The Fatherhood Earnings Penalty in Sweden: Existence, Trends, and Child Gender*

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Abstract

This study examines the impact of parenthood on men's labor market outcomes using annual income data spanning 1960 to 2021 in Sweden. Employing an event study design, I find a discernible fatherhood penalty for the most recent cohorts of men, in contrast to earlier cohorts that experienced a fatherhood premium. The observed penalty is due to a short-term reduction in labor supply (primarily due to the use of paternity leave) and enduring effects of lower wages and hours worked. The size of the penalty varies with the use of paternity leave and across regions in Sweden, but this variation largely disappears when socioeconomic factors are held constant. Moreover, I show that the fatherhood penalty is higher for men having sons relative to daughters and child gender corresponds to 11% of the long-run fatherhood penalty.

Keywords: Parenthood; Fatherhood penalty; Paternity leave; Child gender
JEL-codes: J16; J22; J23

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1 Introduction

The gender gap in labor market outcomes is persistent across industrialized countries (Olivetti and Petrongolo, 2016). The main driver of this persistence is the differential impact that parenthood have on women relative to men (Budig and England, 2001; Bertrand, Goldin and Katz, 2010; Angelov, Johansson and Lindahl, 2016; Goldin et al., 2017; Kleven, Landais and Søgaard, 2019). The substantial negative effect of children on women's earnings contrasts sharply with the non-existent or even positive impact of children on men's earnings.[1] This gender imbalance in the child penalty is consistent across countries (Kleven et al., 2019; Kleven, Landais and Leite-Mariante, 2023) and over time (Kleven, 2023). Even though the existence of this gender imbalance in child penalties is well-documented, the underlying mechanisms remain less thoroughly understood.

The long-run child penalty is nearly identical for women birthing and adopting children, challenging the notion that biological factors (e.g., pregnancy, delivery, breastfeeding) explain the gender earnings gap post-parenthood (Kleven, Landais and Søgaard, 2021; Rosenbaum, 2021; Andresen and Nix, 2022). Recent research also questions the significance of specialization in contributing to child penalties. First, in the Nordic countries, the differential impact of children on men and women is much larger in opposite-gender compared to same-gender couples even when holding couple characteristics in terms of education and earnings constant (Moberg, 2016; Rosenbaum, 2019; Andresen and Nix, 2022; van der Vleuten, Evertsson and Moberg, 2023). Second, in the United States, the motherhood penalty is even higher in "female breadwinner families" (Almond, Cheng and Machado, 2023).

Family policies (e.g., expansion of parental leave and subsidized child care) have also shown limited importance in explaining the long-run child penalties. Schönberg and Ludsteck (2014) show that the expansion of parental leave in Germany had very modest impacts on the labor market outcomes for women three to six years after childbirth. Kleven et al. (2022) find that the expansions of parental leave and childcare in Austria did not affect women's long-run labor market outcomes. Earlier research from Sweden and the US and UK also showed that higher uptake of parental leave did not lead to higher long-run child penalties for women (Waldfogel, 1998; Albrecht et al., 1999).

The leading candidate for understanding the impact of parenthood on earnings is the male breadwinner norm; women are expected to take the primary responsibility for childrearing (Kleven, Landais and Søgaard, 2021; van der Vleuten, Evertsson and Moberg, 2023; Kleven, 2023; Andresen and Nix, 2022). Research by Kleven, Landais and Søgaard (2019) and Åslund, Karimi and Sundberg (2023) highlights a generational transmission of the motherhood penalty where grandparents relative labor market participation and regional origin matters for the size of penalty. Cross-country comparisons also suggest a positive relationship between child penalties and elicited gender norms (Kleven et al., 2019), offering additional support for this

[1] For child penalties in earnings, see Angelov, Johansson and Lindahl (2016) for Sweden, Chung et al. (2017) for the US, Kleven, Landais and Søgaard (2019) for Denmark, and Kleven et al. (2019) for an overview and cross-country comparisons. See also Korenman and Neumark (1992) and Waldfogel (1997) for early evidence on child penalties in wages and Adda, Dustmann and Stevens (2017) and Cortés and Pan (2020) for theoretical contributions.

perspective.

This study focuses on Sweden, a country with a less pronounced male breadwinner norm and the main proponent of the dual-earner/dual-carer model (Ferrarini and Duvander, 2010). Sweden is often seen as a forerunner in gender-egalitarian norms and consistently ranks at the top in gender inequality indices[2] with gender egalitarian views on women in the labor force.[3] Sweden has also received much attention for its long-standing policies to improve gender equality and have the highest maternal employment rate in the OECD (OECD, 2016).[4] In conclusion, Sweden provides an interesting context to study, particularly for countries seeking to implement comparable policies aimed at challenging traditional gender roles and promoting gender equality in the labor market.

Within the Swedish context, there are two papers that closely align with this study. Angelov, Johansson and Lindahl (2016) show the couple earnings gap in Sweden and the importance of relative earnings within couples. Nylin et al. (2021) extends their analysis by showing the couple earnings gap for cohorts over time (childbirths between 1987 and 2007) and the importance of relative educational attainment within couples. I complement their analyses by concentrating specifically on men, exploring variations across a longer panel of birth cohorts (childbirths between 1963 and 2016) as well as variation across regions in Sweden and the use of paternity leave. More specifically, I will give a detailed description of when the fatherhood earnings penalty appeared and where it is concentrated.

I first show that there is a child penalty in earnings for fathers in Sweden in the ten-year horizon after entering parenthood. This penalty in earnings is primarily driven by reduced labor supply in the first years after the child is born (due to the use of paternity leave) but also persists in the longer horizon. The penalty in the long run (5–10 years after entering parenthood) is driven by a reduction in hours worked and lower wages.

The fatherhood earnings penalty has increased over time and for most cohorts of men it has been a fatherhood premium rather than a penalty. Using pensionable income, the short-run fatherhood penalty appeared for fathers born in the late 1960s and early 1970s, and it corresponded to an earnings penalty of around 5%. Before that, men were either unaffected (men born in late 1950s and early 1960s) or positively affected (born in the late 1930s up to early 1950s). For the most recent cohort of men (born in the late 1970s and early 1980s), the short-run earnings penalty is around 10%.[5] There is also some variation along the spatial dimension where the fatherhood penalty is higher in more densely populated areas of Sweden, ranging from no penalty in the most rural areas to a penalty of around 8% in the Stockholm area. These

[2]Examples of such rankings are the World Economic Forum's Global Gender Gap Index (GGI) and the Gender Inequality Index (GII) in the Human Development Report by the United Nations Development Program (UNDP). Sweden ranks 4th after Iceland, Finland, and Norway (GGI, 2020) and 6th with Belgium after Switzerland, Norway, Finland, Netherlands, and Denmark (GII, 2019).

[3]Swedes were the most likely to disagree with the statement that when jobs are scarce, men should have more right to a job than women (Inglehart et al., 2014). Sweden also has the third lowest gender gap in time spent on unpaid/care work relative to paid work in the OECD (OECD, 2016)

[4]During the period studied in the main analysis of this paper (1990–2021), Sweden's female labor force participation rate has been between 86% and 90% of the male labor force participation rate.

[5]Note that in order to have a balanced age composition of cohorts of men I cannot extend it further than men born in the early 1980s, see Tables A.2 and A.3.

regional differences are correlated with men's use of paternity leave, and correlating use of paternity leave with population density yields a similar gradient across regions.

Focusing on the importance of paternity leave, men taking more days of paternity leave have higher earnings penalties, both in the short and in the long run. This comparison of men highlights a large variation in the observed fatherhood penalties (ranging from a fatherhood premium for men taking no leave to a penalty of around 13% for men in the highest decile of paternity leave). Consequently, the average long-run penalty of 6% hides substantial variation across men.

The differences across men using more or less paternity leave is however not primarily driven by the leave itself, but rather by the characteristics of men taking more or less leave. Using a coarsened exact matching (Blackwell et al., 2009; Iacus, King and Porro, 2012) on age, calendar year, region of residence, and education for both the men and their partners, I show that most of the differences in the size of the penalty across men with more or less leave is driven by characteristics of the households. Matching on observable pre-parenthood characteristics reduced the percentage point gap in fatherhood penalties 10 years after the first childbirth by two thirds (from 6 percentage points in the unmatched comparison to 2 percentage points in the matched comparison). In conclusion, the characteristics of the couples are relatively more important than the use of paternity leave.

Research in economics has also identified variations in labor market outcomes based on the gender of children. In both the United States and Germany, studies indicate that men tend to experience an increase in their hourly wage rates and annual hours worked when they have a son compared to having a daughter (Lundberg and Rose, 2002; Choi, Joesch and Lundberg, 2008; Pollmann-Schult, 2017). Additionally, studies have revealed differences in parental behavior influenced by the child's gender, including at what age they engage their children in activities such as reading, singing songs, and teaching letters and words (Lundberg, McLanahan and Rose, 2007; Bertrand and Pan, 2013; Baker and Milligan, 2016). Studies have also shown that fathers invest more time and interaction with their children when they have a son, while mothers' time allocation remains relatively unaffected by the child's gender (Lundberg, 2005; Raley and Bianchi, 2006; Mammen, 2011). Furthermore, in Sweden Lindström (2013) show that the birth of a firstborn son leads to an increase in the use of paternity leave by fathers.

In this paper, I show that the fatherhood penalty is relatively larger for men with a first-born son than a first-born daughter, and child gender can account for 11% of the long-run fatherhood penalty. This result contrasts previous findings from the US and Germany (Lundberg and Rose, 2002; Choi, Joesch and Lundberg, 2008; Dahl and Moretti, 2008). Moreover, aligned with the findings in Lindström (2013), I find that fathers of sons take more parental leave (including more temporary parental leave for caring for sick children) than fathers of daughters.

The rest of the paper is organized as follows. In Section 2, I present the institutional setting for the study and discuss the relevant policies introduced in Sweden over the last decades. Section 3 presents the data and the sample restrictions. In Section 4, I present the empirical strategy using three empirical specifications and the necessary identifying assumptions. In Section 5, I first show that children negatively affect income for men and investigate which

men are more or less affected by entering parenthood. I then show that the child penalty is particularly pronounced for men with sons and discuss the underlying mechanisms. The paper ends with concluding remarks in Section 6.

2 Background

Individual income taxation, expansion of publicly subsidized childcare, and extensive and partly earnings-determined parental leave are all policies implemented in Sweden to increase the incentives for women to work. In 1971, Sweden introduced a compulsory individual income tax, where the tax unit is the individual rather than the household. Since men generally had higher earnings than women, the introduction of the individual income tax system made the marginal contribution of married women to work much higher. The reform had the intended effect of increasing the labor market participation rate of married women (Selin, 2014). The impact was the strongest for women with children married to high-income earners (where the marginal gain increased the most).

In 1974, Sweden was the first country to introduce an earnings-based, job-protected, and gender-neutral parental leave scheme. This reform meant that both men and women had the right to economic compensation for being at home with their small children. Men and women would receive the same number of government-paid days of parental leave but could divide them among themselves without restriction. It was, therefore, common for men to transfer their parental leave to their wives. In 1974, the paid leave was six months, but it gradually increased, reaching 15 months in 1989. Several campaigns were launched in the late 1970s to increase the voluntary use of parental leave by men, and since then, it has grown steadily from 0.5% of total leave in 1974, to 10% in 1998, to 20% in 2006, and to 30% in 2022 (Swedish Social Insurance Agency, 2022, 2023).

Along with the campaigns, the parental leave system was also reformed with "earmarked" parental leave for each parent. First, in 1995, one month of the total 15 months of paid leave could not be transferred to the other parent. The immediate impact of this reform on men's use of parental leave is well-documented (Ekberg, Eriksson and Friebel, 2013; Avdic and Karimi, 2018). In 2002, non-transferable paid leave was extended to two months, with an increase in total paid leave from 15 to 16 months. In 2016, three months of the total 16 months were made non-transferable. The reforms affected all parents to children born on or after 1st of January in each reform year. In addition to fixed parental leave, parents are entitled to government-paid temporary parental leave to care for sick children. There are no restrictions on the division of temporary parental leave between the parents.

Together with short parental leave, expensive childcare is often discussed as one of the main obstacles to women's participation in the labor market. However, Sweden was the first country to introduce public and heavily subsidized universal childcare at a very low cost to families. In 2019, 89% of two-year-olds attended preschool, and 94% of children aged three to five attended preschool (Statistics Sweden, 2019).

3 Data

I use longitudinal administrative data on individuals in Sweden between 1990 and 2021. The data link multiple registers through unique identifiers and cover all individuals residing in Sweden between the ages of 16 and 74. The data is compiled and pseudonymized by Statistics Sweden and held by the Institute for Evaluation of Labor Market and Education Policy (IFAU). It includes annual information on, earnings, social benefits, education, and place of living, combined with multigenerational data on parent-child relationships (Flergenerationsregistret). Relationship status between individuals can be identified by marriage or having a child in common.

The main outcome is annual earnings from the Swedish Tax Agency (Skatteverket), defined as labor income before taxes, excluding paid parental leave, tax deductions, and social benefits. Earnings are winsorized at the 99.5% level and adjusted for inflation using the 2018 consumer price index. Parental benefits are taken from the Swedish Social Insurance Agency (Försäkringskassan) and include job-protected paid leave for parents to care for infants and paid temporary leave for parents to care for sick children. It is registered in spells and total amounts. Parental leave benefits are earnings-based and amount to around 80% of earnings but are also capped at a maximum amount for high earners.

I restrict the main analysis to individuals I can follow for a full sequence of years in the Swedish registers, from five years before to ten years after the first child's birth. Consequently, I focus on individuals with their first child between 1995 and 2011. This sample restriction means that only individuals known, alive, and residing in Sweden for the full sequence of years are included. I make the same restriction with respect to the other parent in order to examine the within-household dimension. I do not impose any restrictions on the marital or cohabitation status of the parents, nor that it must be the first child for both parents, only that it must be the first child for the individual. I do not impose any restrictions on employment or positive earnings. However, I exclude observations in which the individual is a student (identified by the reception of student benefits). I relax this restriction in Figure C.1.

In addition, the data are merged with data containing matched information on employers and employees. These data include a large and representative sample of individuals with information on wages and contracted work hours. The information is complete for individuals employed in the public sector. All workers employed in firms with more than 500 employees are covered, and a random sample is drawn from firms with fewer than 500 employees. The data on workers in private firms include a representative sample with around 50% coverage. This data is used to study wages and contracted work hours. The analysis of these outcomes is therefore conducted on a smaller sample of individuals than the other analyses. Given that it is unusual for individuals to be included in this data set uninterrupted for all 16 years, these outcomes are analyzed using an unbalanced panel.

The workers contracted work hours are stipulated in the work contract and state whether the worker is scheduled to work full-time (40 hours per week) or a percentage of full-time. If an individual's actual working hours exceed or fall short of the contracted work hours, this will not show in the data. Common examples of when this could happen are, for instance, that

an individual's actual working hours exceed the limit of 40 hours per week (working overtime) or that an individual is on parental leave or leave for sickness. Wages are the wage stipulated in the work contract and may also diverge from the actual earnings for the same reasons as contracted work hours.

To look at the historical dimensions, I use pension data from the Pension Authority (Pensionsmyndigheten). Pension data is registered at an annual level with pensionable income from 1960 up to 2021. Pensionable income is labor income with additional measures of income that are pensionable, e.g. parental benefits, sickness benefits, and unemployment insurance. The multigenerational data on childbirths goes back to 1938. In the historical analysis I use an unbalanced panel, and a shorter time frame in order to include as many years and observations as possible. Using pension data, I focus on children born between 1963 and 2016.

4 Empirical strategy

4.1 Unit of analysis

There are two common ways to study child earnings penalties with respect to the unit of analysis to be used. The first alternative is to focus on the couple dimension and treat a unit at the household level, i.e., to focus on the woman's income relative to the man's income before and after family formation. The advantage of this approach is that it is easier to incorporate assortative mating and intra-household bargaining into the analysis of child penalties. As argued by Angelov, Johansson and Lindahl (2016), partner characteristics are important for household decisions about labor market supply both before and after entering parenthood. Within the event-study framework, it also requires weaker assumptions as the potential bias in the estimates will be cancelled out if the bias is the same for men and women.

The second alternative is to focus on differences between men and women before and after family formation and to treat the individual as a unit separate from the household. This approach is the most common in the literature (Waldfogel, 1997; Budig and England, 2001; Kleven, Landais and Søgaard, 2019). The main advantage of the second approach is that it is possible to isolate a fatherhood penalty and a motherhood penalty and to compare them across groups. Since the fatherhood penalty is the study of interest in this paper, I will therefore use individuals rather than couples as the unit of analysis.

4.2 Fatherhood penalties

As argued by Kleven, Landais and Søgaard (2019), the empirical strategies used to identify local treatment effects of an additional child are unsuitable when studying the impact of family formation in general.[6] Studies that focus on a narrowly defined group are also difficult to ex-

[6]For example, instruments in terms of twin births (Rosenzweig and Wolpin, 1980; Bronars and Grogger, 1994) or the fact that parents are more likely to have a third child if they have two children of the same gender relative to opposite-gender siblings (Angrist and Evans, 1998).

trapolate to an overall impact of children on earnings in the population.[7] Since I am interested in documenting and understanding the child penalty for men in general, I will use all first births in population data conditional on the sample restrictions in Section 3. I do not restrict the number of children, so this estimation should be viewed as the impact of family formation rather than the impact of a child.

In this paper, I use three empirical specifications to estimate child penalties and related outcomes. In my first empirical specification, I follow Kleven, Landais and Søgaard (2019) and run the following regression to estimate child penalties:

$$Y_{it} = \boldsymbol{\beta}' \boldsymbol{D}_{it}^{\text{Event}} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_{it}^{\text{Year}} + \varepsilon_{it}, \tag{1a}$$

where Y_{it} is the labor market outcome of interest for individual i in event time t. In all empirical specifications, boldface is used to denote vectors. $\boldsymbol{D}$ refers to vectors of a full set of dummies for event time, age, and calendar year. Individuals are included from 5 years before first birth to 10 years after.

Consequently, the event time dummies are indexed from -5 to 10 where $t = 0$ is the year of first birth. Event time $t = -1$ is omitted to provide the baseline. Therefore, the event time coefficient $\beta_t \in \boldsymbol{\beta}$ is the impact of children relative to one year before the first birth. Standard errors are clustered at the individual level.

I also follow Kleven, Landais and Søgaard (2019) and convert the coefficients to percentage effects using the following specification:

$$P_{it} \equiv \frac{\tilde{\beta}_t}{\boldsymbol{E}[\tilde{Y}_{it} \mid t]}, \tag{1b}$$

where $\tilde{Y}_{it}$ is the predicted counterfactual outcome of having children. Consequently, the coefficient P_{it} is income relative to income in $t = -1$. Identification comes from individuals of the same age in the same calendar year but with a first child at a different age since all individuals in the regressions have children at some point. I am interested in the difference in fatherhood penalties across generations of men, across regions in Sweden and across men with different characteristics. Therefore, I run Specification 1a separately for each group, which allows for group-specific age and calendar year effects.

Crucially, this strategy assumes that the decision to have children is exogenous to the labor market outcome studied. This assumption is strong for the long-run outcomes since one cannot rely on the smoothness assumption when extrapolation earnings profiles. If unobserved earnings potential is correlated with age at first childbirth, the estimated long-run child penalties will be biased. In Norway, Bensnes, Huitfeldt and Leuven (2023) use IVF treatments to show that women tend to time their fertility as their earnings profile flattens, which leads to an overestimation of the motherhood penalty using the standard event-study framework. This is due to an overestimation of the counterfactual earnings for the women that had children at an early age. Caution is therefore warranted when interpreting the point estimates of the long-run child penalties using this frame work.

[7] For example, the use of successful IVF treatment relative to unsuccessful treatment (Lundborg, Plug and Rasmussen, 2017).

This limitation notwithstanding, the method can handle decisions made close to entering parenthood. For example, if individuals change to a lower-paying job with more work flexibility in a period close to entering parenthood, this would be detectable as a pre-trend and violate the identifying assumption. Thus, the method is well suited to estimate child penalties related to decisions or outcomes that occur close to entry into parenthood.

However, the method cannot incorporate educational choices already made when entering the labor market. Given that this method only includes individuals that have children at some point and normalize outcomes to a pre-child level, it does not consider early investments that all individuals make in the anticipation of parenthood. For example, individuals could self-select into specific occupations that enable a job with better family-oriented work flexibility but lower earnings potential in anticipation of parenthood. This self-selection would arguably also be a child penalty that would not be detectable with the given method. From this perspective, the estimated child penalties should be seen as a lower bound on the actual child penalties conditional on that the identifying assumptions hold (Kleven, Landais and Søgaard, 2019).

When comparing men with different use of paternity leave, I use coarsened exact matching to compare men with similar observable characteristics. As seen in Table A.4, men who take more parental leave also have a higher pre-parenthood educational level and earnings than men who take less parental leave. Men taking more parental leave also earn slightly more than their partners. Therefore, to better understand the importance of paternity leave take-up for the size of child penalties, I match individuals on pre-parenthood characteristics using coarsened exact matching (Blackwell et al., 2009; Iacus, King and Porro, 2012). I match variables one year before the birth of the first child. Given that the characteristics of both the father and mother are potentially important, I match the education level of both parents across the groups.

Moreover, I match calendar year and age to handle business cycles, life cycle trends, and the fact that paternity leave increases over the studied period (Figure 4). I use one-to-one matching, meaning only individuals with a perfect match are included, and the rest are excluded. Therefore, the analysis is done on a reduced sample of individuals. I match variables classified in larger groups to avoid being too restrictive and excluding too many observations. Age is classified in intervals of two years, education level is divided into six classes (compulsory school, secondary education, low tertiary education, medium tertiary education and high tertiary education). Place of living is classified into (i) small towns and rural areas, (ii) medium-sized towns and their surroundings, and (iii) large towns and their suburbs. Men who take more than the median number of days of paternity leave in a given year of childbirth are matched with men who take less than the median number of days of paternity leave in a given year of childbirth. See Tables A.4 and A.5 for a comparison of unmatched and matched samples.

4.3 Child gender

To study the impact of child gender, I modify Specification 1a to estimate the effect of having sons relative to daughters on labor market outcomes:

$$Y_{it} = \boldsymbol{\phi}' \boldsymbol{D}_{it}^{\text{Event}} + \boldsymbol{\beta}' \left(\boldsymbol{D}_{it}^{\text{Event}} \times D_{i}^{\text{Son}} \right) + \boldsymbol{\delta}' \boldsymbol{D}_{it}^{\text{Children}} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_{it}^{\text{Year}} + \varepsilon_{it} \tag{2}$$

where I include an interaction term with the event time dummies and a dummy for having sons along with a full set of dummies for the number of children. Conditioning on the number of children means that the impact of sons is distinguished from the effect of additional children. This control is important because the number of children depends on the gender of the first child, and families that have a male first child are more likely to have a higher number of children over the 10-year follow-up horizon (see Appendix B for more details).

The coefficient of interest $\beta_t \in \boldsymbol{\beta}$ is the effect of having sons relative to having daughters. In this model, I use the randomness of the gender of the first child to estimate the impact of the child's gender on income. Therefore, this model relies on a weaker assumption than Specification 1a. In this model, the assumption is instead that the gender of the first child is exogenous to earnings.[8]

I run Specification 2 in two versions. First, I only focus on the gender of the first-born child. This specification has the benefit of relying on the arguably weak assumption that the gender of the first child is random. The downside with the specification is that instead of estimating the effect of having sons relative to daughters, it estimates the impact of having a first-born son relative to a first-born daughter. It should therefore be seen as a lower bound as individuals in the control group (individuals with a first-born daughter) are potentially treated later in the post-period (higher order child being a son).

Second, to estimate the impact of having sons relative to daughters, I run the model only for time periods in which the individuals have no children (pre-period) or one or more children of the same gender (post-period). This restriction means that individuals are censored if they have children of opposite genders. The benefit of this restriction is that it estimates the impact of sons relative to daughters, assuming the identifying assumption holds. The downside is that the model relies on a stronger assumption; individuals should not be following a fertility-stopping rule based on the gender of the children. Given that there is a tendency for both men and women to have more children when their first child is a son (0.013 more children for men and 0.018 for women), such a rule might be in place. Interpretations of the estimates should be made with this in mind.

In addition, I run both specifications of Specification 2, including indicator variables for whether the individual is living (i) with their partner, (ii) in a single household with children, or (iii) in a single household without children to control for relationship status. The reason for including these as control variables are to rule out that the impact of child gender on income goes through the relationship status of the parents.

[8]Table A.6 shows the statistics for the main sample of analysis, one year before the birth of the first child, divided by the gender of the parent and the gender of the first child. All the descriptive statistics are very similar with respect to the gender of the child, which supports the identifying assumption that the gender of the first-born child is random.

In my third empirical specification, I estimate the impact of the child's gender on outcomes that do not have a pre-period. This restriction means that I cannot visually verify the randomness of the first child using the pre-trends. I use the following specification and focus only on the post-child periods:

$$Y_{it} = \alpha + \beta D_i^{\text{Son}} + \boldsymbol{\phi}' \boldsymbol{D}_{it}^{\text{Event}} + \boldsymbol{\delta}' \boldsymbol{D}_{it}^{\text{Children}} + \boldsymbol{\gamma}' \boldsymbol{D}_{it}^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_{it}^{\text{Year}} + \varepsilon_{it} \tag{3a}$$

where D^{Son} is a dummy variable equal to one if the first child is a son and zero if it is a daughter. Again, the bold $\boldsymbol{D}$ refers to vectors of a full set of dummies for event time, number of children, age, and calendar year, respectively. Individuals are included from the year of birth of their first child up to ten years later. Standard errors are clustered at the individual level. The coefficient of interest is β, which is the effect of having either a first-born son relative to a first-born daughter or the impact of having only sons relative to only daughters. To estimate the percentage effects, I again convert the coefficient β using the following transformation:

$$P_i \equiv \frac{\tilde{\beta}}{\boldsymbol{E}[\tilde{Y}_i]}, \tag{3b}$$

where $\tilde{Y}_i$ is the predicted counterfactual outcome to having a son or only sons (i.e., having a daughter or only daughters).

The outcomes of interest for Specification 3a are relationship status and parental leave use. Note that for relationship status, I focus on marriage and whether the man and the woman are registered as living in a single household or not. I deviate from most of the US literature by not focusing primarily on marriage and divorce. The reason for concentrating on cohabitation rather than marriage is that in the Swedish population, only 25% of the men and 26% of the women are married when they have their first child. It is arguably more relevant for labor market decisions whether an individual shares a household with someone than whether they are married.

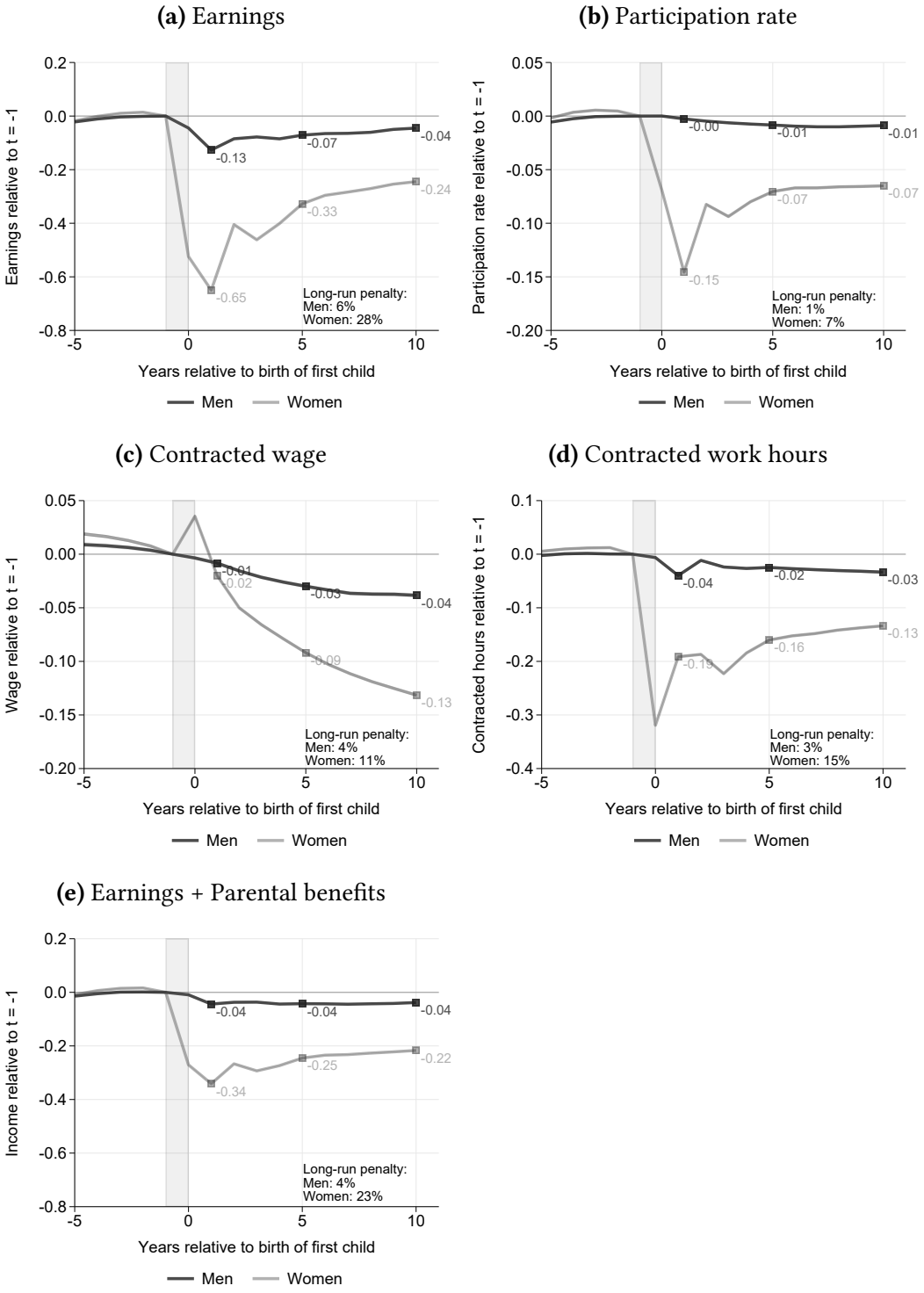
5 Results

Figure 1 shows that the fatherhood penalties in earnings, contracted hours, participation rate, wage rates, and income with parental benefits added. The figure shows that most of the earnings penalty in the short run is driven by reduced labor supply. The labor market participation rate, contracted work hours, and contracted wage rates are all relatively unaffected in the short run.[9] In the longer run, 5 to 10 years after first childbirth, primarily wages but also contracted work hours contribute to the earnings penalty. There is essentially no impact on the labor market participation rate among men.

Figure 1e shows that most of the drop in earnings in the short run is compensated by parental benefits. The impact of parental leave is transitory and in the long run, the penalty

[9]Note, however, that contracted work hours and wage rates require a change in the employment contract; reduced work hours not included in the employment contract are not covered (e.g., declining to work certain turns, working less overtime, or being on leave). The drop in hours worked could therefore be higher than the drop in contracted hours, which is also suggested by the reduction in earnings.

Figure 1: Impact of children on labor market outcomes



Notes: The long-run penalty is defined as the average child penalty between 5 and 10 years after the first childbirth. The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. See Figure A.1 for the raw earnings gap and Figure A.2 for predicted counterfactual earnings and estimated impact of children on earnings in SEK. The empirical specifications are shown in Specifications 1a and 1b in Section 4. Participation rate is an indicator variable for earning more than the 1st quintile of the earnings distribution in a given year.

in earnings and income including parental benefits are very similar. This result aligns with the fact that parental leave is mainly taken in the first two years after a child is born. The slightly higher penalty in earnings in the longer horizon is reasonable given that men might also be on parental leave for subsequent children and the fact that parental benefits do not account for 100% of the earnings. In the longer horizon, changing preferences regarding the employer, work flexibility, and working hours are more likely to play a larger role, together with employer reactions to a potential change in worker behavior. This can be seen when looking at wages and hours worked, which are both decreasing over time.

In the standard procedure in the literature, one does not consider the number of children but rather the impact of family formation. However, it is also possible to separate fathers depending on the number of children they have during the ten-year horizon from their first child. Figure A.3 shows that the child penalty for men is relatively similar regardless of the number of children. Hence, the impact of children on the fatherhood penalty is mainly driven by having at least one child rather than the number of children.[10]

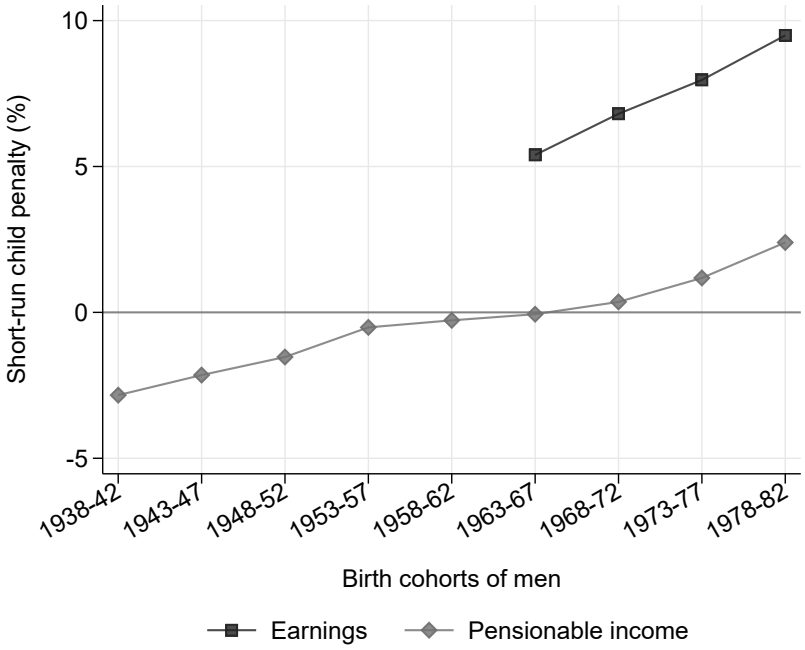
Figure 2 shows the short-run child penalties for men born between 1938 and 1982 in terms of pensionable income and for men born between 1963 and 1982 in terms of earnings. The short-run child penalty is defined as the average annual child penalty for the first 5 years after entering parenthood. Men are divided in cohorts of 5 years and for each cohort the penalty has increased, both in terms of pensionable income and in terms of earnings. The main difference between pensionable income and earnings is that pensionable income also include income that is meant to replace lost labor income, for example, parental benefits, sickness benefits, and unemployment insurance. Arguably, the penalty in pensionable income can therefore be seen as a lower bound to the fatherhood penalty, but is still valuable to compare across cohorts of men as it measures the same income over a long period of time.

Focusing on the latest cohorts of men where I also have information on earnings, it is clear that both measures of income captures an increased fatherhood penalty. Notably, men born before the 1960s had a fatherhood premium in pensionable income, meaning that they were on average positively affected by having children. This contrasts sharply to the child penalties for the same cohorts of women where the motherhood penalty was much higher for earlier cohorts (Figure A.6).

Focusing on spatial variation, Figure 3 shows that there is a clear correlation between population density and the size of the fatherhood penalty, where the penalty is higher in more urban regions. Regions are divided based on local labor markets by Statistics Sweden with the aim to have regions that are "relatively independent from the outside world in terms of labor supply and demand" (Statistics Sweden, 2023). For some rural areas there is still fatherhood premium, while the penalty is highest in the most densely populated region of Stockholm-Solna. Going from the least populated region to the most populated region increases the size of the fatherhood penalty with 7.5% percentage points. These regional differences are strongly correlated with men's use of paternity leave, and correlating use of paternity leave with pop-

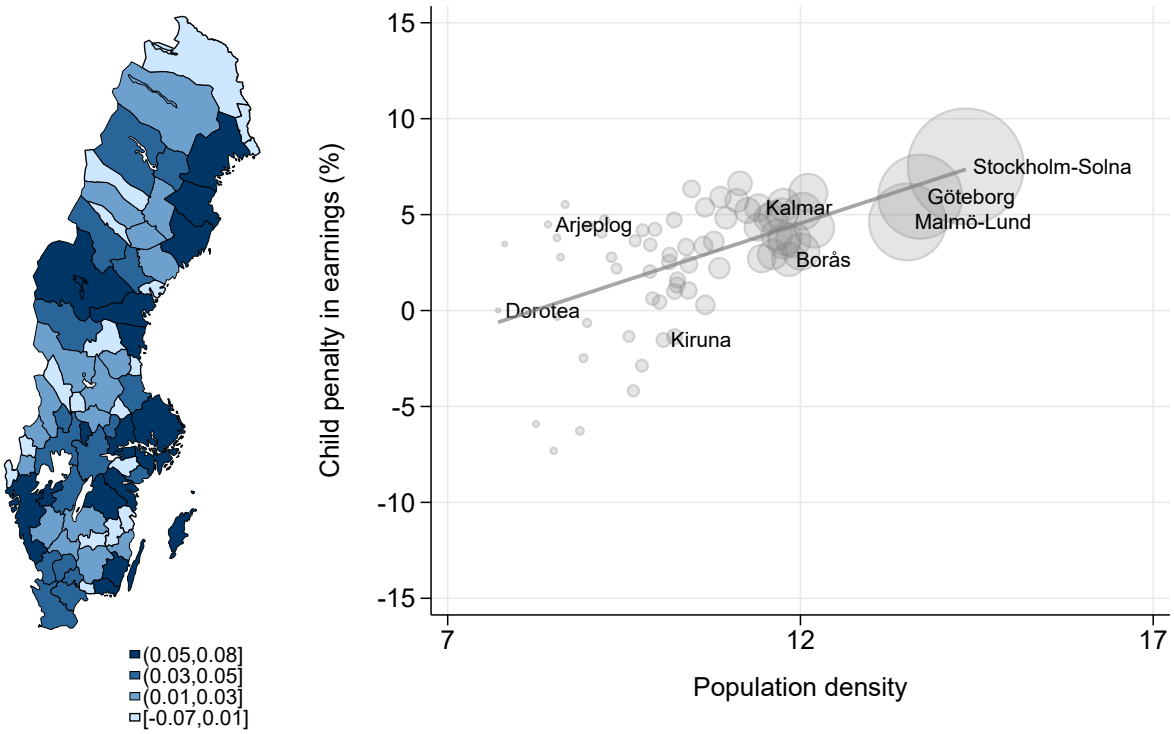
[10] An important note is that we cannot rule out endogeneity in the number of children men have. Men with more children also tend to be younger when they have their first child. Men with one or four children have lower earnings and education levels than men with two or three children at the age of their first child (Table A.1).

Figure 2: Short-run impact of children on pensionable income and earnings for men born between 1938 and 1982



Notes: The figures show the short-run impact of children for men separated by birth cohort. The short-run child penalty in the lower panel is defined as the average annual child penalty for first 5 years after entering parenthood. Note that a negative fatherhood penalty can be seen as a fatherhood premium. See Figure A.4 for underlying event study estimations and Figure A.5 for cohorts based on year for first child birth.

Figure 3: Regional variations in the size of the fatherhood penalty

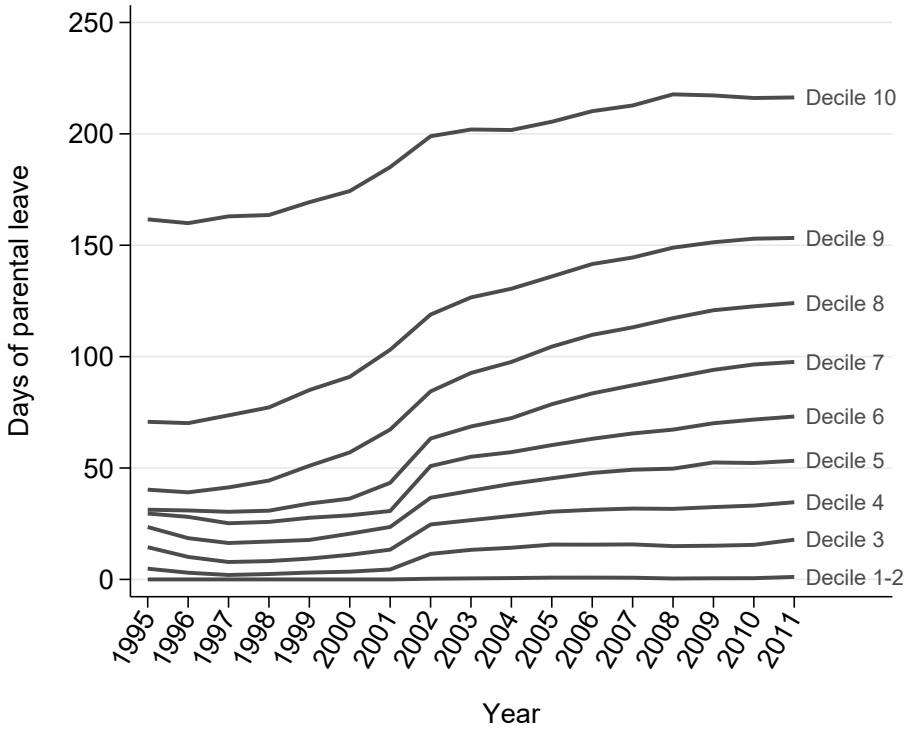


Notes: The figures show the child penalties for men separated by region of residence one year prior to first child birth. Regions are divided by labor markets according to Statistics Sweden. The child penalty is defined as the average annual penalty for the first 10 years after entering parenthood. Population density is defined as the natural logarithm of the number of people living in a region. The size of the circles in the right panel is the relative size of the population within a region. Highlighted regions in the right panel are arbitrarily assigned to give examples of regions across the scale of population density. See Figure A.8 for a separation of the child penalty into a short-run and and a long-run penalty.

ulation density yields a similar gradient across regions (Figure A.9).

The regional analysis shows that one potentially important determinant for the size of the fatherhood penalty is the use of paternity leave. Figure 4 shows that the average number of days of paternity leave has increased during the period of study (child births between 1995 and 2011). While the median number of days of paternity leave for the first two years after the child was born was 25 in 1995, it was 52 in 2011, an increase with over 100%. As the number of days of paternity leave has increased substantially during my study period, I will look at where men were in the distribution of paternity leave in the year when they had their first child. The idea is that the relative position in the distribution is more interesting than the actual number of days if one is to capture differences in gender norms across men in a given time.[11]

Figure 4: Distribution of paternity leave for the main analysis period

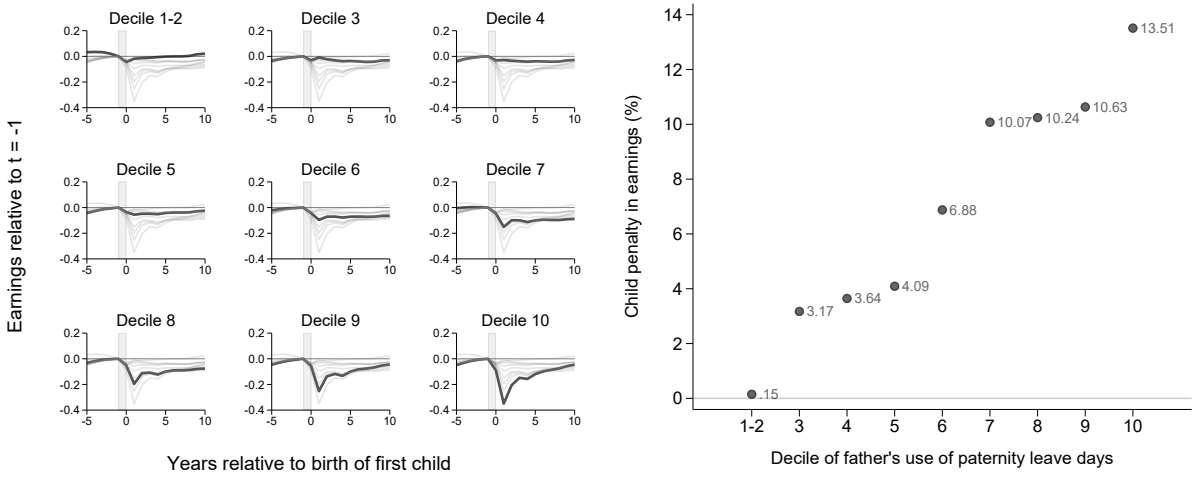


Notes: The figure shows the number of days of paternity leave taken for the first two years after first childbirth. Men which had their first child between 1995 and 2011 are included separated into deciles based on their placement of days of paternity leave in the distribution that given year.

In the following analysis, I will use the deciles from Figure 4 and look at the respective child penalty for each position in the distribution. As can be seen in Figure 5, the fatherhood earnings penalty increase for each decile, primarily in the first two years due to increased use of leave-taking, but also in the longer horizon of up to 10 years from first childbirth. The left

[11]See Figure A.11 for a division based on fathers' share of parental leave within the household in terms of deciles.

Figure 5: Fatherhood penalties by use of paternity leave



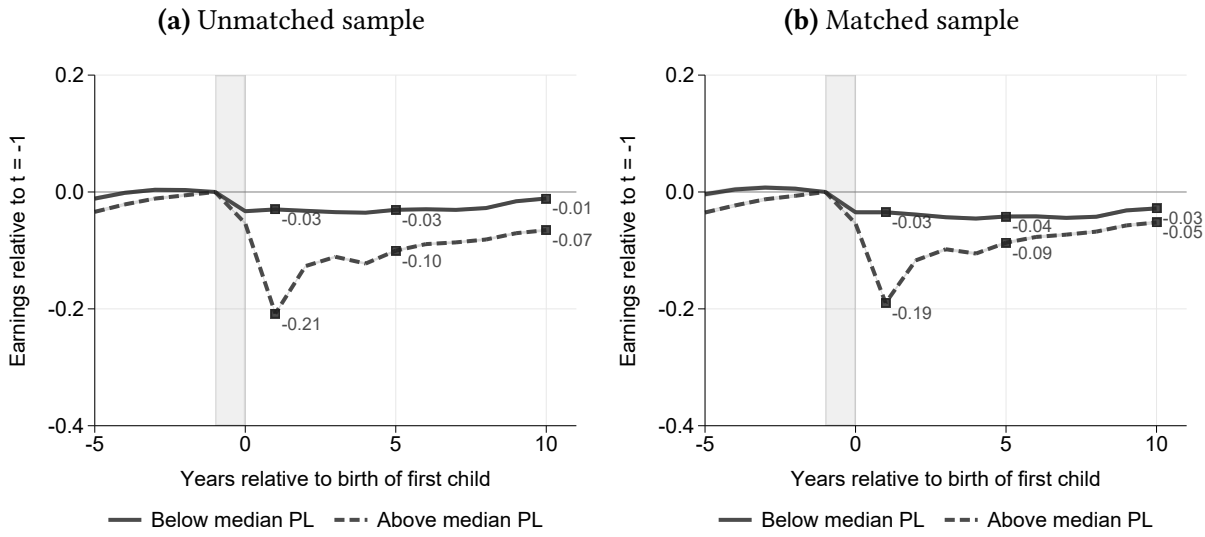
Notes: The figures show the child penalties for men separated by use of paternity leave in the first two years after first child birth. The child penalty in the right panel is defined as the average annual penalty for the first 10 years after entering parenthood. See Figure A.10 for a separation of the child penalty into a short-run and and a long-run penalty.

panel in Figure 5 shows the earnings estimated impact of children in the 10 years following first childbirth, and the right panel shows the estimated average annual child penalty for the 10 years following first childbirth. Consequently, the use of paternity leave is very informative of the size of the fatherhood penalty.

However, men that take more paternity leave are different from fathers that take less parental leave. Men taking more days of paternity leave tend to be older, more educated, and have higher pre-parenthood earnings and they also tend to have partners that are older, more educated and with higher pre-parenthood earnings (Table A.4). In Figure 6, men using paternity leave below the median are matched with men using paternity leave above the median. As seen from the figure, a majority of the variation in fatherhood penalties depending on the use of paternity leave disappears when matched on pre-parenthood characteristics. Hence, when comparing men of similar age, with similar levels of education and with partners of similar age and education, the fatherhood penalties are relatively equal in size in the long run, although the gap does not close completely.

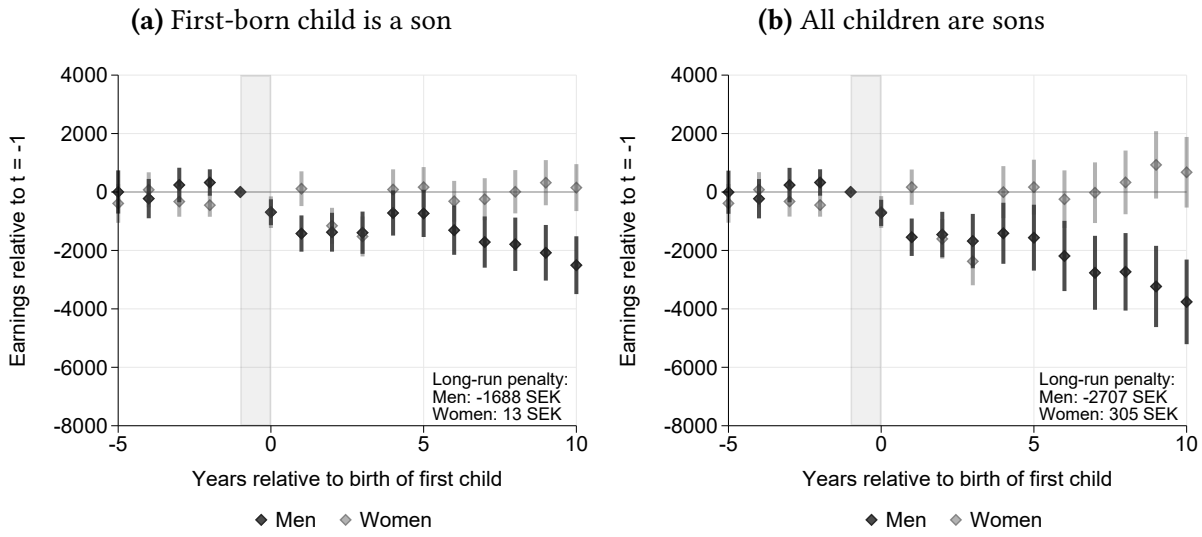
The fatherhood penalty is also higher for men with sons than for men with daughters. Using the randomness of the gender of the first child, Figure 7a shows the negative earnings effect for men having a first-born son relative to a first-born daughter. The figure shows an average annual negative impact of 1,688 SEK in the long run. Figure 7b shows the negative earnings effect for men in terms of the overall gender composition of the children. This figure only includes observations where the individual has same-gender children, establishing an average annual negative impact of 2,707 SEK in the long run. Thus, the results are qualitatively similar, but the effect sizes are mitigated in the former figure because some of the individuals in the control group (whose first-born child is a daughter) are treated later in the post-period (higher-order child is a son).

Figure 6: Child penalty in earnings by use of paternity leave: Comparison between matched and unmatched sample



Notes: The figure shows outcomes for two groups, men taking above median paternity leave in a given year for their first childbirth compared to men taking below. In the left panel, all men are included and in the right panel men are matched on own and partner's observable characteristics as described in Section 4. See Tables A.4 and A.5 for details on the characteristics of the unmatched and matched sample. Outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Specifications 1a and 1b in Section 4.

Figure 7: Child penalties from sons relative to daughters



Notes: The figures plot the estimates from additional child penalties in earnings from having sons. The left figure shows the impact of having a first-born son relative to a first-born daughter. The right figure shows the impact of having sons relative to the same number of daughters. Event time is relative to the birth of the first child and the outcomes are relative to one year before the first childbirth. The empirical specifications are shown in Specification 2 in Section 4. The long-run penalty is defined as the average child penalty between 5 and 10 years after the first childbirth.

Focusing on the magnitude of the long-run son penalty, it accounts for 11% (7% for the first-born child) of the long-run fatherhood penalty. For women, the corresponding numbers are less than 0.3%. In conclusion, there is a notable difference in the child penalties among fathers depending on the gender composition of the children but not among mothers.

The gradual decline in income indicates that the child's gender becomes gradually more important as the child ages. This pattern is aligned with previous research on the importance of child gender for fathers' participation in activities with their child (Morgan, Lye and Condran, 1988; Baker and Milligan, 2016). This finding highlights that it is when the child approaches school age and the age of leisure activities (e.g., football practice) that the child's gender makes a difference in terms of the fatherhood penalty.

As shown in Figure A.13, sons are 9.4% (6.7% focusing on the gender of the first child only) more likely to live with their fathers than daughters conditional on being a single household. This result is aligned with previous findings (Dahl and Moretti, 2008; Blau et al., 2020). Including controls for the relationship status when estimating child penalties does, however, not alter the conclusion that fathers to sons have higher child penalties than fathers of daughters (Figure A.12). Therefore, the additional son penalty to the fatherhood penalty is not driven by single household fathers being more common with sons than daughters. Fathers of sons also take slightly more parental leave than fathers of daughters (1.2% for only sons relative to only daughters and 0.7% for first-born son relative to first-born daughter). The corresponding numbers for temporary leave to care for sick children are 5.8% and 3.3%, respectively.

6 Conclusion

This paper contributes to the literature on gender inequality in the labor market by focusing on men's labor market outcomes in an environment with less traditional gender norms. The paper shows that there is considerable variation in the size of the penalty across regions in Sweden, with the average penalty ranging from a fatherhood premium in some regions to a penalty of around 7% in others. There is a positive correlation between the size of the penalty and the population density across regions, with more urban areas having a higher penalty.

The existence of a fatherhood penalty is a relatively recent phenomenon, present only for the most recent cohorts of fathers. For men born before the late 1970s, there was no or even a positive effect of parenthood on earnings. Moreover, the penalty is driven by men taking paternity leave, and a matching procedure shows that it is the selection into more paternity leave, rather than paternity leave per se, that matters the most in the long run.

Finally, the fatherhood penalty is higher for men who have sons compared to daughters. The gender composition of the children corresponds to 11% of the fatherhood penalty, and the gender of the first-born child corresponds to 7%. The fact that fathers of sons have lower earnings than fathers of daughters contrasts with studies from the United States and Germany. One potential explanation for this contradiction is that Sweden has gender norms and institutions that differ from those in the US and Germany. While the higher earnings of men with sons is often discussed in the literature in terms of a role model effect (see e.g. Raley and Bianchi (2006)), this effect may only be present in the context of stronger breadwinner norms (US and Germany), but not in an environment with more gender egalitarian norms (Sweden).

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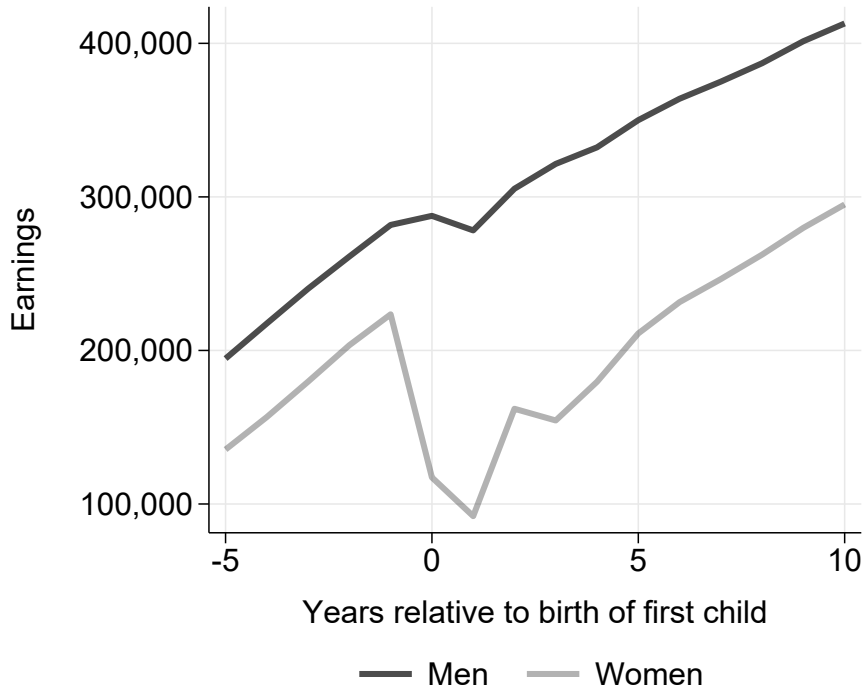
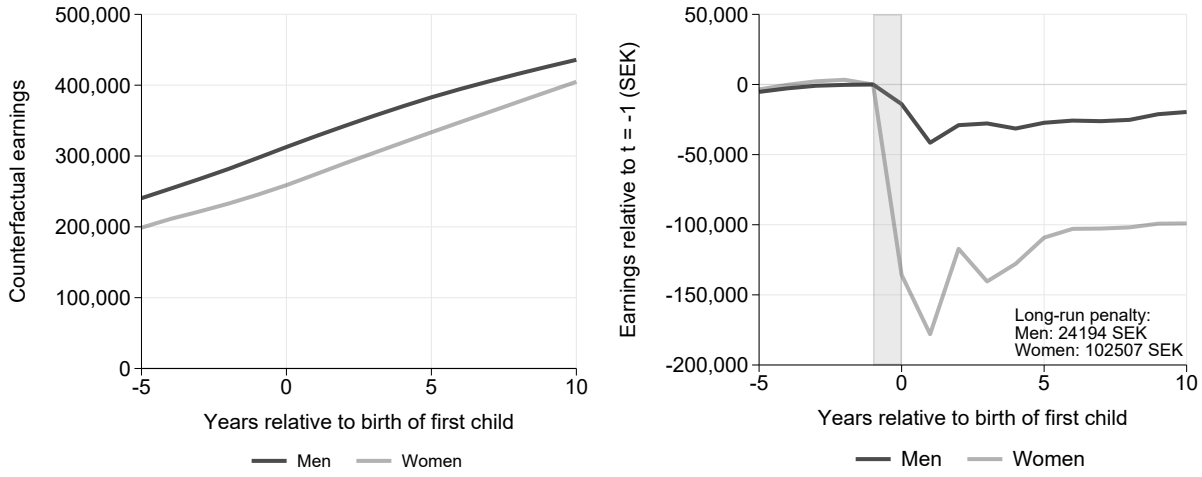
Appendix A Additional Figures and Tables

Figure A.1: Raw earnings gap from parenthood



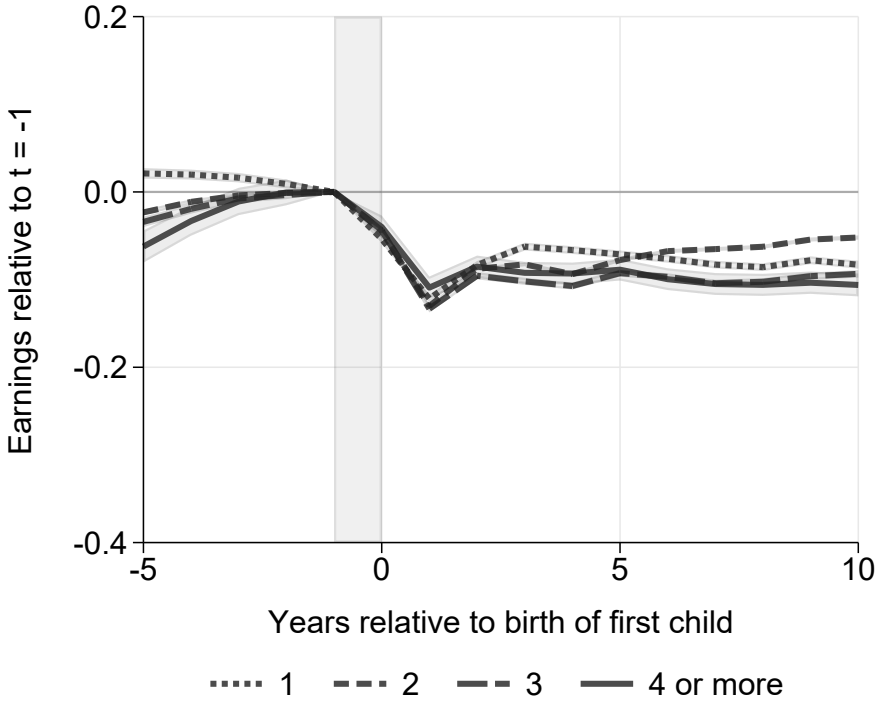
Notes: The figure shows the raw earnings gap for men and children after entering parenthood for men and women having their first child between 1995 and 2011. Earnings are adjusted to the consumer price index in 2018.

Figure A.2: Estimated counterfactual earnings and estimated child penalties in SEK



Notes: The long-run penalty is defined as the average child penalty between five and ten years after the first childbirth. The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Specifications 1a and 1b in Section 4.

Figure A.3: Fatherhood penalties in earnings by the number of children



Notes: The figure plots the estimates from child penalties in earnings. The legend shows the number of children ten years after the birth of the first child. The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Specifications 1a and 1b in Section 4. The shaded regions are 95% confidence intervals.

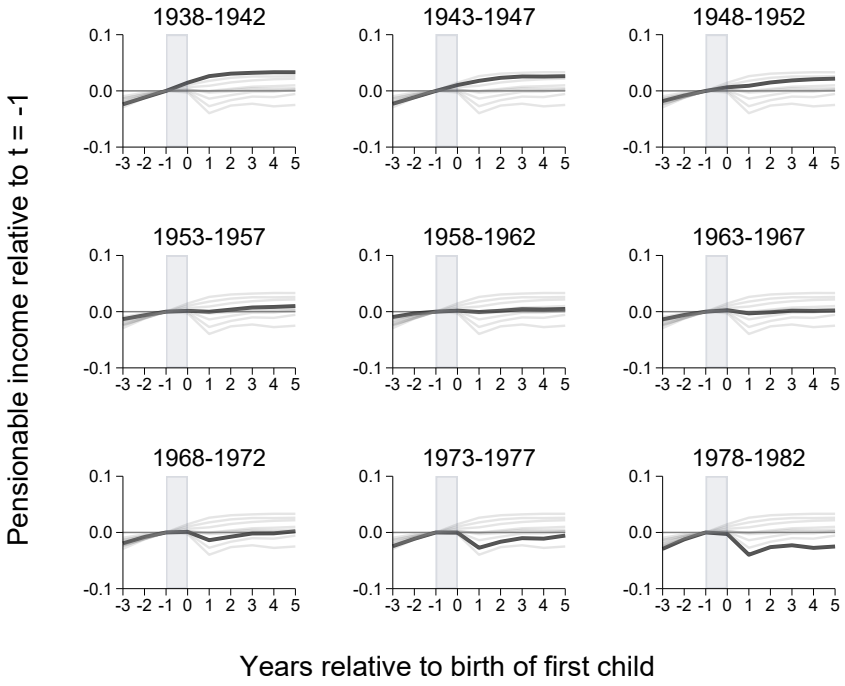
Table A.1: Descriptive statistics—Number of children

	Men				Women			
	1	2	3	4	1	2	3	4
Child birth year	2003.1	2003.4	2003.4	2003.4	2003.3	2003.4	2003.4	2003.3
	(4.945)	(4.876)	(4.820)	(4.817)	(4.962)	(4.860)	(4.802)	(4.799)
Age	32.90	30.98	29.67	28.70	31.88	29.14	27.60	26.31
	(5.492)	(4.434)	(4.180)	(4.337)	(5.459)	(4.070)	(3.720)	(3.663)
Annual earnings (1000 SEK)	262.5	293.3	277.7	238.3	213.7	236.2	216.1	173.8
	(168.6)	(165.2)	(169.9)	(164.6)	(144.0)	(132.0)	(132.3)	(127.3)
Share of household earnings	0.566	0.549	0.552	0.564	0.459	0.459	0.455	0.441
	(0.260)	(0.219)	(0.232)	(0.263)	(0.258)	(0.218)	(0.231)	(0.262)
Employment	0.806	0.866	0.836	0.769	0.770	0.848	0.799	0.693
	(0.395)	(0.341)	(0.371)	(0.422)	(0.421)	(0.360)	(0.401)	(0.461)
Monthly wage (1000 SEK)*	27.46	28.21	27.76	26.20	24.24	24.09	23.65	22.44
	(9.319)	(9.506)	(9.566)	(8.774)	(7.477)	(7.030)	(6.619)	(6.122)
Contracted work hours*	0.753	0.761	0.752	0.727	0.787	0.807	0.790	0.736
	(0.418)	(0.415)	(0.418)	(0.428)	(0.355)	(0.344)	(0.348)	(0.363)
Education (years)	11.89	12.54	12.69	12.20	12.46	13.08	13.18	12.55
	(1.994)	(2.083)	(2.158)	(2.143)	(2.105)	(2.037)	(2.085)	(2.148)
Tertiary education	0.268	0.386	0.418	0.322	0.384	0.496	0.521	0.395
	(0.443)	(0.487)	(0.493)	(0.467)	(0.486)	(0.500)	(0.500)	(0.489)
Observations	110611	327343	108940	15449	94685	323425	104478	14177

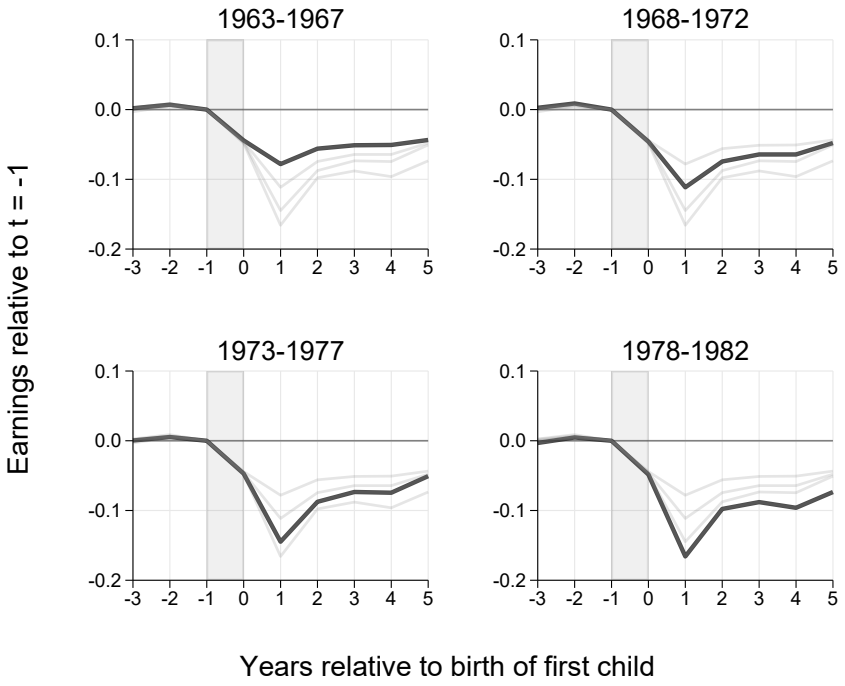
Notes: The table is separated by the total number of children ten years after the individual's first child. All variables are one year before the birth of the first child (except age which is the age at the year of birth of the first child). Annual earnings (2018 SEK) are taken from tax registers and adjusted to the consumer price index in 2018. Annual earnings (percentile) are the placement in the income distribution of that given year. Employment is an indicator variable for earning more than the 1st quintile of the earnings distribution in a given year. Public employment is an indicator variable for being employed in the public sector. Contracted work hours are the percentage of full-time work (40 hours per week). Tertiary education is an indicator variable for having tertiary education. Observations are individuals.
*These variables are taken from a representative matched employer-employee sample that corresponds to 60–70% of the full analysis sample. See Section 3 for more details.

Figure A.4: Short-run fatherhood penalties

(a) Pensionable income for men born between 1938 and 1982

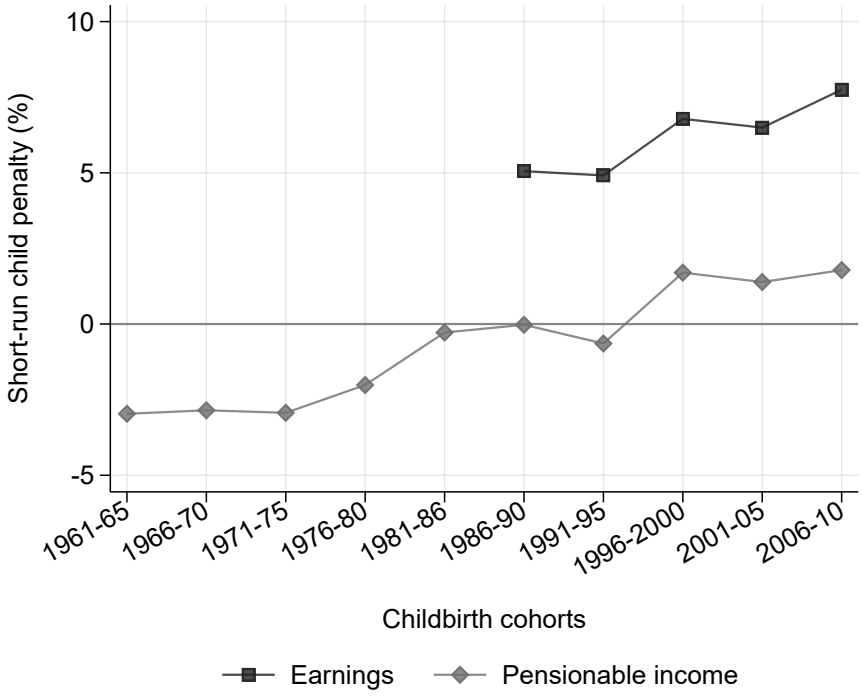


(b) Earnings for men born between 1963 and 1982



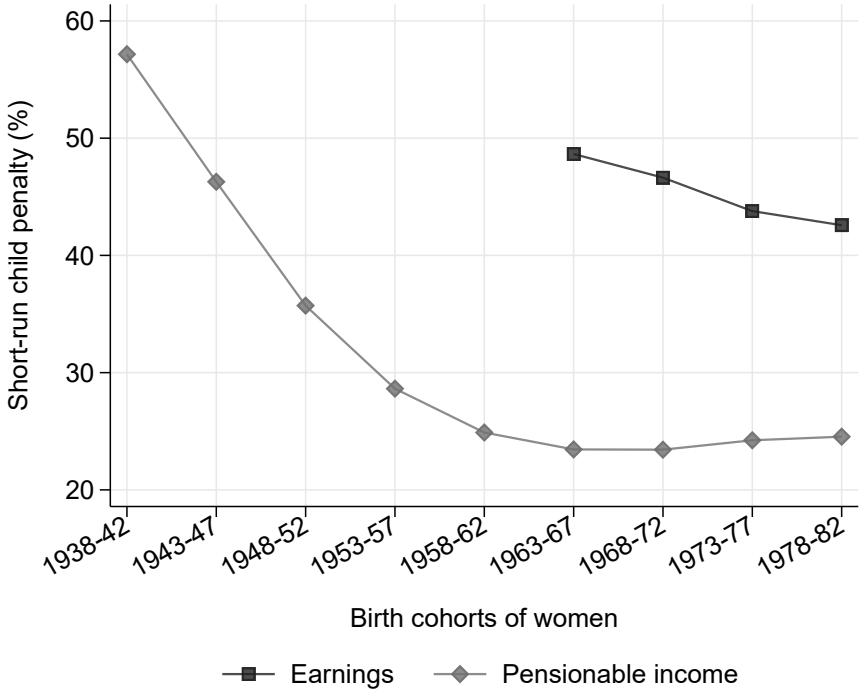
Notes: The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Specifications 1a and 1b.

Figure A.5: Short-run fatherhood penalties for children born between 1961 and 2010



Notes: The figures show the short-run child penalties in earnings for men separated by the birth year for their first child.

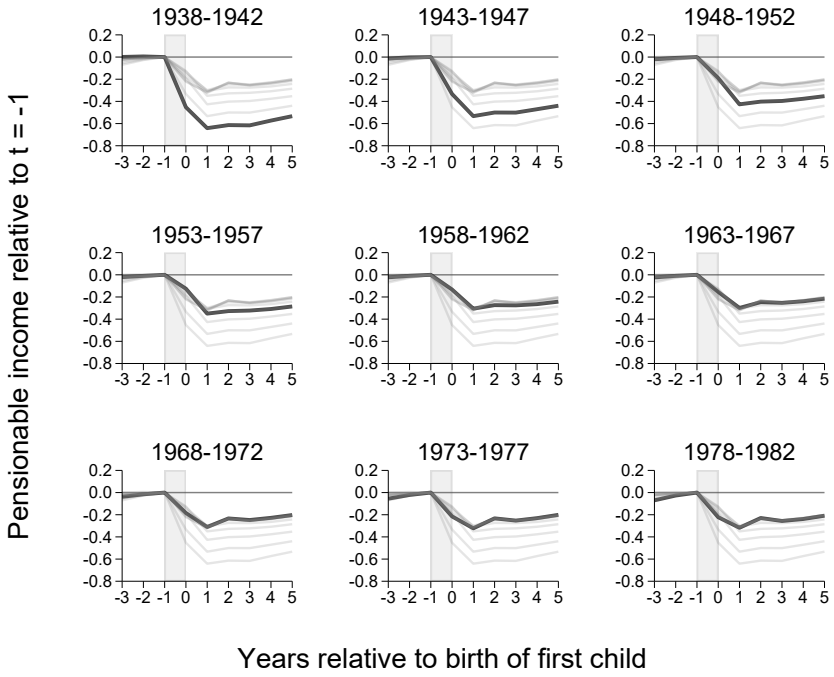
Figure A.6: Short-run motherhood penalties for women born between 1938 and 1982



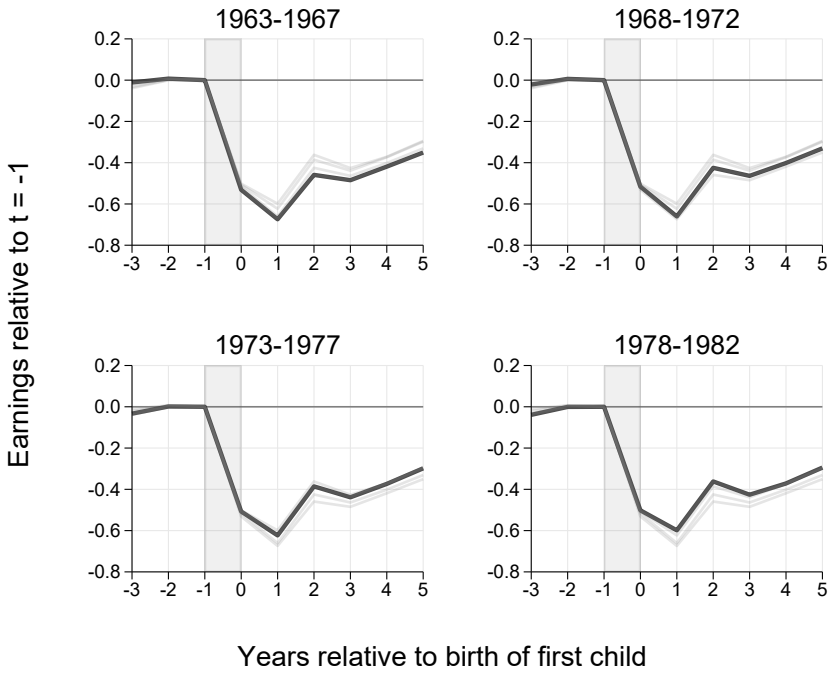
Notes: The figures show the short-run child penalties for women separated by when they were born. The short-run child penalty in the lower panel is defined as the average annual child penalty for first 5 years after entering parenthood. For underlying event study estimations see Figure A.7.

Figure A.7: Short-run motherhood penalties

(a) Pensionable income for women born between 1938 and 1982



(b) Earnings for women born between 1963 and 1982



Notes: The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Specifications 1a and 1b.

Table A.2: Descriptive statistics for birth cohorts of men

	1938–42	1943–47	1948–52	1953–57	1958–62	1963–67	1968–72	1973–77	1978–82
Age at first birth	27.59	27.18	27.88	28.96	29.69	30.01	30.96	31.53	31.01
	(4.842)	(5.346)	(5.653)	(5.842)	(5.823)	(5.989)	(5.847)	(5.126)	(4.584)
Pensionable income	182441.8	186293.2	192720.7	201829.1	209907.9	219292.5	231296.3	247419.1	258100.4
(2018 SEK)	(70890.0)	(73187.1)	(70345.1)	(68335.4)	(73600.8)	(80933.5)	(95049.3)	(109658.8)	(120184.9)
Observations	170651	240299	224160	213061	211195	246420	231512	222264	206206

mean coefficients; sd in parentheses

Notes: The table shows descriptive statistics in terms of age at first child birth and pensionable income (2018 SEK) one year prior to year of first child birth.

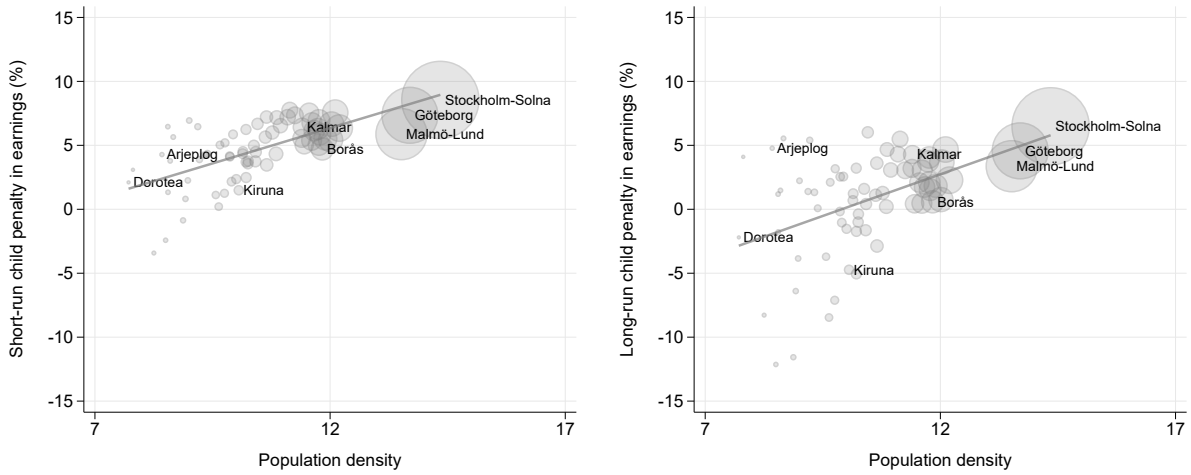
Table A.3: Descriptive statistics for birth cohorts of men

	1963–67	1968–72	1973–77	1978–82
Age at first birth	29.73	30.46	31.18	30.83
	(5.324)	(5.593)	(5.212)	(4.702)
Earnings (2018 SEK)	224687.7	255559.1	286849.0	300492.0
	(150705.8)	(181647.3)	(193925.0)	(196325.4)
Observations	243184	231459	223423	206248

mean coefficients; sd in parentheses

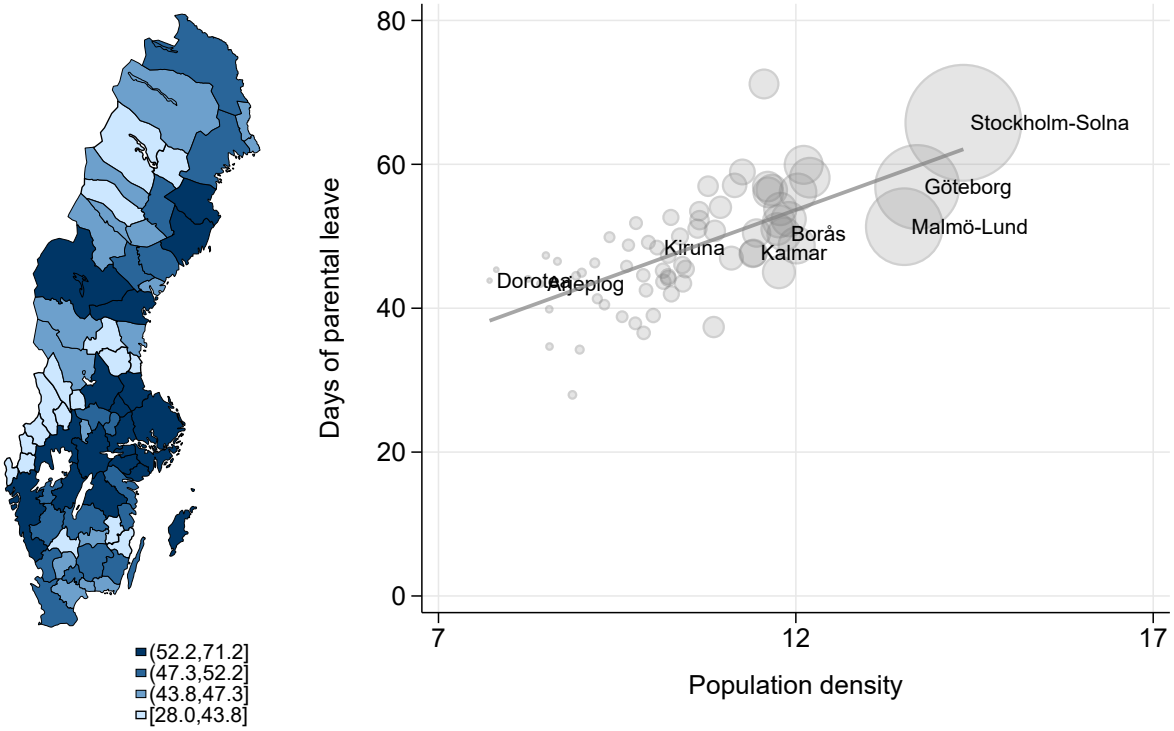
Notes: The table shows descriptive statistics in terms of age at first child birth and earnings (2018 SEK) one year prior to year of first child birth.

Figure A.8: Short-run and long-run fatherhood penalties across regions



Notes: The figures show the child penalties for men separated by region of residence one year prior to first child birth. Regions are divided by labor markets according to Statistics Sweden. The child penalty in the left panel is defined as the average annual penalty 0–4 years after first child birth, and the right panel as the child penalty 5–10 years after first child birth. Population density is defined as the natural logarithm of the number of people living in a region. The size of the circles is the relative size of the population within a region. Highlighted regions are arbitrarily assigned to give examples of regions across the scale of population density.

Figure A.9: Regional variations in the use of paternity leave



Notes: The figures show the number of days of paternity leave for men separated by in which region they lived one year prior to entering parenthood. Regions are divided by labor markets according to Statistics Sweden. Population density is defined as the natural logarithm of the number of people living in a region. The size of the circles in the right panel is the relative size of the population within a region. Highlighted regions are arbitrarily assigned to give examples of regions across the scale of population density.

Table A.4: Descriptive statistics—Men's paternity leave (unmatched)

	Men		Women	
	PL below median	PL above median	PL below median	PL above median
Child birth year	2003.4	2003.5	2003.4	2003.5
	(4.890)	(4.882)	(4.890)	(4.882)
Age	30.67	31.39	28.88	29.76
	(4.853)	(4.523)	(4.671)	(4.387)
Annual earnings (1000 SEK)	265.5	308.4	206.1	244.2
	(175.0)	(157.3)	(121.4)	(139.0)
Annual earnings (percentile)	60.10	68.51	50.00	56.99
	(28.44)	(23.77)	(22.31)	(23.84)
Share of household earnings	0.530	0.548	0.470	0.452
	(0.201)	(0.159)	(0.201)	(0.159)
Employment	0.905	0.964	0.937	0.952
	(0.293)	(0.187)	(0.242)	(0.214)
Monthly wage (1000 SEK)*	27.57	28.39	22.70	24.89
	(9.716)	(9.391)	(6.079)	(7.420)
Contracted work hours*	0.705	0.800	0.769	0.823
	(0.444)	(0.387)	(0.358)	(0.334)
Education (years)	12.10	12.93	12.53	13.45
	(1.929)	(2.179)	(1.951)	(2.066)
Tertiary education	0.296	0.474	0.368	0.588
	(0.456)	(0.499)	(0.482)	(0.492)
Observations	28553	28121	28553	28121

Notes: The table is separated by the father's share of the parental leave days taken during the first two years for the first child. All variables are one year before the birth of the first child (except age which is the age at the year of birth of the first child). Annual earnings (2018 SEK) are taken from tax registers and adjusted to the consumer price index in 2018. Annual earnings (percentile) are the placement in the income distribution of that given year. Employment is an indicator variable for earning more than the 1st quintile of the earnings distribution in a given year. Public employment is an indicator variable for being employed in the public sector. Contracted work hours are the percentage of full-time work (40 hours per week). Tertiary education is an indicator variable for having tertiary education. Observations are individuals.

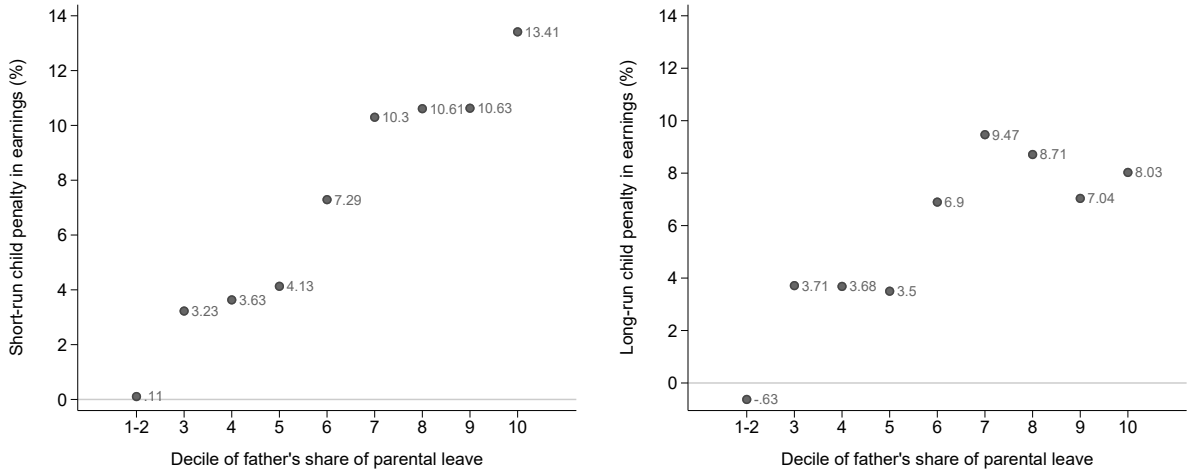
*These variables are taken from a representative matched employer-employee sample that corresponds to 60–70% of the full analysis sample. See Section 3 for more details.

Table A.5: Descriptive statistics—Men's paternity leave (matched)

	Men		Women	
	PL below median	PL above median	PL below median	PL above median
Child birth year	2003.0	2003.0	2003.0	2003.0
	(4.897)	(4.907)	(4.897)	(4.907)
Age	30.86	30.84	29.09	29.09
	(4.680)	(4.630)	(4.486)	(4.464)
Annual earnings (1000 SEK)	269.7	286.7	211.3	223.8
	(178.2)	(147.1)	(123.6)	(129.5)
Annual earnings (percentile)	60.98	65.96	51.31	53.60
	(28.60)	(23.74)	(22.51)	(23.23)
Share of household earnings	0.528	0.553	0.472	0.447
	(0.198)	(0.166)	(0.198)	(0.166)
Employment	0.908	0.958	0.940	0.944
	(0.288)	(0.200)	(0.238)	(0.230)
Monthly wage (1000 SEK)*	27.76	27.07	22.89	23.57
	(10.20)	(8.733)	(6.396)	(6.720)
Contracted work hours*	0.703	0.778	0.781	0.803
	(0.445)	(0.403)	(0.354)	(0.343)
Education (years)	12.11	12.19	12.66	12.72
	(1.917)	(1.860)	(1.987)	(1.932)
Tertiary education	0.301	0.302	0.409	0.411
	(0.459)	(0.459)	(0.492)	(0.492)
Observations	17676	17676	17676	17676

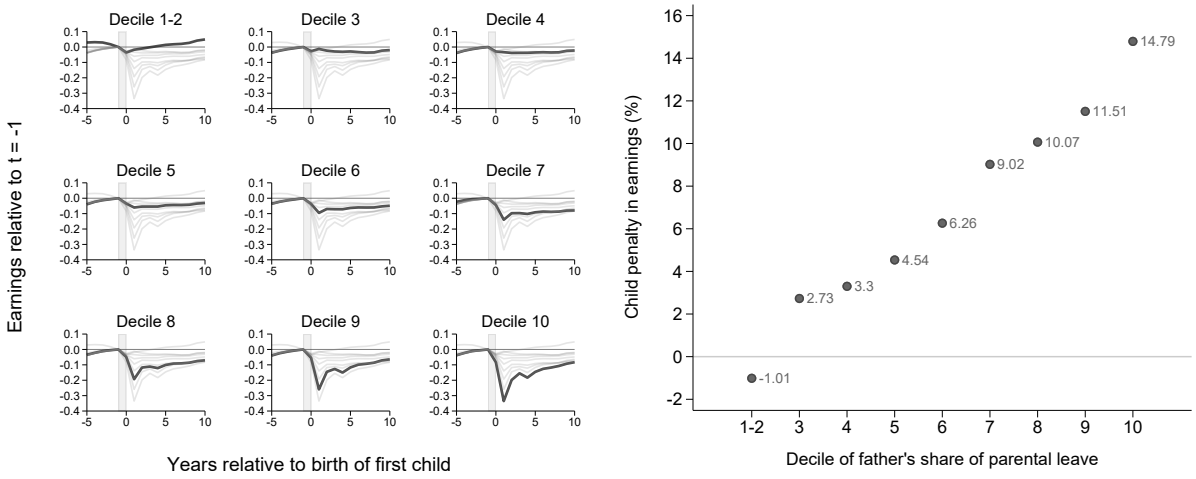
Notes: The table is matched using coarsened exact matching according to the procedure outlined in Section 4. The table is separated by the father's share of the parental leave days taken during the first two years for the first child. All variables are one year before the birth of the first child (except age which is the age at the year of birth of the first child). Annual earnings (2018 SEK) are taken from tax registers and adjusted to the consumer price index in 2018. Annual earnings (percentile) are the placement in the income distribution of that given year. Employment is an indicator variable for earning more than the 1st quintile of the earnings distribution in a given year. Public employment is an indicator variable for being employed in the public sector. Contracted work hours are the percentage of full-time work (40 hours per week). Tertiary education is an indicator variable for having tertiary education. Observations are individuals.
*These variables are taken from a representative matched employer-employee sample that corresponds to 60–70% of the full analysis sample. See Section 3 for more details.

Figure A.10: Short-run and long-run fatherhood penalties by use of paternity leave



Notes: The figures show the estimated fatherhood penalties separated by the days of paternity leave taken for the first two years after first child birth. Division into deciles is based on the placement in the distribution of days of paternity leave in a given year of child birth. The left panel shows the short-run penalties (average annual penalty 0–4 years after first child is born) and the right panel shows the long-run penalties (average annual penalty 5–10 years after first child is born). See Figure 5 for the underlying event study graphs for each decile.

Figure A.11: Short-run and long-run fatherhood penalties by use father's share of parental leave within household



Notes: The figures show the estimated fatherhood penalties separated by the father's share of parental leave of the total parental leave taken by the household in the first two years from childbirth. The left panel shows the underlying event study graphs for each decile, and the right panel the average annual penalty by decile of father's share of parental leave.

Table A.6: Descriptive statistics—Gender of the child

	Men		Women	
	Son	Daughter	Son	Daughter
Child birth year	2003.4	2003.4	2003.4	2003.4
	(4.889)	(4.880)	(4.894)	(4.883)
Age	31.01	31.02	29.12	29.12
	(4.744)	(4.745)	(4.515)	(4.512)
Annual earnings (1000 SEK)	281.4	282.0	223.3	223.7
	(167.3)	(167.3)	(134.2)	(133.9)
Share of household earnings	0.553	0.553	0.456	0.457
	(0.232)	(0.232)	(0.231)	(0.230)
Employment	0.845	0.844	0.814	0.816
	(0.362)	(0.363)	(0.389)	(0.387)
Monthly wage (1000 SEK)*	27.87	27.91	23.85	23.86
	(9.435)	(9.472)	(6.930)	(6.953)
Contracted work hours*	0.756	0.757	0.797	0.795
	(0.417)	(0.416)	(0.347)	(0.348)
Education (years)	12.42	12.43	12.92	12.92
	(2.098)	(2.098)	(2.071)	(2.070)
Tertiary education	0.365	0.367	0.465	0.466
	(0.481)	(0.482)	(0.499)	(0.499)
Observations	275789	293328	275583	292207

Notes: The table is separated by the gender of the individual and the gender of the individual's first-born child. All variables are one year before the birth of the first child (except age which is the age at the year of birth of the first child). Annual earnings (2018 SEK) are taken from tax registers and adjusted to the consumer price index in 2018. Annual earnings (percentile) are the placement in the income distribution of that given year. Employment is an indicator variable for earning more than the 1st quintile of the earnings distribution in a given year. Public employment is an indicator variable for being employed in the public sector. Contracted work hours are the percentage of full-time work (40 hours per week). Tertiary education is an indicator variable for having tertiary education. Observations are individuals.

*These variables are taken from a representative matched employer-employee sample that corresponds to 60–70% of the full analysis sample. See Section 3 for more details.

Table A.7: Impact of sons relative to daughters—First-born child is a son

	MEN				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0009	0.0008	0.0141***	71.09***	126.84***
	(0.0011)	(0.0006)	(0.0020)	(19.77)	(8.85)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
# Children	Yes	Yes	Yes	Yes	Yes
Baseline	0.47	0.14	0.21	10161	3799
Percent effect	-0.2%	0.6%	6.7%	0.7%	3.3%
Observations	6102309	6102309	765063	6102309	6102309
	WOMEN				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0007	0.0009	-0.0080***	-25.95	72.68***
	(0.0011)	(0.0006)	(0.0017)	(27.64)	(9.75)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
# Children	Yes	Yes	Yes	Yes	Yes
Baseline	0.48	0.13	0.84	29676	3738
Percent effect	-0.1%	0.7%	-0.9%	-0.1%	1.9%
Observations	5782472	5782472	744211	5782472	5782472

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.

Notes: The table shows the impact of having a first-born son relative to a first-born daughter on a range of outcomes for men and women respectively. The baseline is the average predicted outcome when having a first-born daughter. The percent effect is the percentage increase in the relevant outcome when the first child is a son relative to a daughter. Model specifications are shown in Specifications 3a and 3b.

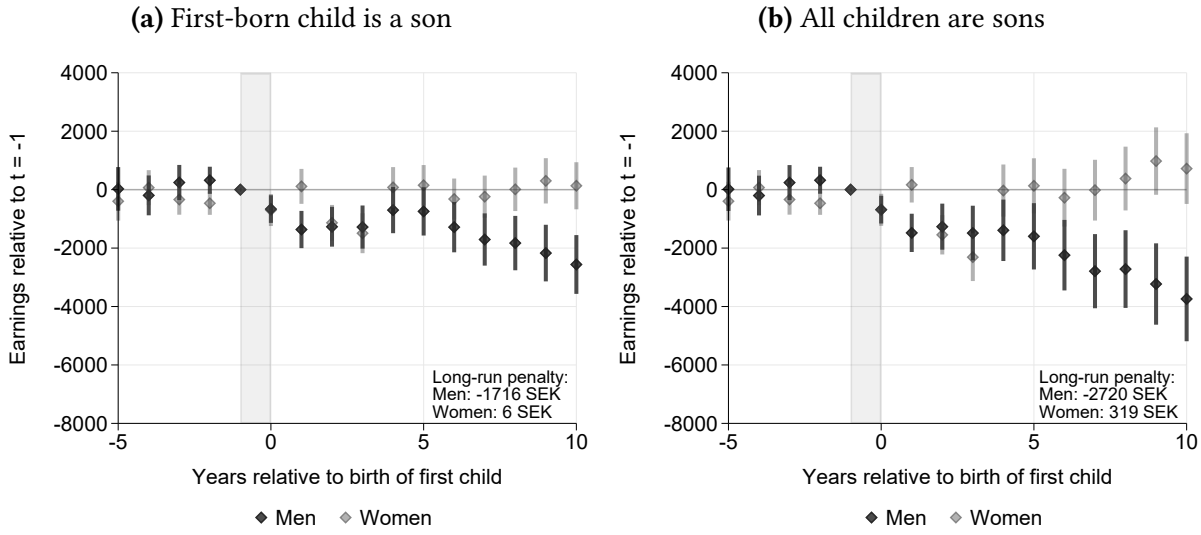
Table A.8: Impact of sons relative to daughters—All children are sons

	MEN				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0019	0.0018**	0.0176***	110.78***	197.55***
	(0.0012)	(0.0008)	(0.0022)	(21.25)	(9.55)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
# Children	Yes	Yes	Yes	Yes	Yes
Baseline	0.43	0.16	0.19	9100	3426
Percent effect	-0.4%	1.1%	9.4%	1.2%	5.8%
Observations	4170009	4170009	604752	4170009	4170009
	WOMEN				
	Marriage	Single household	Single household with child	Parental leave	Temporary parental leave
Son	-0.0019	0.0021**	-0.0117***	25.03	120.18***
	(0.0013)	(0.0008)	(0.0020)	(29.68)	(10.88)
Year	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes
Event time	Yes	Yes	Yes	Yes	Yes
# Children	Yes	Yes	Yes	Yes	Yes
Baseline	0.43	0.15	0.85	29363	3303
Percent effect	-0.4%	1.4%	-1.4%	0.1%	3.6%
Observations	3909941	3909941	584644	3909941	3909941

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.

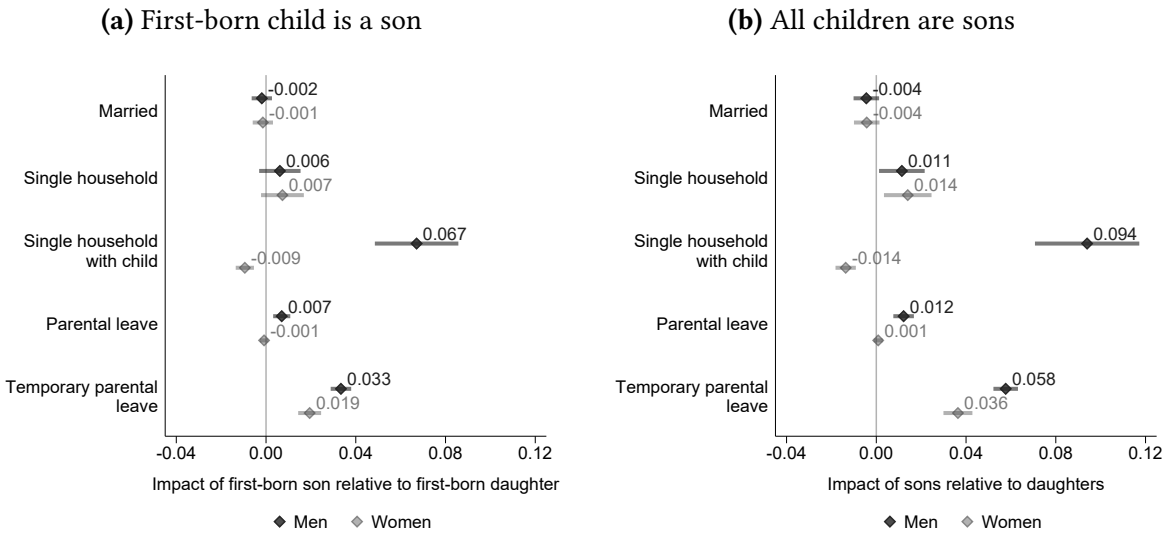
Notes: The table shows the impact of having two first-born sons relative to two first-born daughters on a range of outcomes for men and women respectively. The baseline is the average predicted outcome when having two first-born daughters. The percent effect is the percentage increase in the relevant outcome when the first child is a son relative to a daughter. Model specifications are shown in Specifications 3a and 3b.

Figure A.12: Child penalties from sons relative to daughters (controlling for relationship status)



Notes: The figures plot the estimates from additional child penalties in earnings from having sons. The left figure shows the impact of having a first-born son relative to a first-born daughter. The right figure shows the impact of having sons relative to the same number of daughters. Event time is relative to the birth of the first child and the outcomes are relative to one year before the first childbirth. The empirical specifications are shown in Specification 2 in Section 4. The long-run penalty is defined as the average child penalty between five and ten years after the first childbirth.The regressions include indicator variables for whether the individual is living (i) with their partner, (ii) in a single household with children, or (iii) in a single household without children. The bars are 95% confidence intervals.

Figure A.13: Impact of having sons relative to daughters



Notes: The figures show the relative impact of having sons compared to daughters in the ten years following first childbirth. The left figure shows the impact of having a first-born son relative to a first-born daughter. The right figure shows the impact of having sons relative to the same number of daughters. Effects are converted to relative effects by dividing them with the predicted counterfactual outcome, i.e., had the first child been a daughter (panel a) or had the children been daughters (panel b). The empirical specifications are shown in Specification 3a and 3b in Section 4. The full regressions are shown in Tables A.7 and A.8. The bars are 95% confidence intervals.

Appendix B Child gender and fertility

To estimate the effect of the gender of the first child on fertility, I modify Specification 3a accordingly:

$$Y_i = \alpha + \beta D_i^{\text{Son}} + \boldsymbol{\gamma}' \boldsymbol{D}_i^{\text{Age}} + \boldsymbol{\lambda}' \boldsymbol{D}_i^{\text{Year}} + \varepsilon_i \tag{4}$$

where I look only at the number of children ten years after the birth of the first child. I follow Dahl and Moretti (2008) and estimate the impact of the gender of the first child on the number of children and the likelihood of having at least two, three, and four children, respectively. I again transform the coefficient β using:

$$P_i \equiv \frac{\tilde{\beta}}{\boldsymbol{E}[\tilde{Y}_i]}, \tag{4b}$$

where $\tilde{Y}_i$ is the predicted counterfactual outcome to having a son.

As seen in Table B.1, if a man has a first-born son, the likelihood of having at least two children increases by 0.29% (0.57% for women). This finding shows that there is not a general son preference for parents in Sweden but rather a preference for daughters. This result is aligned with previous research in the Swedish context (Andersson et al., 2006) and more recent data on the US (Blau et al., 2020) but contrasts earlier research from the US (Dahl and Moretti, 2008). Moreover, it highlights the importance of controlling for the total number of children when identifying the impact of child gender (and not the impact of the number of children).

Table B.1: First child's gender and fertility

	MEN			
		Breakdown by number of children		
	Total number of children	Two or more children	Three or more children	Four or more children
Son	0.0129***	0.0030***	0.0090***	0.0007
	(0.0019)	(0.0010)	(0.0011)	(0.0004)
Year	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Baseline	2.04	0.80	0.21	0.03
Percent effect	0.63%	0.38%	4.25%	2.43%
Observations	569117	569117	569117	569117
	WOMEN			
		Breakdown by number of children		
	Total number of children	Two or more children	Three or more children	Four or more children
Son	0.0177***	0.0051***	0.0114***	0.0012***
	(0.0018)	(0.0009)	(0.0011)	(0.0004)
Year	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Baseline	2.07	0.82	0.21	0.03
Percent effect	0.86%	0.62%	5.31%	4.48%
Observations	567790	567790	567790	567790

Standard errors in parentheses, * p < 0.1, ** p < 0.05, *** p < 0.01.

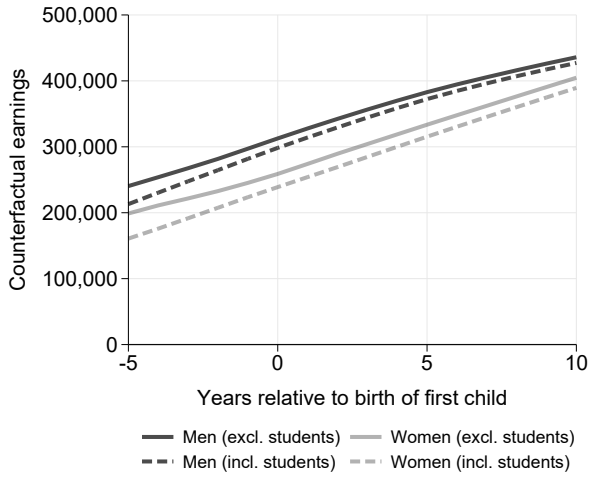
Notes: The table shows the impact of having a first-born son relative to a first-born daughter on fertility for men and women respectively. The baseline is the average predicted outcome when having a first-born daughter. The percent effect is the percentage increase in fertility when the first child is a son relative to a daughter. Model specifications are shown in Specifications 4 and 4b.

Appendix C Student restriction

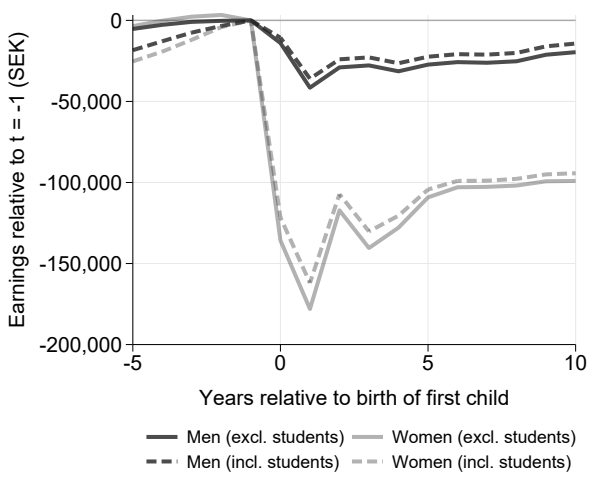
In the main analysis, I exclude observations where the individuals are students. This deviates from previous studies using Scandinavian data that include students and, in general, also have stronger pre-trends (Kleven, Landais and Søgaard, 2019; Kleven et al., 2019; Sieppi and Pehkonen, 2019; Andresen and Nix, 2022; van der Vleuten, Evertsson and Moberg, 2023). When including students in the analysis, the pre-trends suggests that individuals that have children at a younger age have a steeper age-earnings profile relative to individuals who have children at an older age. This is however driven by students having no income at a younger age, and underestimates the predicted counterfactual outcome to having children. As shown in the figure, it does not impact the size of the relative child penalties, however.

Figure C.1: Sample restriction—Removing students

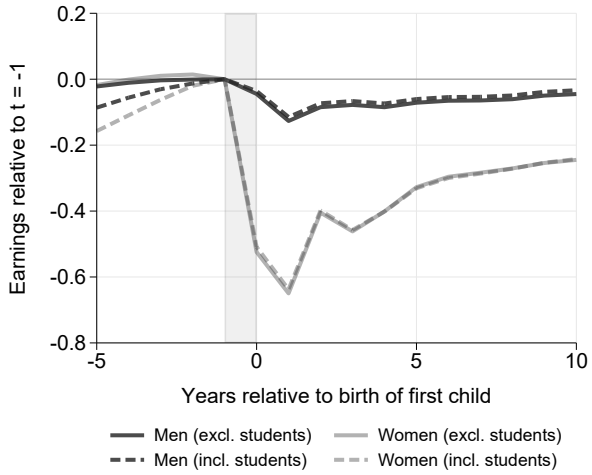
(a) Predicted counterfactual earnings



(b) Estimated child penalty in SEK



(c) Estimated child penalty in %



Notes: The figures plot the estimates from child penalties in earnings. The outcomes are relative to one year before the first childbirth and are converted to relative effects by dividing them with the predicted counterfactual outcome for individual i in period t. The empirical specifications are shown in Specifications 1a and 1b in Section 4. The shaded regions are 95% confidence intervals.