty is strongly related to the work history of her parents, but not to the work history of her partner's parents. Our findings are consistent with the idea that a woman's preferences over family and career are shaped during her childhood. Our analysis is related to Fernandez *et al.* (2004), but they consider a transmission mechanism between the woman and the parents of the man (where we find no effect). Our approach also differ in that we consider the intergenerational transmission of child penalties—i.e., labor supply changes of women relative to men around child birth—rather than the intergenerational transmission of labor supply levels. Working with such labor supply changes directly takes care of some of the key omitted variable concerns encountered when working with labor supply levels.

Our analysis leverages the availability of the administrative ATP measure of hours since 1964. Specifically, for each family i we observe the cumulative sum of all recorded ATP hours between 1964-1979 of the mother's mother h_i^{mm} and of the mother's father h_i^{mf}. To capture the relative labor supply of the maternal grandparents, we rank families in deciles of the distribution of the difference $h_i^{mf} - h_i^{mm}$. We do the same for the relative labor supply of the paternal grandparents $h_i^{ff} - h_i^{fm}$.

Panel A and B of Figure 7 start by plotting the average family-level child penalties in earnings and hours by deciles of the relative labor supply of the maternal grandparent's (relative to first decile). They reveal a very strong and significant correlation between the child penalty in earnings and hours on the mother and the relative labor supply of the maternal grandparents. In families

in the top decile of the relative labor supply of maternal grandparents, i.e. where the grandmother worked very little compared to the grandfather, the mother pays an earnings penalty that is 10pp larger and an hours penalty that is 7pp larger than in families from the bottom decile.

There are two potential concerns in the interpretation of these (unconditional) intergenerational correlations. First, rather than reflecting a transmission of gendered preferences, they could be driven by other transmissible characteristics of the maternal grandparents that correlate with child penalties in every generation. The prime candidates are education and income/wealth levels. Second, while our cumulative measure of hours for grandparents between 1964-1979 has the advantage of minimizing noise compared to snapshots of cross-sectional data, part of the variation in this measure might be driven by grandparents in different cohorts being observed at different points in the lifecycle.

To investigate the robustness of our findings to these potential concerns, we adopt a methodology similar to section 5.1 by exploring the effect of the relative labor supply of grandparents, conditional on a rich set of socio-demographic controls for the maternal grandparents. We include in the regression a complete set of dummies for the birth cohort of the grandmother and the grandfather, which will control for observing grandparents at different points in the lifecycle. We also include detailed controls for the education of both the grandfather and the grandmother, with twenty-two dummies for each grandparent that capture education level and field. We finally 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 deciles of the within-generation wealth rank of the grandfather.[9] The results, displayed in panels C and D of Figure 7, confirm that these intergenerational correlations are very robust to controlling for the characteristics of grandparents.

Interestingly, when we replicate the same exercice using paternal grandparents in appendix Figure A.V, we find no significant relationship between grandparents' work history and female child penalties. This differential pattern is interesting for two reasons. First, it is consistent with child penalties being driven by female gender identity formed during her childhood, as opposed to child penalties being driven by male gender identity formed during his childhood. This makes sense when considering the event study evidence presented above. The effects in those event studies are entirely driven by sharp changes in the behavior of women, with men being essentially unaffected by parenthood, and so it makes sense that the underlying reason for such behaviors

[9]This comprehensive wealth information on the universe of Danish taxpayers was collected for the purpose of the wealth tax.

should be sought in the woman's childhood environment, not the man's. Second, the differential pattern of intergenerational correlations between maternal and paternal grandparents rules out the treat from omitted variables that are present in both sets of grandparents, i.e. family background variables (not captured by the detailed education and wealth controls) that both parents have and which affect child penalties.

Overall, these findings are pointing to an influence of nurture in the formation of gender attitudes regarding children and career. They suggest the existence of heterogeneous preferences or norms across families regarding gender roles in the labor market and in the home, preferences that are transmitted in the family across generations, and differentially between daughters and sons. Such intergenerational transmission mechanisms may play an important role in the persistence of child penalties on women over time.

5.3 Parental Leave Policy: Part of the Solution or Part of the Problem?

As described in section 2.1 Denmark has been at the forefront of implementing family policies that offer job-protected and paid maternity and parental leave. An important question is whether such policies are helpful for reducing gender inequality or if they are counterproductive. While an evaluation of the long-run effect of such policies on gender inequality is outside the scope of this paper, in this section we provide evidence that the take-up of gender-neutral family leave policies is extremely unequal across gender. Our findings here are consistent with the evidence above that preferences over family vs career are strongly gendered, and this raises potential concerns with respect to the effect of family leave policies on the gender gap.

To study this question we consider a 2002 reform, which extended parental leave from 26 weeks to 52 weeks. Apart from the introduction of 4 weeks of pregnancy leave, the rest of the expansion took the form of parental leave that could be freely allocated between the two parents. Hence the reform did not directly change the relative prices of leave taken for women relative to men. However, if preferences over family vs career are strongly gendered as we have shown above, then a gender-neutral provision of parental leave may effectively subsidize leave taken by woman and this could potentially excaserbate gender inequality.

Our findings are presented in Figure 8. Panel A shows the amount of parental leave taken by men and women around the 2002 reform, relative to 2001. We see a very sharp increase in parental leave by women of about 15 weeks on average and only a tiny increase in parental leave by men. The difference-in-differences estimate of the effect on parental leave for women relative to men is

14.4 weeks.

Panel B considers heterogeneity in treatment effects by skill. This panel plots the difference in parental leave between men and women (still relative to 2001) by relative skill within the family prior to child birth. Specifically, we split the sample into two groups depending on whether the woman has a lower wage rate than her partner prior to giving birth (in which case we label her the "secondary earner") or a higher wage rate than her partner prior to giving birth (in which case we label her the "primary earner"). Two key findings emerge from the figure. First, the increase in parental leave by women relative to men is smaller when she is the primary earner prior to birth, consistent with a comparative advantage channel being in operation. Second, while the differential treatment effect across relative skill levels is clear and statistically significant, it is economically very small. Even in families where the woman is the higher-skill person, she takes an extra 12.7 weeks of parental leave compared to the man when it is offered to both of them. Note that the additional leave comes on top of the 26 weeks already offered prior to the reform, and so the effects are not easily explained by comparative advantage in infant care. In other words, the effect of comparative advantage in the market place is there, but it is swamped by other factors that make women the prime caregivers to children.

While these findings do not identify the long-run effect of family policies on gender inequality (or welfare), they do raise some questions regarding the desirability of gender-neutral family leave policies in a world where preferences are extremely gendered (possibly because of environmental preferences as analyzed above). Future work will investigate further the link between these policy effects and the long-run child penalties and gender gaps analyzed above.

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 childrearing. We have presented three main sets of results.

First, we have identified large child penalties on the careers of women relative to men in a broad range of dimensions. The female child penalty in earnings is around 20% even 20 years after the birth of the first child. Underlying this effect, we find sharp effects of children on labor force participation, hours of work, 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, by estimating child penalties in earnings in each year after birth and for each birth cohort, we have decomposed gender inequality into what can be attributed to children and and what is driven by other factors (such as human capital or discrimination). We have shown that the fraction of total gender inequality that can be explained by children has increased dramatically over time, from 30% in 1980 to 80% in 2011. In other words, to a first approximation, the remaining gender inequality is all about children. This has important implications for future work on gender inequality, which should focus on understanding what drives gender outcomes in relation to parenthood.

Third, we have provided evidence in favor of environmental influences in the formation of gendered preferences over family vs career. In particular, we have shown that the female child penalty is strongly related to the work history of her parents: for example, women who grow up in traditional families with a male breadwinner and a female homemaker end up paying much larger career penalties when they become mothers themselves. At the same time, the female child penalty is unrelated to the work history of her partner's parents. 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. We have argued that the differential pattern between maternal and paternal grandparents is consistent with the fact that the career effects of children are entirely driven by sharp changes in the behavior of women, not men, and so it is natural that the underlying reason for such behaviors should be sought in women's childhood environment.

While these patterns of intergenerational transmission of child penalties are suggestive of environmental influences on the strongly gendered preferences over family vs career, conclusive causal testing is obviously challenging due to the fact that family background is not randomly allocated. Future work should continue to study the underlying mechanisms as this will ultimately determine the welfare and policy implications of the patterns we have uncovered here.

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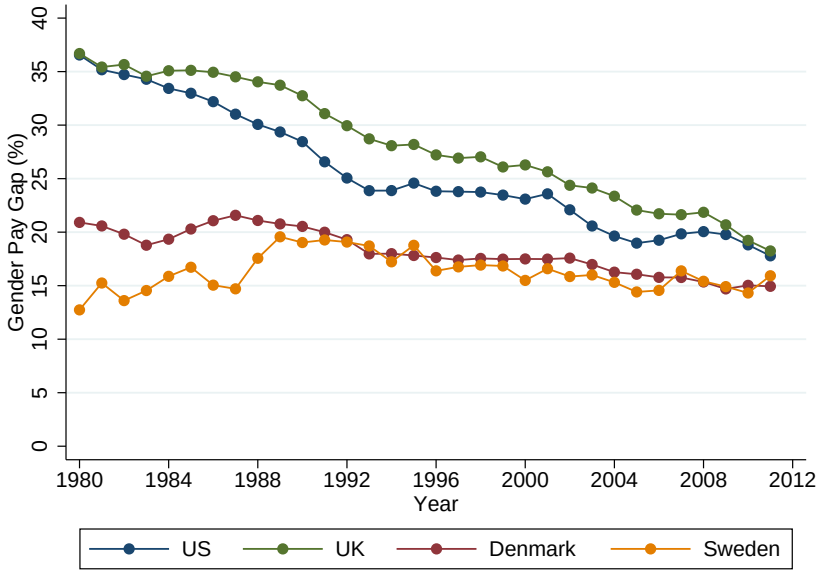
Figure 1: Gender Gaps Across Countries 1980-2011

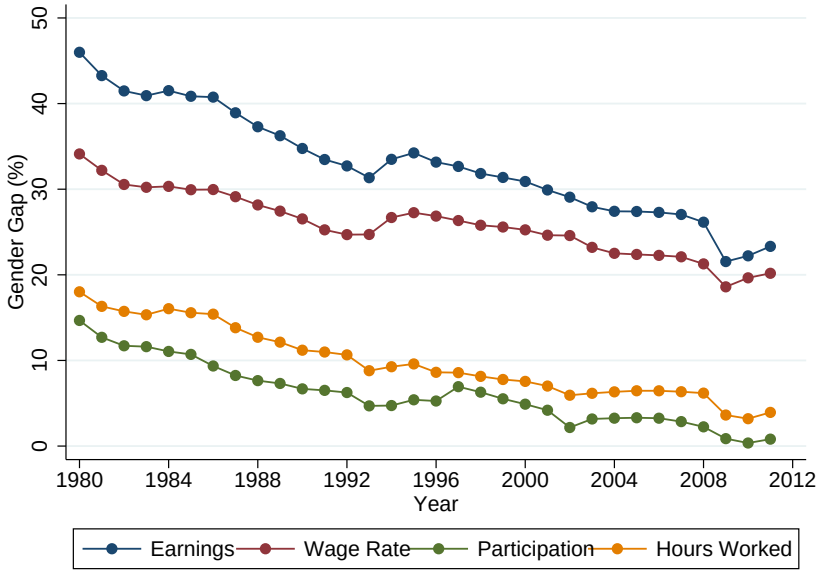
A: Convergence of the Gender Pay Gap Across Countries

Median Earnings for Full-Time Workers



B: Evolution of Gender Gaps in Denmark

Means for All Workers

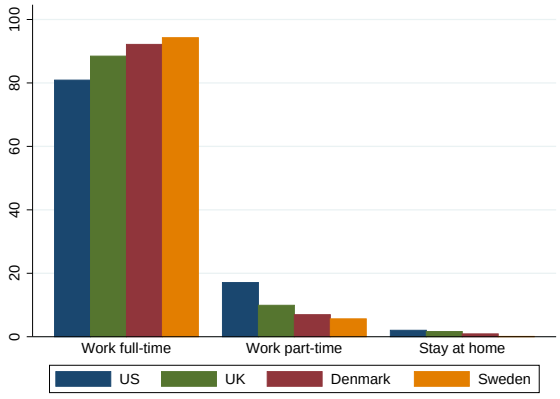


Notes: The time series in Panel A are drawn from OECD.org, except for Denmark where we use our own calculation of median earnings for full-time workers aged 16-64 (as defined by the ATP hours measure described in section 2.2). Our calculation for Denmark uses the same underlying data as the official 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 among the entire population aged 16-64 regardless of employment status, while the gaps in hours worked and the wage rate is calculated conditional on participation (ATP hours > 0).

Figure 2: Gender Norms Across Countries

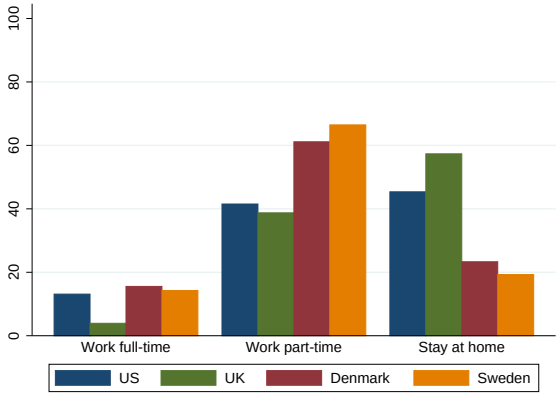
A: Women Without Children

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?



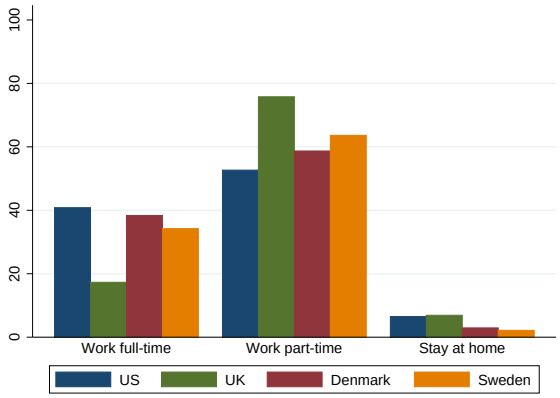
B: Women With Children Under School Age

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?



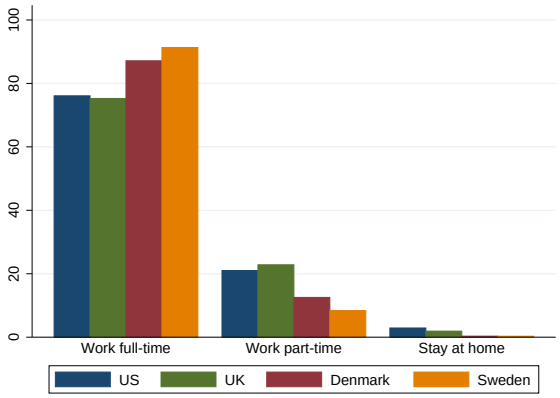
C: Women With Children In School

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?



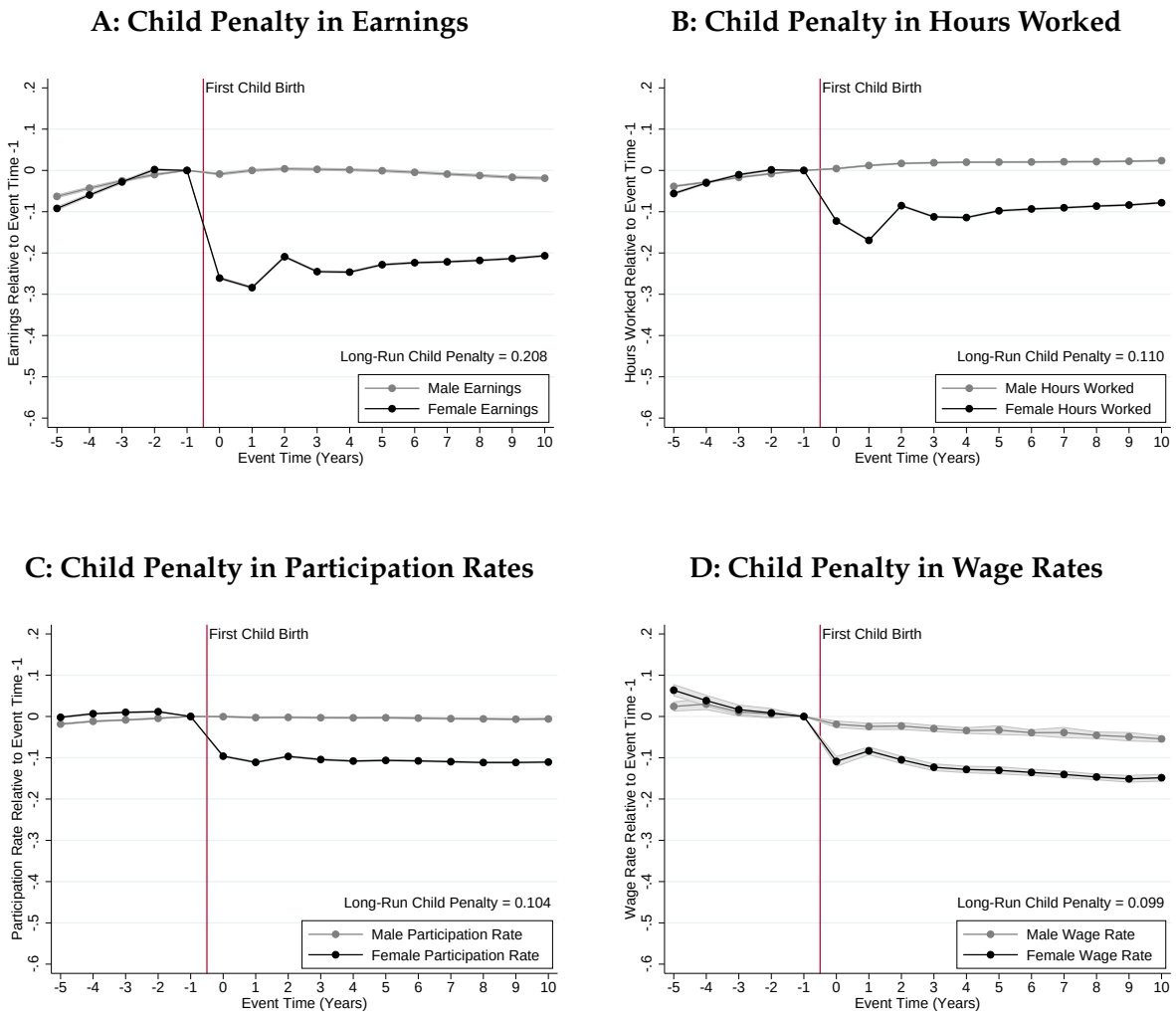
D: Women With Children Who Have Left Home

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 3: Child Penalties on Women

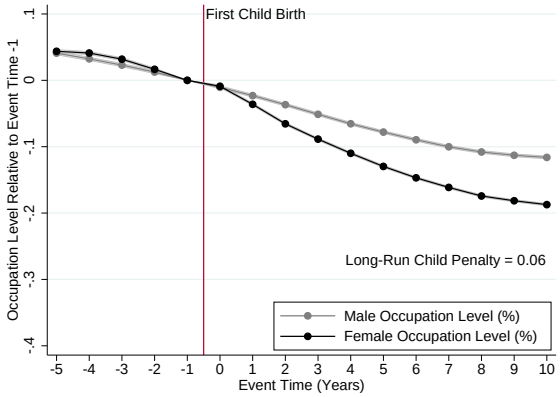


Notes: The panels show estimated coefficients on the event time dummies in equation (1) as a fraction of the predicted outcome when omitting the contribution from the event dummies (i.e., $P_t^k = \hat{\alpha}_t^k / E[\hat{Y}_{ist}^k|t]$ defined in section 3). The coefficients are estimated on a balanced sample of parents, who have their first child between 1985-2001 and who we observe in the data during the entire period between 5 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 the wage rate are estimated conditional on participation (ATP hours > 0). The shaded 95 % confidence intervals are based on robust standard errors.

Figure 4: Anatomy of Child Penalties

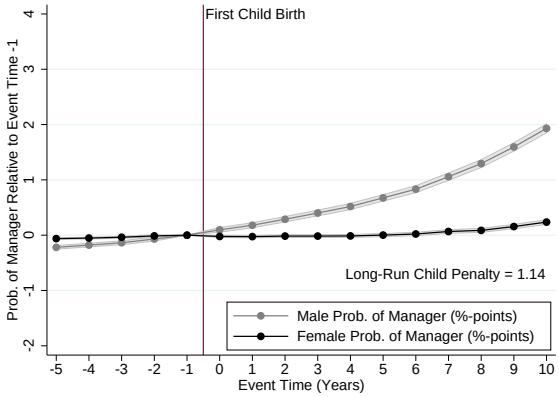
A: Occupational Rank

Levels 1-5 from Unskilled Labor to Manager



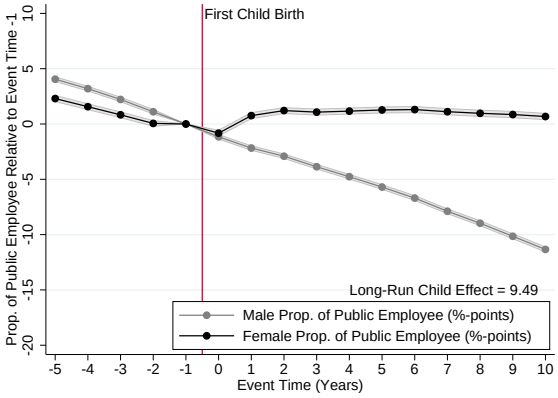
B: Probability of Being Manager

Manager Dummy



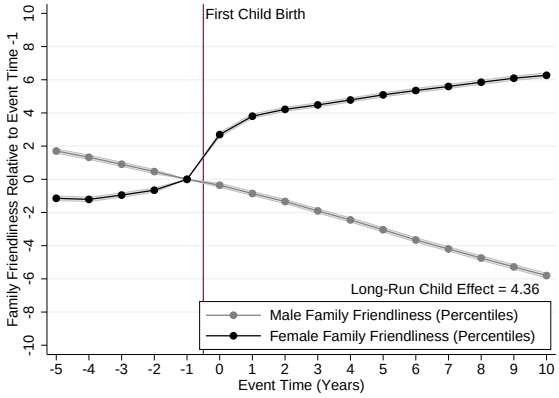
C: Probability of Public Sector Job

Public Sector Dummy



D: Family Friendliness of Firm

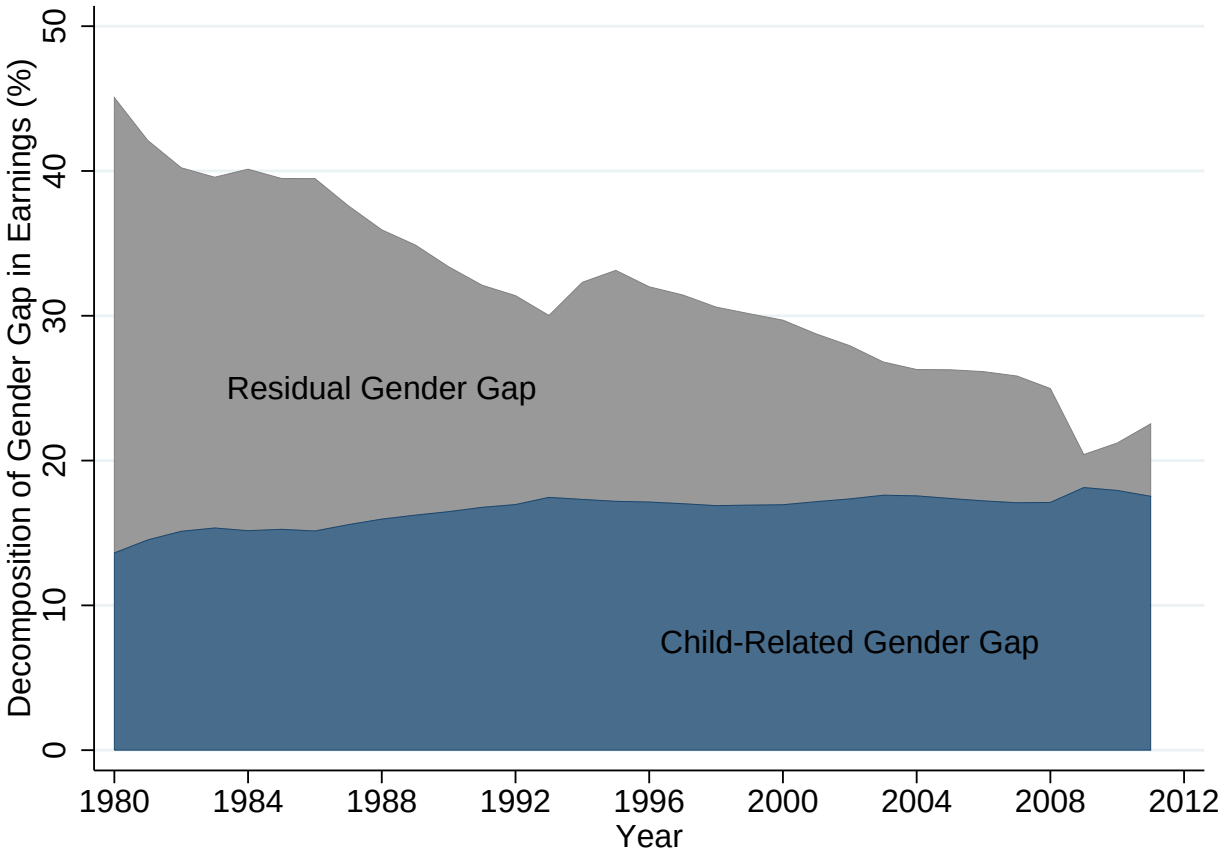
Share of Women with Young Children in the Firm



Notes: The panels show the estimated coefficients on the event time dummies in equation (1) as a fraction of the predicted outcome when omitting the contribution from the event dummies (similar to Figure 3). The effect on occupational rank is estimated conditional on not being self-employed or an assisting spouse. The effect on the probability of being employed in the public sector is estimated conditional on having a recorded sector variable. The effect of the family friendliness of the firm (i.e., share of women with young children—defined as children below age 15—in the firm) is estimated conditional on being in a firm with more than 10 employees. Moreover, the share of women with young children in a given woman's firm is calculated excluding the woman's own child (jack-knifed). The long-run child penalty/effect is calculated as the average effect for women relative to men over the period from event time 5 to 10. The long-run effect on the family friendliness of the firm takes into account the differential pre-trend between men and women.

Figure 5: Decomposing the Gender Gap Over Time

Child-Related Earnings Gap vs Non-Child Earnings Gap

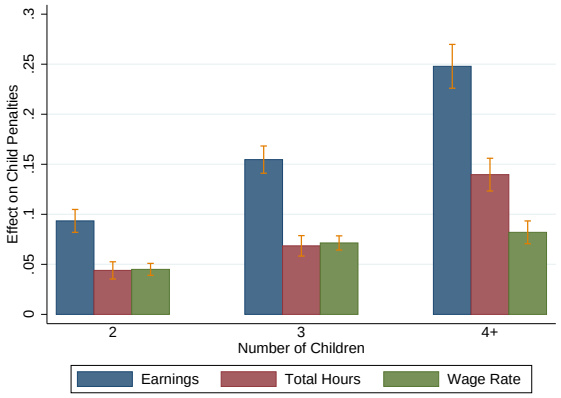


Notes: The gender gap in earnings is calculated using the entire population aged 16-64 regardless of employment status, and so the total gap (residual + child-related) corresponds to the earnings gap shown in Figure 1B. The decomposition of the total gap is based on the methodology developed in section 4.3. The residual gap is based on female earnings adjusted for effect of children using the estimated child penalties for each calendar and event year, namely earnings $\hat{Y}_{ist} = Y_{ist} / (1 - \Delta P_{st})$ where Y_{ist} is actual earnings and $\Delta P_{st} = P^m_{st} - P^w_{st}$ is the child penalty in calendar year s and event time t. The cohort-specific penalties ΔP_{st} are estimated based on equation (5) on a semi-balanced panel, with linear extrapolation to cover cohorts outside our data period as described in section 4.3.

Figure 6: Heterogeneity in Child Penalties

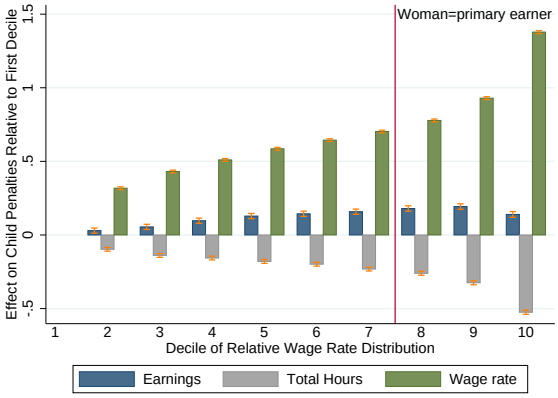
A: Number of Children

Total Fertility of 2, 3, or 4+



B: Relative Skill of Parents

Decile of Relative Wage Rate Distribution

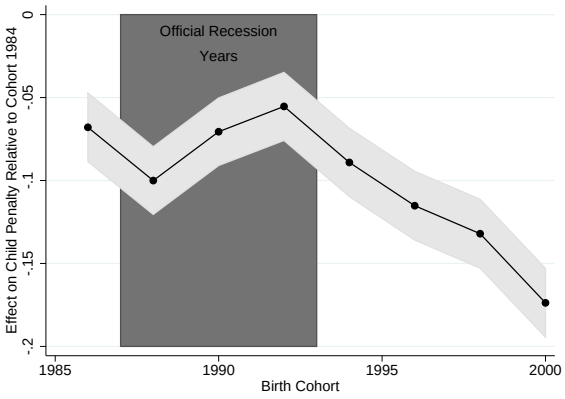


C: Work Environment of the Mother

Public Sector and Family Friendliness of Firm

D: Birth Cohort

Year of the First Child Birth

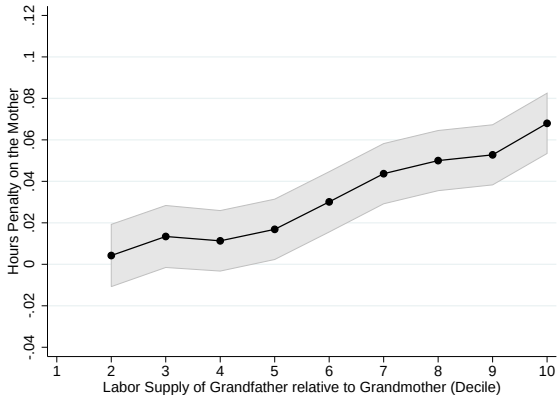


Notes: These panels show coefficient estimates (with robust standard errors) based on specification (4) and correspond to the estimates reported in column (2) of the appendix tables A.I-A.III. The dependent variable in this regression is the family-level penalty defined in equation (3). The total number of children is measured as of 2013, which leaves at least a 12-year window to have extra children as we consider first child births between 1985-2001. The relative wage rate of the parents in a family is based on an average over event time -5 to -1. The work environment of the woman (sector and firm) is measured at event time -1.

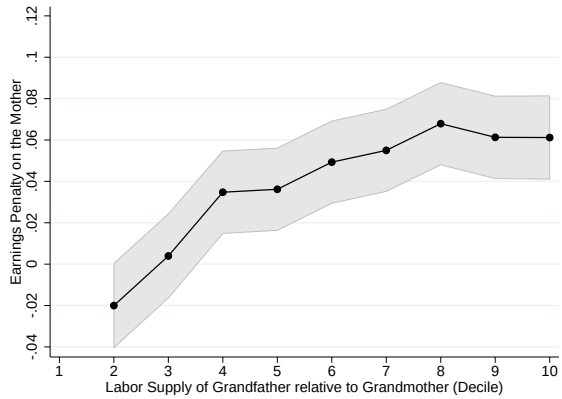
Figure 7: Intergenerational Transmission of Child Penalties (Maternal)

A: Earnings Penalties (Unconditional)
Effect of Maternal Grandparents' Work History

B: Hours Penalties (Unconditional)
Effect of Maternal Grandparents' Work History



C: Earnings Penalties (Conditional)
Effect of Maternal Grandparents' Work History



D: Hours Penalties (Conditional)
Effect of Maternal Grandparents' Work History

Notes: The panels show the correlation between family-level child penalties (as defined in equation (3)) and the work history of the maternal grandparents, specifically total hours worked by the maternal grandfather relative to the maternal grandmother. Relative hours worked by the grandparents is based on the cumulative ATP contributions over the period 1964-79. Panels A-B show correlations without any controls, while panels C-D show correlations conditional on a rich set of socio-economic characteristics of the (maternal) grandparents, including their birth cohort, education level/fields, and within-generation wealth rank.

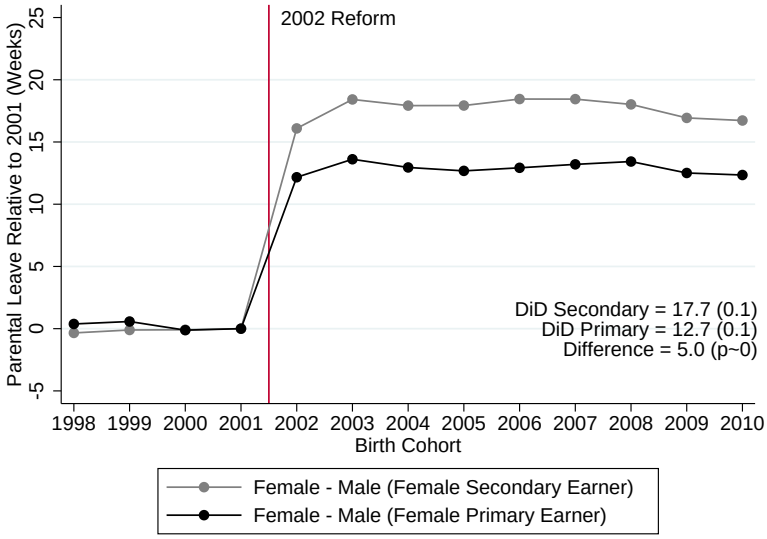
Figure 8: Effects of Job-Protected Parental Leave

A: Effect of Parental Leave Extension

Parental Leave of Males and Females around Reform

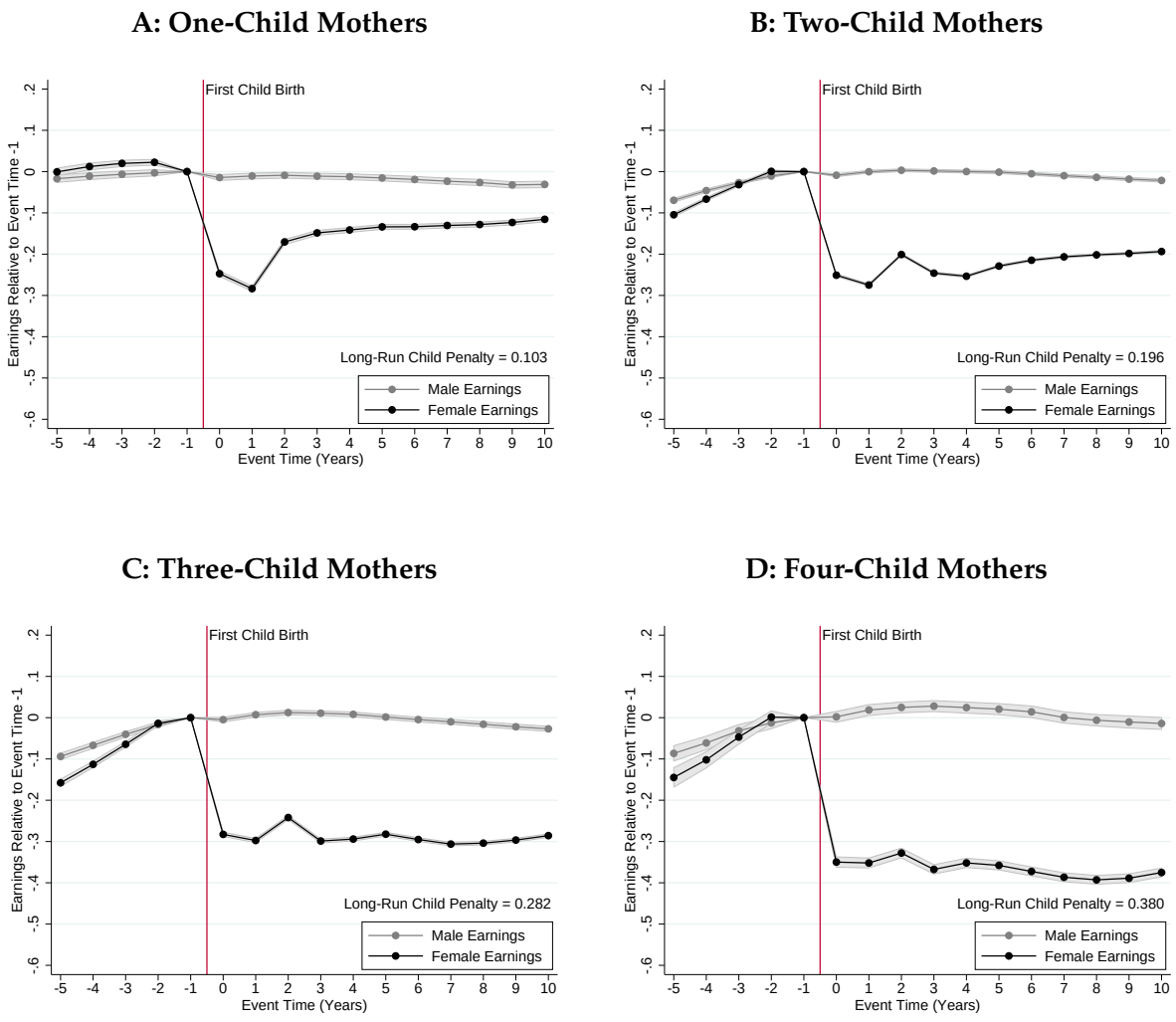
B: Effect of Parental Leave Extension by Skill

Parental Leave of Females − Males by Relative Wage Rates



Notes: Panel A shows weeks on parental leave (relative to the level in 2001) for males and females, respectively, around the 2002 reform, which extended the total length of job-protected and paid parental leave from 24 to 50 weeks. Panel B considers the difference in parental leave between males and females (still relative to 2001) and splits the sample into two groups depending on whether the female has a lower or higher wage rate than the male in the year prior to child birth (labelled as the female being the secondary and primary earner, respectively, in the panel).

Figure A.I: The Child Penalty in Earnings by Number of Children

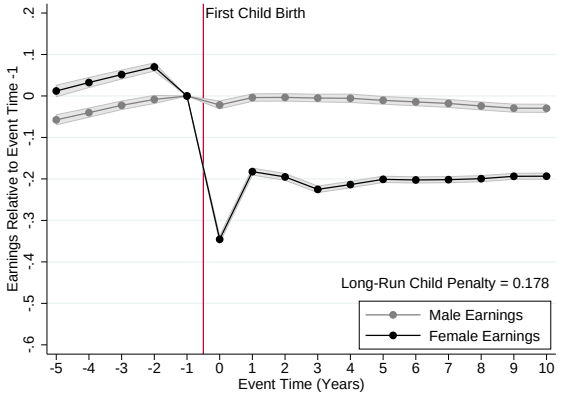


Notes: The panels show estimated coefficients on the event time dummies in equation (1) as a fraction of the predicted outcome when omitting the contribution from the event dummies (i.e., $P_t^k = \hat{\alpha}_t^k / E[\hat{Y}_{ist}^k|t]$ defined in section 3). The focus is on the earnings penalty as in Figure 3A, splitting the sample by the woman's total number of children as of 2013 (1, 2, 3, or 4 children). The long-run child penalty is calculated as the average effect for women relative to men over the period from event time 5 to 10.

Figure A.II: Female Child Penalties for January Births

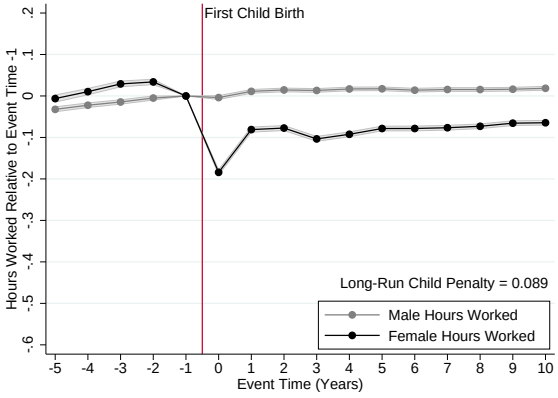
A: Child Penalty in Earnings

January Births



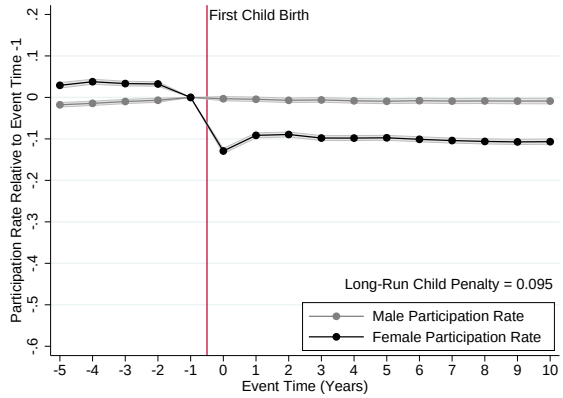
B: Child Penalty in Hours Worked

January Births



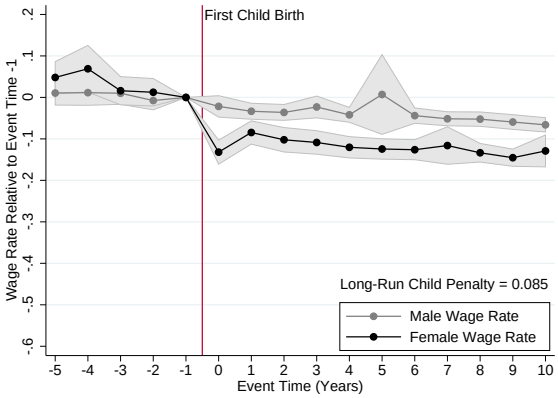
C: Child Penalty in Participation Rates

January Births



D: Child Penalty in Wage Rates

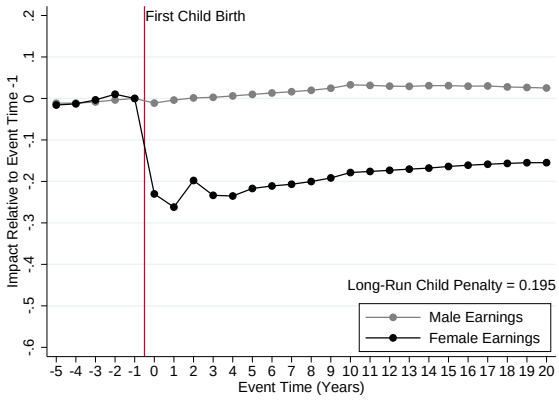
January Births



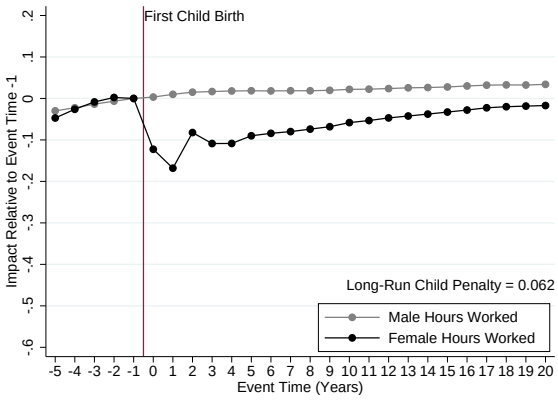
Notes: This figure is constructed exactly as Figure 3 except that the estimation is run only for those who have their first child in January.

Figure A.III: Female Child Penalties Over a 20-Year Horizon

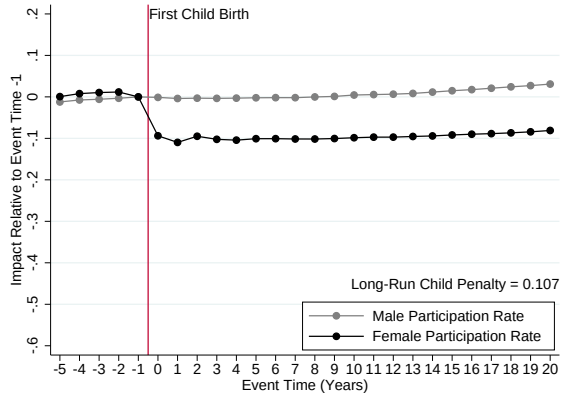
A: Child Penalty in Earnings
20 Years After Child Birth



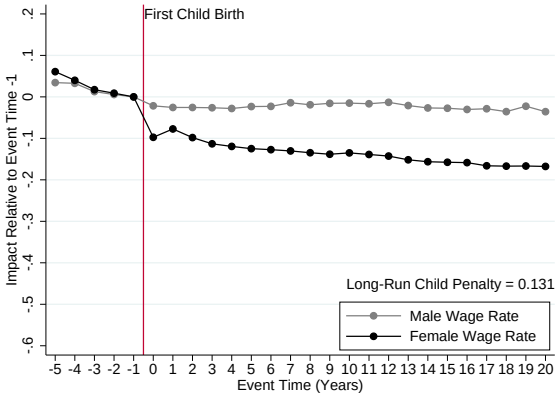
B: Child Penalty in Hours Worked
20 Years After Child Birth



C: Child Penalty in Participation Rates
20 Years After Child Birth



D: Child Penalty in Wage Rates
20 Years After Child Birth

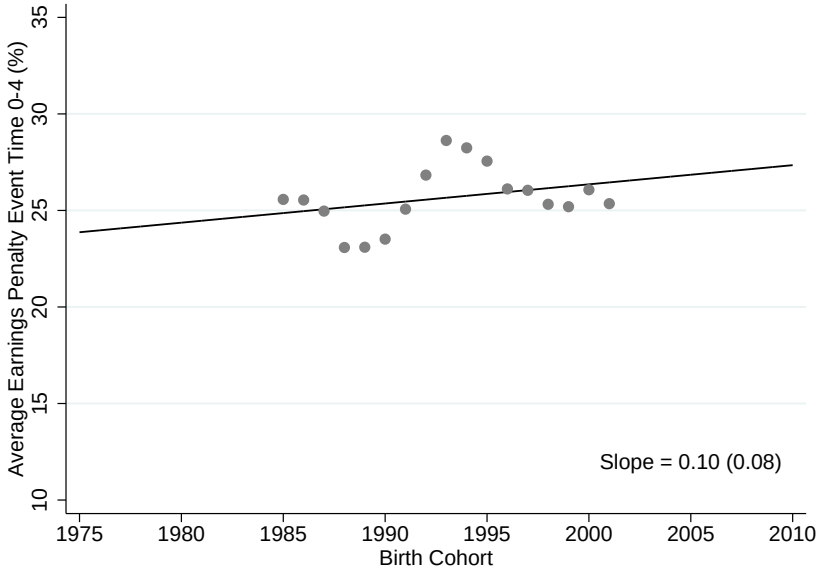


Notes: The panels are based on the regression specification in (5) and a semi-balanced panel running from event time -5 to 20. Only parents who have their first child between 1985-1991 are observed in all 26 event years; parents who have their first child after 1991 are kept in the sample for the maximum possible number of event years. Given the long event time window and the interaction with year dummies, the specification includes more than 800 dummies and in order to reduce the computational requirements we aggregate the micro data into year-age-event time cells and run the regressions on this data weighted by the number of observations in each cell. The panels show average estimated coefficients across birth cohorts 1985-1991 (the cohorts observed over the entire 26-year event time window) as a fraction of the average predicted outcome when omitting the contribution from the event dummies, similar to the estimates reported in Figure 3.

Figure A.IV: Earnings Penalties by Birth Cohort and Extrapolation

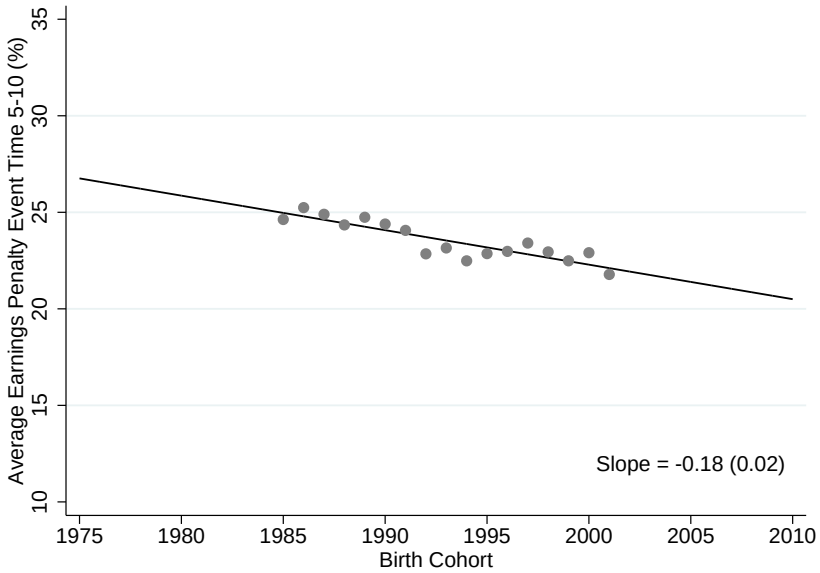
A: Average Earnings Penalty From Event Time 0-4

No Significant Time Trend



B: Average Earnings Penalty From Event Time 5-10

Significant Negative Time Trend

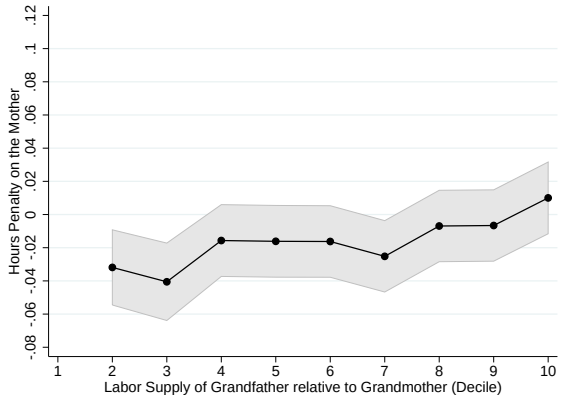


Notes: The panels show cohort-specific child penalties estimated based on (5). Panel A shows the average penalties over event time 0-4, while Panel B shows the average penalties over event time 5-10. Each panel also shows a linear OLS fit and the estimated slope coefficient (with standard errors in parentheses). There is no statistically significant trend in the short-run penalty, but there is a statistically significant downward trend in the long-run penalty.

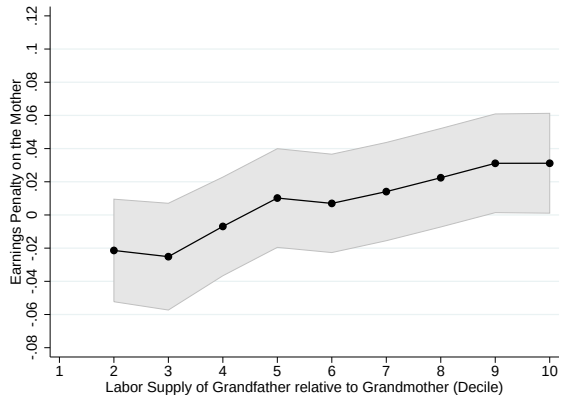
Figure A.V: Intergenerational Transmission of Child Penalties (Paternal)

A: Earnings Penalties (Unconditional)
Effect of Paternal Grandparents' Work History

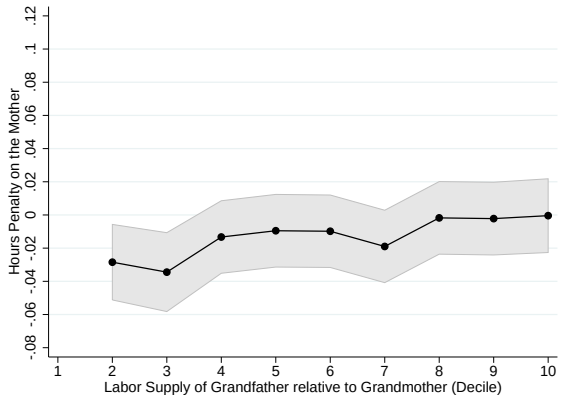
B: Hours Penalties (Unconditional)
Effect of Paternal Grandparents' Work History



C: Earnings Penalties (Conditional)
Effect of Paternal Grandparents' Work History



D: Hours Penalties (Conditional)
Effect of Paternal Grandparents' Work History



Notes: The panels show the correlation between family-level child penalties (as defined in equation (3)) and the work history of the paternal grandparents, specifically total hours worked by the paternal grandfather relative to the paternal grandmother. Relative hours worked by the grandparents is based on the cumulative ATP contributions over the period 1964-79. Panels A-B show correlations without any controls, while panels C-D show correlations conditional on a rich set of socio-economic characteristics of the (paternal) grandparents, including their birth cohort, education level/fields, and within-generation wealth rank.

Table A.I: Heterogeneity in Family-Level Earnings Penalties

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Robust regression
			Conditional on graduation before birth	Conditional on working full time before birth	
	Number of children				
Two children	0.0898***	0.0946***	0.103***	0.104***	0.0970***
	(0.00572)	(0.00557)	(0.00567)	(0.00560)	(0.00530)
Three children	0.155***	0.170***	0.196***	0.209***	0.181***
	(0.00681)	(0.00664)	(0.00699)	(0.00695)	(0.00632)
Four children and +	0.259***	0.269***	0.315***	0.343***	0.293***
	(0.0108)	(0.0105)	(0.0120)	(0.0125)	(0.0100)
	Work environment: Family friendliness and sector				
Quartile 2	-0.0000305	-0.000772	-0.0184*	-0.0302***	-0.0105
	(0.00738)	(0.00719)	(0.00750)	(0.00749)	(0.00684)
Quartile 3	-0.0134	-0.0143*	-0.0413***	-0.0527***	-0.0293***
	(0.00702)	(0.00684)	(0.00716)	(0.00717)	(0.00651)
Quartile 4	-0.0787***	-0.0624***	-0.0600***	-0.0660***	-0.0705***
	(0.00768)	(0.00748)	(0.00792)	(0.00797)	(0.00712)
Public sector	-0.112***	-0.0649***	-0.0345***	-0.0354***	-0.0701***
	(0.00508)	(0.00500)	(0.00530)	(0.00528)	(0.00476)
	Relative years of education				
Quartile 2	-0.00963	0.00961	-0.00164	-0.00813	0.0106
	(0.00871)	(0.00850)	(0.00904)	(0.00888)	(0.00809)
Quartile 3	-0.0314***	-0.00560	-0.00762	-0.00930	0.00461
	(0.00910)	(0.00889)	(0.00935)	(0.00920)	(0.00846)
Quartile 4	-0.0815***	-0.0375**	-0.0396**	-0.0425**	-0.0242
	(0.0146)	(0.0142)	(0.0149)	(0.0146)	(0.0135)
	Deciles of relative wage rate distribution				
Decile 2	0.0299***	0.00849	0.0409***	0.0439***	0.0225**
	(0.00868)	(0.00845)	(0.00904)	(0.00905)	(0.00804)
Decile 3	0.0770***	0.0306***	0.0842***	0.0848***	0.0453***
	(0.00867)	(0.00846)	(0.00909)	(0.00909)	(0.00805)
Decile 4	0.124***	0.0528***	0.115***	0.116***	0.0668***
	(0.00870)	(0.00850)	(0.00913)	(0.00913)	(0.00809)
Decile 5	0.145***	0.0525***	0.128***	0.125***	0.0702***
	(0.00875)	(0.00857)	(0.00917)	(0.00919)	(0.00816)
Decile 6	0.176***	0.0681***	0.153***	0.153***	0.0836***
	(0.00880)	(0.00864)	(0.00920)	(0.00923)	(0.00822)
Decile 7	0.197***	0.0781***	0.165***	0.165***	0.0965***
	(0.00886)	(0.00871)	(0.00926)	(0.00931)	(0.00829)
Decile 8	0.194***	0.0686***	0.178***	0.176***	0.0845***
	(0.00989)	(0.00971)	(0.00931)	(0.00939)	(0.00924)
Decile 9	0.234***	0.0912***	0.214***	0.224***	0.113***
	(0.00914)	(0.00900)	(0.00941)	(0.00958)	(0.00857)
Decile 10	0.175***	0.0326***	0.230***	0.291***	0.0569***
	(0.00976)	(0.00959)	(0.00986)	(0.0105)	(0.00913)
	Additional controls (full set of dummies)				
Age	×	×	×	×	×
Cohort	×	×	×	×	×
Occupation	×	×	×	×	×
Deciles of household earnings	×	×	×	×	×
Experience		×	×	×	×
N	257469	257469	185872	159595	257469

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.II: Heterogeneity in Family-Level Hours Penalties

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Robust regression
			Conditional on graduation before birth	Conditional on working full time before birth	
	Number of children				
Two children	0.0379***	0.0389***	0.0419***	0.0435***	0.0383***
	(0.00422)	(0.00413)	(0.00408)	(0.00378)	(0.00309)
Three children	0.0627***	0.0706***	0.0851***	0.0963***	0.0780***
	(0.00500)	(0.00490)	(0.00503)	(0.00468)	(0.00366)
Four children and +	0.137***	0.141***	0.161***	0.184***	0.154***
	(0.00788)	(0.00770)	(0.00859)	(0.00837)	(0.00576)
	Work environment: Family friendliness and sector				
Quartile 2	0.0117*	0.0106*	-0.00579	-0.0217***	-0.00628
	(0.00541)	(0.00529)	(0.00539)	(0.00504)	(0.00396)
Quartile 3	0.00319	0.00191	-0.0240***	-0.0366***	-0.0204***
	(0.00515)	(0.00504)	(0.00515)	(0.00483)	(0.00377)
Quartile 4	-0.0668***	-0.0569***	-0.0503***	-0.0544***	-0.0583***
	(0.00564)	(0.00551)	(0.00569)	(0.00537)	(0.00412)
Public Sector	-0.0907***	-0.0573***	-0.0324***	-0.0303***	-0.0510***
	(0.00372)	(0.00368)	(0.00381)	(0.00355)	(0.00275)
	Relative years of education				
Quartile 2	-0.0000864	0.0108	0.0109	0.00391	0.0121**
	(0.00635)	(0.00622)	(0.00647)	(0.00594)	(0.00465)
Quartile 3	-0.0175**	-0.00218	0.00955	0.00402	0.00979*
	(0.00665)	(0.00652)	(0.00669)	(0.00617)	(0.00488)
Quartile 4	-0.0339**	-0.00689	0.00183	-0.00379	0.00302
	(0.0106)	(0.0104)	(0.0107)	(0.00975)	(0.00778)
	Deciles of relative wage rate distribution				
Decile 2	-0.0986***	-0.114***	-0.0586***	-0.0535***	-0.0771***
	(0.00636)	(0.00622)	(0.00651)	(0.00608)	(0.00465)
Decile 3	-0.131***	-0.163***	-0.0641***	-0.0602***	-0.120***
	(0.00636)	(0.00623)	(0.00654)	(0.00611)	(0.00466)
Decile 4	-0.149***	-0.198***	-0.0668***	-0.0624***	-0.144***
	(0.00638)	(0.00626)	(0.00657)	(0.00614)	(0.00468)
Decile 5	-0.178***	-0.242***	-0.0746***	-0.0698***	-0.176***
	(0.00642)	(0.00631)	(0.00659)	(0.00617)	(0.00472)
Decile 6	-0.195***	-0.268***	-0.0794***	-0.0716***	-0.197***
	(0.00645)	(0.00635)	(0.00663)	(0.00621)	(0.00476)
Decile 7	-0.223***	-0.304***	-0.0873***	-0.0773***	-0.221***
	(0.00649)	(0.00640)	(0.00667)	(0.00626)	(0.00479)
Decile 8	-0.255***	-0.340***	-0.0939***	-0.0832***	-0.248***
	(0.00733)	(0.00722)	(0.00670)	(0.00631)	(0.00540)
Decile 9	-0.308***	-0.405***	-0.103***	-0.0851***	-0.298***
	(0.00667)	(0.00659)	(0.00677)	(0.00643)	(0.00493)
Decile 10	-0.505***	-0.603***	-0.179***	-0.124***	-0.457***
	(0.00710)	(0.00701)	(0.00706)	(0.00701)	(0.00524)
	Additional controls (full set of dummies)				
Age	×	×	×	×	×
Cohort	×	×	×	×	×
Occupation	×	×	×	×	×
Deciles of household earnings	×	×	×	×	×
Experience		×	×	×	×
N	267825	267825	190129	162649	267825

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table A.III: Heterogeneity in Family-Level Wage Rate Penalties

	(1) OLS	(2) OLS	(3) OLS	(4) OLS	(5) Robust regression
			Conditional on graduation before birth	Conditional on working full time before birth	
	Number of children				
Two children	0.0513***	0.0523***	0.0518***	0.0506***	0.0482***
	(0.00308)	(0.00308)	(0.00358)	(0.00371)	(0.00256)
Three children	0.0812***	0.0820***	0.0840***	0.0825***	0.0763***
	(0.00364)	(0.00364)	(0.00442)	(0.00460)	(0.00302)
Four children and +	0.0939***	0.0948***	0.101***	0.103***	0.0897***
	(0.00574)	(0.00573)	(0.00766)	(0.00830)	(0.00477)
	Work environment: Family friendliness and sector				
Quartile 2	-0.00379	-0.00408	-0.00632	-0.00146	-0.00865**
	(0.00395)	(0.00394)	(0.00476)	(0.00496)	(0.00328)
Quartile 3	-0.00943*	-0.00974**	-0.0110*	-0.00869	-0.0137***
	(0.00377)	(0.00376)	(0.00454)	(0.00476)	(0.00312)
Quartile 4	0.0143***	0.0175***	0.000989	-0.00134	0.00957**
	(0.00411)	(0.00410)	(0.00502)	(0.00528)	(0.00341)
Public Sector	-0.00278	0.00150	0.00810*	0.00813*	-0.00307
	(0.00270)	(0.00272)	(0.00335)	(0.00350)	(0.00226)
	Relative years of education				
Quartile 2	-0.00985*	-0.00585	-0.0210***	-0.0186**	-0.00323
	(0.00459)	(0.00459)	(0.00565)	(0.00583)	(0.00382)
Quartile 3	-0.0192***	-0.0148**	-0.0222***	-0.0214***	-0.0105**
	(0.00482)	(0.00482)	(0.00585)	(0.00605)	(0.00401)
Quartile 4	-0.0191*	-0.0153*	-0.0318***	-0.0316***	-0.0114
	(0.00767)	(0.00766)	(0.00932)	(0.00956)	(0.00637)
	Deciles of relative wage rate distribution				
Decile 2	0.326***	0.326***	0.212***	0.166***	0.314***
	(0.00459)	(0.00458)	(0.00571)	(0.00599)	(0.00381)
Decile 3	0.450***	0.448***	0.285***	0.230***	0.434***
	(0.00459)	(0.00459)	(0.00573)	(0.00602)	(0.00381)
Decile 4	0.541***	0.537***	0.327***	0.268***	0.521***
	(0.00460)	(0.00461)	(0.00575)	(0.00604)	(0.00383)
Decile 5	0.615***	0.609***	0.359***	0.295***	0.593***
	(0.00462)	(0.00463)	(0.00577)	(0.00608)	(0.00385)
Decile 6	0.681***	0.674***	0.393***	0.325***	0.656***
	(0.00465)	(0.00466)	(0.00580)	(0.00611)	(0.00388)
Decile 7	0.755***	0.747***	0.418***	0.348***	0.724***
	(0.00467)	(0.00469)	(0.00583)	(0.00615)	(0.00390)
Decile 8	0.829***	0.820***	0.448***	0.371***	0.799***
	(0.00528)	(0.00530)	(0.00586)	(0.00620)	(0.00440)
Decile 9	0.977***	0.965***	0.513***	0.424***	0.939***
	(0.00476)	(0.00479)	(0.00592)	(0.00632)	(0.00398)
Decile 10	1.439***	1.425***	0.762***	0.618***	1.386***
	(0.00496)	(0.00498)	(0.00617)	(0.00690)	(0.00414)
	Additional controls (full set of dummies)				
Age	×	×	×	×	×
Cohort	×	×	×	×	×
Occupation	×	×	×	×	×
Deciles of household earnings	×	×	×	×	×
Experience		×	×	×	×
N	273878	273878	187153	161089	273878

Standard errors in parentheses
* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$