# Dual-Earners without Childcare: Intergenerational Impacts and Policy-Sequencing Lessons

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**Abstract**

A growing share of children are raised in dual-earner families. Yet childcare provision has not kept pace. We study the long-run consequences for children using Sweden's 1971 transition from joint to individual taxation. The reform sharply reduced tax rates for secondary earners and increased married women's employment, but preceded the large-scale expansion of public childcare. Combining cross-family variation in reform-induced tax changes with within-family variation in siblings' ages, we find that exposure to working parents during childhood reduced cognitive and non-cognitive skills, educational attainment, and adult earnings. Exposed children also reflect more gender-egalitarian norms in their marital and parental choices. The human-capital losses, which also persisted in the third generation, are not inherent; they are concentrated in regions with scarce childcare. The findings point to a policy-sequencing lesson: expanding parental employment without first providing childcare can come at an intergenerational cost.

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One of the most transformative changes in the labor market over the past half-century has been the global and secular rise in female labor force participation. The rise reflects, above all, the entry of married women into paid work (Albanesi et al., 2023). As a result, the share of children growing up in dual-earner households has more than doubled during this period (see Fox et al. (2013) and Figure 2b). What are the long-run consequences for the next generations? The answer is theoretically ambiguous. Higher household income may benefit children, but reduced parental time and supervision may offset these gains (Page, 2024). At the same time, maternal employment may alter family dynamics more broadly: fathers may increase their involvement in childcare, and children may internalize new norms about gender roles and labor market participation (Galassi et al., 2024).

The answer also depends on the relevant counterfactual. When both parents work, child development depends critically on the availability and quality of non-parental care. In settings where public childcare provision has not kept pace with demand—as remains true in many countries today (Gromada and Richardson, 2021)—families must rely on private childcare or informal arrangements, such as grandparents, nannies, or older siblings, whose quality is unevenly distributed but likely to fall short of parental care. In the Swedish context we study, the quality of available substitutes was low: the vast majority of family daycare providers had no relevant childcare training, and a single childcare worker in formal daycare centers was typically responsible for between five and ten children (SOU, 1967, 1972). Yet most evidence on the long-run effects of maternal employment comes from periods and countries in which formal childcare was already widely available (Danzer et al., 2022a; Lo Bue et al., 2023). This leaves open whether the effects of maternal employment differ when increases in mothers' labor supply are not accompanied by broad access to formal care.

To address these issues, we analyze Sweden's 1971 tax reform, which shifted from joint to individual taxation and sharply increased work incentives for married women. The reform constituted a central component of the fourth and final phase of the Swedish welfare system expansion, implemented in the 1970s . As a key part of this expansion the reform was introduced ahead of the roll-out of universal public childcare beginning in 1974—a process that unfolded over two decades. This policy sequencing meant the tax reform incentivized mothers to work at a time when formal non-parental care was largely unavailable. This context allows us to investigate the effects of increased maternal employment on next generations' outcomes in an environment where formal childcare was scarce and of limited quality. Our findings speak directly to current policy debates in countries such as the U.S. and Germany, where proposals for similar tax reforms coincide with limited public childcare provision.

Our empirical strategy leverages both within-family and across-family variation in exposure to the tax reform. Across families, we exploit differences in pre-reform marginal tax rates among mothers, which are determined by their husbands' pre-reform earnings. Within families, we compare siblings who experienced different years of exposure to increased maternal labor supply depending on their age at the time of the reform.

This dual approach enables us to disentangle the causal effect of maternal employment from confounding family characteristics and broader economic trends. Using a potential-outcomes framework, we derive an age-specific estimand and show how sibling differences in age at reform, together with cross-family variation in reform intensity, identify the causal effect under standard instrumental-variables assumptions. Because intergenerational income transmission differs by gender—sons' incomes are more strongly linked to their fathers' (Nekoei and Sinn, 2021)—we restrict our main analysis to same-gender sibling pairs, which removes a potential source of bias in the within-family comparisons.

The first stage of our analysis is to show that the 1971 reform generated a large and persistent increase in married mothers' labor supply. We built a tax calculator based on the Swedish tax code, similar to what is used in the literature on elasticities of taxable income (e.g. Gruber and Saez, 2002). Using the tax calculator, we simulate each mother's reform-induced change in the marginal net-of-tax rate. Because joint taxation tied wives' marginal tax rates to their husbands' income, the move to individual taxation raised work incentives most strongly for mothers with higher-income husbands. The relationship between changes in weekly hours and the simulated change in the marginal net-of-tax rate is strongly positive and approximately linear. Using husbands' pre-reform earnings as the source of variation in marginal tax rates, we estimate that a one percentage point increase in the net-of-tax rate raises weekly hours by about 0.23, implying an overall labor supply elasticity of 1.21, with a corresponding intensive-margin elasticity of 0.33 and extensive-margin elasticity of 0.60. These magnitudes are broadly consistent with existing estimates for mothers and married women (e.g. Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Selin, 2014; Kleven, 2024).

Maternal labor supply has a lasting effect on the children's labor-market outcomes. Measuring lifetime income as total labor earnings between ages 30 and 50, we find that effects vary by age of exposure and gender. Negative effects are concentrated in the preschool years: a one-year earlier exposure to a persistent 10-hour increase in maternal weekly hours reduces sons' lifetime income by approximately 4.7 percent. Effects attenuate sharply after school entry—approaching zero for boys and small, imprecise negatives for girls. For preschool boys, the earnings loss reflects a downward shift in the middle of the distribution, with little change at the tails. Under childcare scarcity, the long-run earnings costs of maternal employment fall primarily on sons exposed in early childhood.

The long-run earnings losses spill over to children's households. For sons, preschool-exposure losses are partly offset by higher wives' earnings; for daughters, the household penalty operates primarily through lower husbands' income.

To discipline our empirical investigation, we embed it in a parsimonious intergenerational model linking parental decisions to children's environments, decisions, and outcomes. We develop an overlapping-generations model in which parental labor supply affects children through two distinct channels. Parents allocate their time between work and their children, and spend their income on consumption or investment in their children. Children choose their study effort,

which complements parental investments and raises their adult human capital. At the same time, children learn gender norms by observing the division of household caregiving.

The human-capital channel has a standard implication: lower parental time investment reduces children's future wages, lowering the opportunity cost of time at home and inducing them, as adults, to work less and spend more time with their own children. The norm channel generates a different set of predictions. When mothers work more, children acquire stronger beliefs in gender equality, but the behavioral implication depends on the child's sex, as daughters are more often second earners. For sons, the human-capital and norm channels reinforce each other, both leading to lower market work and greater time at home. For daughters, the two channels operate in opposite directions: gender-equitable norms increase labor supply and reduce time at home, offsetting the effect of lower human capital. The model therefore delivers a sharp asymmetry: maternal labor supply has reinforcing effects for sons but countervailing effects for daughters

We investigate these two channels through which individual and household income can be shaped: human capital and marriage. We find strong evidence for a human-capital channel. Maternal employment during childhood reduces their sons' cognitive and non-cognitive skills and lowers educational attainment, with the clearest schooling losses appearing for boys exposed in preschool and for girls exposed in primary school. These effects are quantitatively important: among sons, skill losses account for about one-third of the decline in lifetime earnings, education accounts for about one-half, and education and skills jointly account for roughly two-thirds of the total income loss. For daughters, we also find negative effects on education, although these do not translate into a comparable decline in lifetime income.

We investigate childcare quality as a mediating channel, proxying for care quality using grandparent availability and municipal-level availability of formal and informal childcare workers at the time of the reform. The results reveal a clear quality gradient. Children with access to grandparents experience substantially smaller negative effects, suggesting grandparent care is of higher quality than the formal and informal alternatives available at the time. Among children without nearby grandparents, negative effects are paradoxically larger in municipalities with greater formal and informal childcare availability—consistent with these alternatives being of low quality relative to parental or grandparental care. The long-run costs of maternal employment thus reflect not merely the absence of maternal time, but the quality of the care that substitutes for it.

The family channel operates not through whether children form families, but through how those families are formed and how childcare is allocated within them. Exposure has essentially no effect on partnering rates, spouse age, the likelihood of having children, or completed fertility. Instead, exposed daughters sort into less educated partners, while exposed sons enter parenthood earlier, consistent with the schooling losses documented above an earlier transition into adult life. Within the next generation of households, childcare is reallocated in a more gender-equal direction: exposed sons take more parental leave and their wives take less, whereas exposed daughters take less leave and their husbands take more. Maternal employment thus reshapes within-household specialization across generation, without affecting family size.

The costs of maternal employment under childcare scarcity extend to the third generation. Children of those exposed to maternal employment during preschool years perform worse at the end of compulsory school, with the largest losses among the children of exposed sons and little evidence of harm when exposure begins only in primary school. This pattern is consistent with intergenerational transmission through parental human capital and household resources: the reform reduced education, skills, and adulthood income among the cohort that later became parents, and a more equal division of childcare in the next generation does not fully offset these losses. The costs of maternal employment under childcare scarcity, therefore, persist across generations.

Our findings imply that reforms of couple-based taxation and childcare policy are complements and should not be evaluated in isolation. A move to individual taxation raises secondary-earner employment and promotes *couple equity* a la Goldin (2021), but its consequences for children's human capital depend on the childcare environment, creating a dynamic coordination problem in which the welfare effects of the transition hinge on which policy moves first (Lindbeck et al., 1999; Lindbeck, 2001). The Swedish experience illustrates the transitional incidence: dual-earner households spread faster than formal childcare, and the exposed cohorts bore persistent costs in skills, education, and earnings. Expanding childcare first protects transitional children but requires large up-front fiscal commitments under uncertain demand; reforming taxation first reveals the scale and location of childcare demand and may build a political constituency for public provision, but exposes some children to inadequate substitute care while supply adjusts slowly under entry, credit, and informational frictions. The externally relevant conclusion is therefore conditional: policies that raise secondary-earner employment and policies that expand high-quality childcare are complements, but their optimal sequencing depends on the institutional environment.

The remainder of the paper proceeds as follows. After a short summary of literature, Section 1 describes the administrative data, our sibling-pair sample, and the childcare environment in Sweden around 1971. Section 2 presents the 1971 transition from joint to individual taxation and documents its effect on married women's labor supply. Section 3 develops the empirical framework, a sibling-difference IV design that combines cross-family variation in reform-induced tax changes with within-family variation in siblings' ages at the reform. Section 4 presents an overlapping-generations model in which parental labor supply affects children through human-capital and gender-norm channels. Section 5 reports the results: long-run effects on children's individual and household income, the mediating roles of skills, education, and childcare quality, effects on family formation and the within-couple allocation of childcare, transmission to the third generation, and placebo evidence on instrument validity. Section 6 discusses implications for the design and sequencing of family taxation and childcare policy, and Section 7 concludes.

### Related Literature

This paper is most closely related to the causal literature on maternal employment during childhood and children's human capital. One strand studies school performance, including test scores,

grades, and related measures of academic achievement (James-Burdumy, 2005; Løken et al., 2018; Nicoletti et al., 2023). A second studies longer-run educational attainment and schooling outcomes (Ermisch and Francesconi, 2013; Mosca et al., 2021). A third focuses on skill formation more directly, focusing on cognitive and socio-behavioral outcomes (Baum, 2004; Peter, 2016; Agostinelli and Sorrenti, 2021). Taken together, this literature does not deliver a single sign: maternal employment can reduce maternal time with children, but it can also raise family resources and change the type of non-parental care received, so estimated effects depend on the child's age, the employment margin affected, and the relevant counterfactual care arrangement. Causal evidence on adult outcomes remains comparatively thin, as few papers in this literature follow children into adulthood.

A related literature study parental leave and maternal time at home around birth, rather than maternal employment per se. This strand spans a wide range of outcomes—school performance, educational attainment, and early health and socio-emotional development (Liu and Nordström Skans, 2010; Danzer and Lavy, 2018; Rasmussen, 2010; Dahl et al., 2016; Dustmann and Schönberg, 2012; Ginja et al., 2020; Rossin, 2011; Baker and Milligan, 2010, 2015; Huber, 2019; Huebener et al., 2019; Albagli and Rau, 2019; Houmark et al., 2024; Canaan, 2022)—and earlier U.S. evidence specifically links very early returns to work to worse infant health and developmental outcomes (Berger et al., 2005). Fewer papers trace effects into adulthood, although some do examine later health and labor-market outcomes (Danzer et al., 2022b; Heisig and Zierow, 2025). The central lesson for our purposes is that effects appear to depend critically on what maternal time at home replaces: moderate leave extensions tend to have small average effects on grades and attainment, but can matter for health and socio-emotional development, while very long extensions need not be beneficial.

A third relevant strand examines child care and the availability of non-parental care—particularly relevant here given that formal care was limited in Sweden when the 1971 reform shifted mothers into employment. This includes structural work on mothers' employment and child-care choices (Bernal, 2008; Bernal and Keane, 2011) and a large quasi-experimental literature studying cognitive and developmental outcomes (Drange and Havnes, 2019; Felfe and Lalive, 2018; Cornelissen et al., 2018; Fort et al., 2020), school performance and longer-run educational outcomes (Black et al., 2014; Felfe et al., 2015; Datta Gupta and Simonsen, 2016; Havnes and Mogstad, 2011)m and adult outcomes, including earnings, labor-market participation, welfare dependence, health, and crime (Havnes and Mogstad, 2011, 2015; Baker et al., 2019; Herbst, 2017). The central lesson is that effects depend on what formal care replaces and on program quality. High-quality expansions that displace informal or lower-quality alternatives tend to improve outcomes, especially for disadvantaged children (Havnes and Mogstad, 2011, 2015; Cornelissen et al., 2018). When formal care instead substitutes for parental care, however, the evidence is more mixed: some expansions still improve later cognitive and schooling outcomes (Felfe et al., 2015), whereas others generate persistent non-cognitive losses (Baker et al., 2008, 2019), and the returns to public programs more generally can differ sharply when close substitutes exist (Kline and Walters, 2016). Home-care reforms that jointly reduce maternal employment and formal care are best treated as adjacent to this

literature (Bettinger et al., 2014). Our paper connects to all three literatures, but it is closest to the maternal-employment strand because the 1971 reform primarily shifts mothers' labor supply; the parental-leave and child-care literatures are nonetheless central for interpreting mechanisms. Together, these three strands motivate our focus on the still-limited causal evidence tracing early-life changes in maternal labor supply and care arrangements into adult outcomes.

A further related literature studies the intergenerational transmission of gender norms and female labor supply. Daughters of working mothers are more likely to work (McGinn et al., 2019; Olivetti et al., 2020) and experience smaller child penalties (Kleven et al., 2019). Moreover, men raised by working mothers are more likely to have working wives (Fernández et al., 2004; Bütikofer, 2013; Schmitz and Spiess, 2022). Mothers' attitudes predict daughters' and daughters-in-law's labor supply even when the mother herself did not work (Farré and Vella, 2013; Johnston et al., 2014), and predict children's attitudes in developing-country settings (Dhar et al., 2019). Sons of working mothers also undertake more family care (McGinn et al., 2019). Moving beyond maternal employment to paternal home involvement, Farré et al. (2023) exploit the introduction of paternity leave in Spain in a local difference-in-differences design and show that children of eligible fathers hold more gender-egalitarian attitudes, perceive less stereotypical social norms, and engage in less gender-stereotyped behavior—causal evidence that counter-stereotypical parental behavior shapes the next generation's norms.

Our work is also related to a large literature estimating female labor supply elasticities, especially studies exploiting tax reforms that shift between joint and individual taxation (Crossley and Jeon, 2007; LaLumia, 2008; Selin, 2014; Kalíšková, 2014). We build most directly on Selin (2014), who exploits the same 1971 Swedish reform to estimate participation elasticities for married women, using dummies for deciles of husbands' pre-reform income as instruments for changes in the participation tax rate. We extend this approach in two directions: we more precisely isolate the tax rate changes driven by the joint-to-individual shift, and we estimate elasticities along both the intensive and extensive margins. Our hours elasticity of 1.3 and extensive margin elasticity of 0.5 are broadly consistent with estimates from the EITC literature (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Eissa and Hoynes, 2004), though above more recent estimates (Kleven, 2024). The 1971 reform is particularly well-suited for this purpose as it generated large and salient changes in married women's marginal tax rates, which matters given that adjustment frictions may cause small reforms to understate true elasticities (Chetty, 2012a).

# 1 Data and Childcare Around 1971

## 1.1 Data, Sample, Variables

**Datasets** Our analysis draws on several linked administrative registers maintained by Statistics Sweden, identified through the Swedish personal identity number.

Family structure and intergenerational links are constructed from the Multigenerational Register (Flergenerationsregistret) (Statistics Sweden, 2017a), which links all individuals in Sweden to

their biological parents and covers the full population. This register is the basis for constructing our sample of mothers, their children, and the third generation.

Data on maternal employment and working hours around the time of the reform come from the Population and Housing Censuses (Folk- och bostadsräkningarna, FOB; Statistics Sweden, 1990), conducted every five years between 1960 and 1990. The censuses collected detailed information on employment status, hours of work, occupation, and household composition from the near-universe of the Swedish population, with near-complete response rates due to mandatory participation.

Income and employment outcomes are drawn from the Income and Taxation Register (Inkomst- och taxeringsregistret, IoT), which provides annual information on income, taxes, and transfers from 1968 onwards.

Educational attainment, field of study, and parental leave take-up are drawn from the Longitudinal Integration Database for Health Insurance and Labour Market Studies (LISA; Statistics Sweden, 2020), which covers all Swedish residents aged 16 and above on an annual basis from 1990 onwards. LISA integrates data from the Education Register (Statistics Sweden, 2017b), the Income and Taxation Register, and records from the Swedish Social Insurance Agency, including parental leave days.

Cognitive and non-cognitive skills are measured for men using data from the military enlistment register (Swedish National Archives, 2005). Until the abolition of mandatory conscription, all Swedish men underwent enlistment testing at age 18, comprising assessments of cognitive ability across four subtests—inductive reasoning, spatial ability, technical understanding, and verbal comprehension—as well as non-cognitive traits assessed through a structured interview with a certified psychologist. The overall non-cognitive score reflects social maturity, psychological energy, intensity, and emotional stability (Mood et al., 2012).

School performance for the third generation is measured using results from national standardized exams taken at the end of compulsory schooling (9th grade, age 16), administered by the Swedish National Agency for Education (Ramstedt, 2005). These exams are high-stakes, as GPA determines eligibility for oversubscribed upper-secondary programs.

**Sample** Our primary sample consists of first- and second-born sibling pairs in which the younger sibling was born between 1958 and 1970 and was therefore at most 12 years old at the time of the reform. We further restrict the sample to families in which parents filed taxes jointly in 1970 and where the father had positive labor income. Individuals who do not appear in the tax registry between ages 30 and 50 are excluded. These are individuals who either died before age 50, emigrated, or for some other reason did not earn taxable labor income during those ages. As discussed in Section 3.3, the baseline analysis is limited to same-sex siblings, since the identifying assumptions are violated for opposite-sex sibling pairs. Throughout the analysis, we define the preschool-age sample as families in which the younger sibling was below age 7 at the time of the reform, and the primary school-age sample as families in which the younger sibling was between ages 7 and 12.

**Variable Definition** Lifetime income is defined as the mean of an individual's total labor income between ages 30 and 50, with income measures deflated to 1970 prices. This age range is the longest range of post-education adulthood life we can observe in the data for the youngest cohorts in our sample. Educational attainment is measured by the highest completed degree as of 2020. Because maternal labor supply decisions are influenced by the tax schedule applied to the father's earnings, we construct a measure of expected paternal earnings in 1970 that accounts for transitory income fluctuations and mean reversion. Specifically, we estimate a reduced-form income process in which fathers' earnings in 1970 are regressed on their lagged earnings in 1968 and 1969. To allow for heterogeneous income dynamics, we interact lagged earnings with indicators for the father's education category and year of birth. The resulting fitted value serves as our measure of expected paternal earnings and is used throughout the analysis.[1]

## 1.2 Childcare in Sweden Around the 1971 Tax Reform

In the late 1960s and early 1970s, the period before the expansion of public childcare in Sweden, childcare was organized through a mixed system of formal and informal arrangements. To document the prevalence of different childcare arrangements at the time of the reform and to put our findings into context, we use data from a nationally representative interview survey conducted in autumn 1966 (SOU, 1967). To our knowledge, this survey provides the best available evidence on childcare use prior to the 1971 tax reform. The childcare survey was implemented as a special supplement to Statistics Sweden's Labor Force Survey and covered children whose custodial parent(s) worked at least 15 hours per week. For each child, the survey recorded the main childcare arrangement in use at the time of the interview. In the main text, we focus on childcare availability for preschool-aged children (below age 8), while Appendix C extends the analysis to children below age 10 and provides further institutional details on childcare provision during this period.[2]

Public childcare comprised both center-based childcare institutions (*barnstugor*), which existed in several forms differing in time coverage and function, and municipal family daycare (*kommunala familjedaghem*), in which children were cared for in private homes by childminders under municipal organization and supervision. Public provision of childcare, however, was limited and highly rationed, covering only 18 percent of children (Figure 3a). As a result, the majority of working parents relied on alternative arrangements to meet their childcare needs.

There were two important alternatives. The first option was private family daycare (*privata familjedaghem*), which comprised 23 percent of children of working parents. These consisted of home-based care similar in form to municipal family daycare but operated without municipal oversight or financial support. Family daycare settings—both municipal and private—typically

[1] Appendix Figure A.2 shows the expected labor income against the actual income in 1970. The correlation is high and around 0.98.

[2] We exclude from the sample children who are cared for by one of the two parents. The full breakdown of survey responses is available for the survey population, which are children aged 0–10, and partially for the subset of younger children aged 0–7. To break down the share of children taken care of by grandmother and relatives out of children at home or in private childcare, we assume the same proportion for children 0–7 as children aged 0–10.

involve a single childminder caring for a small group of three to five children.

The second option was grandparents, or, according to the survey, grandmothers, who cared for more than 23 percent of children.[3] An additional 8 percent were cared for by other relatives or siblings. The third main option was privately arranged childcare in the child's own home, including nannies and domestic employees (*hemhjälp*). The remainder of the children were taken care of by neighbors or friends, or the children were left on their own.

The observed reliance on informal childcare arrangements is most likely to have reflected the scarcity of public childcare rather than parental preferences. Direct evidence on this comes from a survey conducted by the Preschool Commission (*Barnstugeutredningen*) in 1972, after the reform (SOU, 1972). Among children aged 0–6 with working mothers, 68 percent were reported to demand a place in either daycare centers or family daycare. Moreover, stated preferences were strongly tilted toward public provision: conditional on demanding childcare, 48 percent of parents expressed a preference for a place in a daycare center, and 24 percent in family daycare.

# 2 The 1971 Tax Reform and Its Effect on Female Labor Supply

## 2.1 Transition from Joint to Individual Taxation

**The pre-reform system.** Prior to 1971, married couples in Sweden were taxed jointly at the central-government level. When income taxation was introduced in 1902, joint taxation reflected prevailing legal and social norms: husbands were the legal guardians of their wives, and the labor market participation among married women was low (Nyberg, 2017). This remained the operative regime for half a century. A reform in 1952 replaced the single schedule with two separate federal schedules for married couples and singles. While this reform eliminated the so-called "marriage penalty" that arose when both spouses had earnings, it simultaneously created a strong disincentive for wives to work as under the new system the wife's earnings were effectively stacked on top of her husband's as a tax base. The key mechanical consequence of this system was that a wife's tax rate was an increasing function of her husband's earnings. Her income was immediately taxed the couple's marginal tax bracket, even at low levels of her own earnings.

Two additional features of the tax system influenced the effective tax rate faced by married women. First, while local income taxes were levied at a flat rate, they interacted with the progressive central-government tax schedule as they were deductible against the central government tax base, attenuating effective tax progressivity. Second, a work deduction (*förvärvsavdrag*) applied to women with positive earnings. For married women with children below age 16, the deduction equaled 25 percent of earnings up to a threshold, partially offsetting statutory marginal rates for this group. Despite this deduction, the marginal and participation tax rates faced by wives remained strongly increasing in their husbands' income under joint taxation.

[3] This share reflects both care by a grandmother in the child's home or as part of private family daycare organized by the grandmother in her own home.

**Political and economic motivations.** Three distinct arguments converged in favor of individual taxation. The push was initiated by highly educated women who argued that joint taxation was unfair and disproportionately benefited high-income households where the wife did not work (Elvander, 1972; Nyberg, 2017). Political parties were initially divided and indecisive, but during the 1960s all came to endorse individual taxation except the Conservative Party (Elvander, 1972). A second argument was that economic forecasts predicted that sustaining growth would require a substantial increase in labor supply, and married women were identified as the primary untapped reserve. This argument ultimately proved decisive with the Social Democratic Minister of Finance (Elvander, 1972). Third, and perhaps most importantly for the governing Social Democrats, it was shown that the deductions available under joint taxation disproportionately benefited high-income families, allowing the reform to be framed primarily as a redistribution measure. Prime Minister Olof Palme publicly described the move to separate taxation as first and foremost a reform to benefit low-income workers, while also emphasizing its contribution to women's independence (Nyberg, 2017). With all three arguments pointing in the same direction, the reform was passed in 1971.

**The reform.** The reform was announced in a government bill released in March 1970 and took effect on January 1, 1971. In an earlier reform in 1966, optional individual taxation had been introduced for the central government tax and pension insurance fee, but take-up was only around 5 percent of couples and local taxes remained unaffected (Selin, 2014). For the vast majority of married households, joint taxation thus remained binding until the 1971 reform. In our main analysis, we restrict the sample to children whose parents jointly filed taxes in 1970.

Effective January 1, 1971, each spouse's labor earnings were taxed separately under a unified progressive schedule that did not depend on partner income. Unearned income remained jointly taxed until 1988, and the last family-based elements of the income tax were not fully eliminated until the major reform of 1991, but labor income—the relevant margin for employment decisions—was fully individualized from 1971.

Three further structural changes accompanied the reform. First, the deductibility of local taxes from the central government tax base was abolished, substantially increasing effective progressivity. Second, a spousal tax reduction was introduced to partially compensate one-earner households. Third, the work deduction was retained but made gender-neutral, applying to the secondary earner rather than specifically to wives, but was set in nominal terms and therefore declining in real value with inflation over subsequent years before being abolished in 1991. Detailed statutory formulas for deductions, thresholds, and tax reductions are provided in Online Appendix B.

Appendix Figure A.3 illustrates the implied marginal tax schedules for a married woman with two children under the statutory rules. Panel (a) shows the 1970 schedule under joint taxation. For any given level of the wife's earnings, the marginal tax rate (MTR) increases with the husband's income, generating a substantial secondary-earner tax wedge and sharply reducing the net return

to work in high-income households. Panel (b) shows the 1975 schedule following the reform. The wife's MTR no longer depends on her husband's earnings and depends only on her own income, lowering the MTR for almost everyone.

Comparing the two panels shows that most (above 99%) of all mothers face a tax reduction due to the reform. Our empirical design leverages the treatment heterogeneity of this reform, i.e., the difference across lines in panel a (see Section 3). Low- and high-earner husbands correspond to P10 and P90. In 1970, median incomes of married women and men were about 4586 and 27505, respectively (5.2 SEK/USD). In 1975, the corresponding figures were 7209 and 29701 (4.1 SEK/USD). All numbers are in SEK of 1970. There have been minor changes in the tax schedule between the 1971 reform and 1975—the year we are able to measure mothers' labor supply.

The reform, therefore, eliminated the mechanical link between husband's income and the wife's tax rate, substantially weakening work disincentives for married women in high-income families. In net terms, wives married to husbands at the top of the earnings distribution experienced reductions in participation tax rate of approximately 40 percentage points, while wives of lower-income husbands experienced little change or small increases (Selin, 2014).

## 2.2 Effect on Married Women's Labor Supply

Our identification strategy combines between-family variation in exposure to the reform—driven by differences in mothers' marginal tax rates under joint taxation—with within-family variation arising from siblings' differential exposure to the timing of the reform.

Figure 4 presents the first step of this estimation, using between-family variation to estimate the effect on mother's labor supply. Panel (a) plots the simulated, or mechanical, change in married women's marginal net-of-tax rate (MNTR) between 1970 and 1975 against their husbands' labor income in 1970.[4] For this purpose, we built a tax calculator that we describe in Appendix B. We calculate each woman's MNTR before and after the reform using the pre- and post-reform tax schedules.

A key challenge in measuring exposure to the reform is that post-reform tax rates are mechanically related to income. We overcome this challenge by following the simulated-tax-rate approach of Gruber and Saez (2002), in which we measure the wife's marginal net-of-tax rate under the post-reform 1975 tax schedule using her pre-reform income from 1970. This isolates the change in the tax rate from any behavioral response to the reform. To measure the mother's marginal net-of-tax rate in 1970 using her 1970 income and her husband's income.

One concern is that mean-reverting shocks in husbands' pre-reform income generate a spurious relationship between measured tax exposure and subsequent changes in wives' labor supply. To address this, we follow the logic of Weber (2014) and Sigurdsson (2025) and predict 1970 income of husbands using their income in 1969 and 1968 (see Figure A.2 for the result of this prediction).

[4]We use 1975 as the post-reform year in this calculation, as we have data on mothers' working hours from the population census every five years, available both in 1970 and 1975. As an alternative measure of labor supply available every year we use employment.

A mother's marginal net-of-tax rate under the pre-reform joint tax schedule depended on both spouses' labor income, that is $\tau_{\text{pre}}(y_{\text{pre}}, y^f_{\text{pre}}; \sigma_{\text{pre}})$ where $\sigma$ denotes the tax-relevant family composition. The corresponding marginal net-of-tax rate under the post-reform individual tax schedule is $\tau_{\text{post}}\left(y_{post}; \sigma_{\text{post}}\right)$, which no longer depends on the father's income.

As discussed earlier, the simulated reform-induced change in the mother's marginal net-of-tax rate holds pre-reform income fixed and therefore removes behavioral responses to the reform from the tax-rate change measure, that is,

$$\Delta\tau^s := \tau_{\text{post}}\left(y_{pre}; \sigma_{\text{pre}}\right) - \tau_{\text{pre}}\left(y_{pre}, y^f_{pre}; \sigma_{\text{pre}}\right),$$

which is depicted in Figure 4, Panel a. The instrumental variable is the component of this simulated tax change predicted by the father's pre-reform income[5],

$$Z := \mathbb{E}\left[\Delta\tau^s \mid y^f_{pre}\right]. \tag{1}$$

Panel (a) shows that the reform increased the net-of-tax return to work more for women married to higher-earning husbands. Under joint taxation, a married woman's MTR depended on her husband's earnings; as a result, the move to individual taxation generated larger tax cuts, and thus larger increases in the net-of-tax rate, for women with higher-income husbands.

Panel (b) plots the change in married women's weekly hours of work between 1970 and 1975 against their husbands' labor income in 1970. Maternal labor supply rises with the husband's pre-reform income, closely mirroring the pattern in panel (a). This indicates that the tax reform generated economically meaningful increases in married women's labor supply, with the strongest responses concentrated among women who faced the largest increases in the return to work. Because the outcome is weekly hours, and non-employment is recorded as zero hours, the measure captures responses at both the intensive and extensive margins.

Panel (c) combines the information in Panels (a) and (b) by relating the change in married women's weekly hours to the change in their marginal net-of-tax rate resulting from the transition from joint to individual taxation. The reform-induced change in the net-of-tax rate is the predicted component from a non-parametric projection of the simulated tax change on the husband's predicted pre-reform income. This isolates the change in married women's net-of-tax rate resulting from the reform from other changes in the tax code from 1970 to 1975.

The relationship is strongly positive and approximately linear. The estimated slope of 23.4 (s.e. 0.456) implies that a 1 percentage point increase in the marginal net-of-tax rate is associated with about 0.22 additional weekly hours worked between 1970 and 1975. This linear relationship supports using reform-induced changes in the mother's marginal net-of-tax rate, driven by the husband's predetermined income position, as a continuous measure of treatment intensity in the empirical analysis.

[5]To reduce mean-reversion concerns, we use predicted 1970 father income, constructed from his 1968 and 1969 earnings as explained in Section 1.1

The implied labor supply elasticity is 1.21. To see this, note first that the estimated slope in Panel (c) implies that a one percentage point increase in the MNTR is associated with approximately 0.23 additional weekly hours of work. Equivalently, a one percentage point decline in the marginal tax rate yields the same increase in hours. The implied elasticity is $\frac{\Delta H/H}{\Delta(1-\tau)/(1-\tau)}$, where $H$ denotes working hours and $\tau$ the marginal tax rate. The slope in Panel (c) estimates the change in hours for a one-percentage-point change in the net-of-tax rate. Using average pre-reform hours of 11.8 and an average marginal tax rate of 38.7 percent, we obtain a labor supply elasticity of 1.21, which reflects both intensive- and extensive-margin adjustment.

The implied extensive-margin elasticity can be calculated similarly as $\frac{\Delta E/E}{\Delta(1-\tau^p)/(1-\tau^p)}$, where $E$ denotes employment and $\tau^p$ the participation tax rate. An estimated employment effect of 0.5 implies that a one percentage point decline in the participation tax rate is associated with roughly a 0.5 percentage point increase in the employment rate. Relative to the pre-reform employment rate of 0.57 and an average participation tax rate of 31.7 percent, this yields an extensive-margin elasticity of 0.60.

These elasticities are in line with, and in some cases larger than, existing estimates of married women's labor supply responses to tax changes. One plausible reason is that the Swedish reform generated a large and salient change in work incentives. In the presence of adjustment costs, larger reforms are likely to induce stronger behavioral responses than smaller, incremental tax changes (Chetty, 2012b). Related evidence from other settings likewise points to meaningful labor supply responses among married women (Crossley and Jeon, 2007; Selin, 2014; Kalíšková, 2014), although not all studies can map those responses into directly comparable elasticities. For example, LaLumia (2008) estimates that employment of married women in states adopting joint taxation decreased by 2 percent, although the estimated elasticity is not possible due to a lack of data on MNTR.

Three questions are central for interpreting these results and for motivating our empirical framework for studying the effect on children. First, is the estimated response truly driven by the reform, or would wives of high-income husbands have increased their labor supply even in the absence of the tax change? Panel (d) addresses this concern using an event-study difference-in-differences specification in which treatment intensity is given by the reform-induced change in the mother's tax incentive. Let $E_t^m$ be an indicator for whether the mother has positive labor earnings in year $t$, and let $\Delta\tau^m$ denote the simulated reform-induced change in her MNTR. We estimate

$$E_t^m = \mu^m + \sum_{s \neq 1970} \delta_s \mathbf{1}\{t = s\} + \sum_{s \neq 1970} \pi_s \mathbf{1}\{t = s\}\Delta\tau^m + \lambda_{b_t^m t} + \varepsilon_t^m. \tag{2}$$

The omitted year is 1970, so the coefficients $\pi_s$ measure year-specific employment responses relative to the last pre-reform year. The specification includes mother fixed effects $\mu^m$ and flexible controls for mothers' age, $\lambda_{b_t^m t}$, where $b_t^m$ denotes the mother's age bin in year $t$ and the term is saturated over year-by-age-bin cells. We instrument the full set of year-specific tax-incentive terms, $\mathbf{1}\{t = s\}\Delta\tau^m$, with interactions between year indicators and deciles of the father's 1970 labor earn-

ings, matching the source of variation created by joint taxation before the reform. Since working hours are only observed in the population census every five years, the annual event study uses employment measured from tax records.

Alternative

Since working hours are only observed in the population census every five years, the annual event study uses employment measured from tax records. We use an indicator for whether the mother has p$E_t^m$,ositive labor income, $E_t^m$, in the following specification:

$$E_t^m = \mu^m + \delta_t + \phi(a_t^m) + \pi_t \Delta\tau^m + \varepsilon_t^m. \tag{3}$$

where $\phi$ is a flexible control for mother age at reform (we use decile). The omitted year is the last pre-reform year, $\pi_{1970} = 0$, so the coefficients $\pi_t$ measure year-specific employment responses relative to 1970. We instrument year-specific tax-incentive terms, $\mathbf{1}\{t = s\}\Delta\tau^m$, with interactions between year indicators and deciles of the father's 1970 labor income, matching the source of variation created by joint taxation before the reform.

There is no evidence of differential pre-trends before the reform. Employment rises sharply in 1971 for women, who experience larger increases in their marginal net-of-tax rate, with increases continuing over the next few years and then remaining persistently positive. This pattern is consistent with a reform-driven labor-supply response rather than a continuation of pre-existing trends.[6]

Second, did husbands offset their wives' increased labor supply by reducing their own labor supply? Appendix Figure A.9b presents the corresponding results for married men. In contrast to the pattern for women, the change in married men's tax rates is broadly unrelated to their pre-reform income. Moreover, while married men work slightly fewer hours on average after the reform, there is no systematic relationship between husbands' labor-supply response and their pre-reform income. This suggests that husbands did not compensate for their wives' increased hours by reducing their own labor supply.

Third, was the labor-supply response persistent? Appendix Figure A.4 shows that women who entered the labor market following the reform tended to remain employed. Persistence is high for all entrants, but even stronger among wives of higher-income husbands, who experienced the largest increase in work incentives. This persistence is an important foundation for the empirical strategy developed below.

## 3 Empirical Framework

This section develops the empirical framework for estimating the causal effect of mothers' labor supply during childhood on children's long-run outcomes using the 1971 tax reform.

[6] Appendix Figure A.5 further shows that the results are robust to using either the marginal tax rate or the participation tax rate to measure tax incentives.

### 3.1 Data-Generating Process and Estimand

A long-run outcome of a child, $Y$, is a function of nature (genes, gender, etc.) and nurture (environment, education, etc.). Our object of interest is a particular component of the latter: the mother's labor supply during childhood, $L := (l_1, \ldots, l_{\bar{a}})$, where $l_a$ denotes the mother's labor supply when the child is age $a$. The DGP can be written as

$$Y \;=\; F\big(\underbrace{N}_{Nature}, \underbrace{E\,,\,L}_{Nurture}\big), \tag{4}$$

where vector $E$ collects all remaining nurture inputs other than maternal labor supply.[7]

We are interested in the marginal effect of a persistent increase in maternal labor supply that begins at age $a$ on a child's outcome, holding all other inputs fixed. The estimand is the directional derivative, where the direction is a threshold (step) vector $\mathcal{I}_a := (\mathbf{0}_a,\, \mathbf{1}_{\bar{a}-a})$, that is:

$$\gamma_a^g := \lim_{\varepsilon \to 0} \frac{F\Big(N, E, L + \varepsilon \mathcal{I}_a\Big) - F\Big(N, E, L\Big)}{\varepsilon}, \tag{5}$$

The directional derivative is a function of all inputs, $(N, E)$, but we allow it to only vary by gender. In this sense, we take the expectation and omit it.

The *realized* outcome of a child who is exposed to the reform at age $a$ is

$$Y \;=\; F\Big(N, E^{T=1}, L^{T=1}\Big) = F\Big(N, E^{T=1}, L^{T=0} + dl\; \mathcal{I}_a\Big).$$

where $dl$ is the persistent treatment effect on the mother's labor supply and $T = 1$ and $T = 0$ denote the treated and untreated potential outcomes, respectively. Nature inputs by definition do not depend on treatment status.

A first-order Taylor expansion around zero yields the local approximation

$$Y \approx F\Big(N, E^{T=1}, L^{T=0}\Big) + \gamma_a^g\, dl, \tag{6}$$

where the first term is the potential outcome in the presence of the reform but in the absence of the reform-induced change in maternal labor supply.

### 3.2 Research Design

The key identification challenge is that $dl$ is not randomly assigned in observational data. Our research design addresses this endogeneity in two ways. First, it exploits cross-family variation in exposure to the reform. We instrument the exposure dose using the reform-induced simulated change in the mother's tax incentive, as explained by her husband's pre-reform income. This is

[7] We specify $\bar{a}$ as an age threshold beyond which maternal labor supply no longer affects children's outcomes. Our empirical strategy does not require specifying a specific age threshold, but its existence can be tested, or it can provide a placebo test.

exactly the identification method used to measure mothers' labor supply elasticity in Figure 4.

The instrument $Z$ defined in equation (1) isolates the tax-rate change due to the tax reform from other changes in the tax code. We compute the conditional expectation for 200 bins of the father's pre-reform income. The resulting instrument is on the x-axis of Figure 4, Panel c.

Despite generating exogenous variation in mothers' labor supply, exposure to the reform may be correlated with family characteristics, including fathers' income, generating endogeneity in (6). The second part of our research design address this by combining the cross-family differences in exposure to the reform with within-family differences in exposure to the reform. Siblings in the same family share the same $dl$, but they differ in their ages when the reform occurs. We illustrate the two sources of variation that our research design leverages in Figure 5.

$i$ and $i'$ denote two siblings from the same family, with $i$ the younger. $a(i)$ and $a(i')$ denote their respective ages at the time of the reform. Sibling difference with a linearization with a Taylor expansion and ignoring the life-cycle of labor supply of equation 6

$$\Delta Y \approx (\gamma_{a(i)}^{g(i)} - \gamma_{a(i')}^{g(i')})\, dl + \Delta X, \tag{7}$$

where $X_i$ is $(N, E)$.

Our sibling design identifies the *exposure effect* at age $a$ for gender $g$, defined as:

$$\beta_a^g := \gamma_a^g - \gamma_{a+1}^g \tag{8}$$

. Because siblings share the same maternal labor-supply response but experience it at different childhood ages, sibling differencing removes effects common to both children and isolates differences arising from age at exposure. Thus, for same-sex siblings exposed at ages $a < a'$, the design identifies $\gamma_a^g - \gamma_{a'}^g = \sum_{r=a}^{a'-1} \beta_r^g$, rather than the level effect $\gamma_a^g$. In particular, $\beta_a^g$ measures the effect of one additional year of exposure to increased maternal labor supply starting at age $a$ rather than at age $a+1$.

Focusing on the same sex siblings, we posit the following regression:

$$\Delta Y = dl \sum_{r=a(i)}^{a(i')-1} \beta_r^{g(i)} + \theta \Delta X + \lambda_{a(i),a(i')} + \varepsilon. \tag{9}$$

Constant effect for similar age group, $\beta_{a(i)}^{g(i)} = \beta_a^{g(i)}$ for all $a \in \{a(i), a(i') - 1\}$

$$\Delta Y = dl \Delta a\, \beta_a^{g(i)} + \theta \Delta X + \delta_{a(i),a(i')} + \varepsilon. \tag{10}$$

We estimate equation (10) by instrumenting $dl$ with $Z$. The first stage is

$$dl \Delta a = \pi Z + \lambda_{a(i),a(i')} + \nu. \tag{11}$$

### 3.3 Identification Assumptions

This sibling-difference IV design rests on four assumptions.

**Exclusion.** Holding the family fixed, the reform-induced tax change affects the sibling difference in outcomes only through the mother's labor supply: $\frac{\partial \Delta Y}{\partial Z}\big|_{dl} = 0$ or

$$\frac{\partial \Delta X}{\partial Z} \;=\; \frac{\partial}{\partial Z}\Delta F\big(N,\, E^{T=1},\, L^{T=0}\big) \;=\; 0. \tag{12}$$

**Independence.** Conditional on the siblings' ages at the reform, the instrument is as good as randomly assigned across families:

$$\mathbb{E}\big[\varepsilon \,\big|\, Z,\, a(i),\, a(i')\big] \;=\; \mathbb{E}\big[\varepsilon \,\big|\, a(i),\, a(i')\big]. \tag{13}$$

**Relevance.** The instrument shifts mothers' labor supply: $\mathrm{Cov}(Z, dl) \neq 0$

**Monotonicity.** The instrument moves maternal labor supply in the same direction across families, so that there are no "defiers." In potential-outcomes notation for the first stage, this can be stated as $dl_f(Z_f = z_2) \;\geqslant\; dl_f(Z_f = z_1)$ for $z_2 > z_1$ and for all $f$, with the inequality reversed if higher $Z_f$ weakens the incentive to increase labor supply.

Under these conditions, the IV estimand recovers a local average exposure gradient, linking within-family differences in age at reform to exogenous cross-family differences in the reform-induced change in maternal labor supply.

## 4 Theoretical Framework

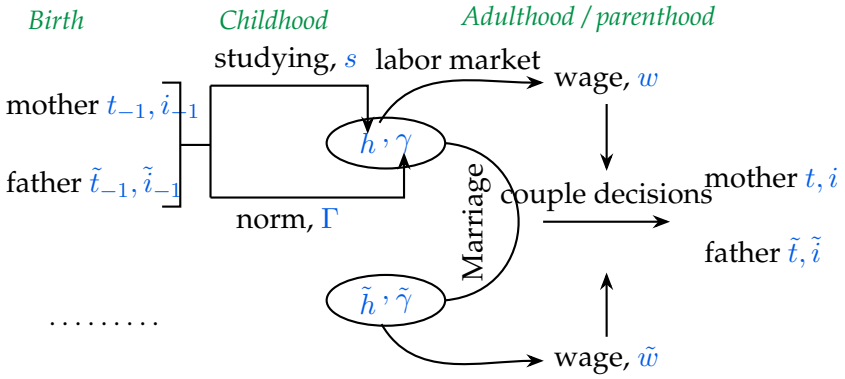



Figure 1: Life-cycle structure of the model.

We develop an overlapping-generations model to discipline our thinking about how a change in taxation of maternal labor income propagates across generations. Each generation consists of households that invest time and money in their children, who in turn accumulate human capital,

sort into couples, and invest in their own children. The model has two novel features relative to the standard human capital investment framework (**?**). First, both parents invest in the child, and the within-couple allocation of childcare affects not only the child's human capital but also an inherited preference for within-couple equality transmitted to the next generation. Second, the marriage market ..., so the return to human capital depends on the marriage market....

### 4.1 Setting

The economy is populated by overlapping generations of two-parent households. Each individual lives for two stages: childhood and adulthood. Individuals derive utility from consumption and leisure during childhood and parenthood.

Gender, assortative, discrimination. Here, we abstract and simplify the matching rate; in this way, we do not model men and husbands; we delegate that to an appendix.

In childhood, the individual receives parental inputs: receives investment in time and money $(T_{-1}, I_{-1})$. Given these investments, Children's choose their study effort $s$ shaping their human capital,

$$h = \mathcal{H}(s, T_{-1}, I_{-1}, H_{-1}, \sigma), \tag{14}$$

where $H_{-1}$ denotes parental human capital and $\sigma$ denotes the quality of available non-parental care, e.g., the presence of grandparents or the care market. This is a generalization of human capital models, e.g., Boca et al. (2026); Berlinski et al. (2024), building on the seminal work of Cunha and Heckman (2007), by allowing a non-linear functional form and including non-parental care.

Childhood time is allocated between leisure and study, i.e., studying comes at the cost of leisure. One specific form $(s, I_{-1}, T_{-1}H_{-1} + (1 - T_{-1})\sigma)$. Human capital translates into adult wages through $w = W(h)$ for women, where $W_g$ is strictly increasing.

The next generation's equality taste is a function of the couple-equality among their own parents $\gamma = \Gamma(E_{-1})$, where $\Gamma(\cdot)$ is strictly increasing. Importantly, we assume a gender-neutral norm transmission.

The first thing an adult does is to marry. We assume that woman $(w, \gamma)$ marries a husband $(m(w), \gamma)$, where $m(x) > x$ for any $x$. A couple then decides their consumption and leisure decisions, as well as investments $(T, I, E)$ for the next generation. We can separate the couple's decision of time and children investment from individual decisions given those. Start with the latter:

Each adult individual chooses consumption, leisure, and labor supply $(c, \ell, l)$, given wage $w$ and time spent elsewhere (devoted to studying in childhood or children in adulthood $t$), money spent elsewhere $x$ (net within family transfer, minus of standrad unearned income):

$$v(w,\, t,\, x) \;:=\; \max_{c,\ell,l}\; u(c,\, \ell) \tag{15}$$

subject to budget and time constraints $c + c_0 + x = wl$, and $t + \ell + l = 1$.

Adult household allocations are selected by fixed Pareto weights $(\eta_w, \eta_m)$, where $\eta_w + \eta_m = 1$. This is the second dimension of gender differences. Denote by $V(T, I, \gamma)$ the value associated with a child raised under parental inputs and norm $(T, I, \gamma)$. The adult woman's problem can then be written compactly as:

$$\mathcal{U}(w, \gamma) \;:=\; \max_{t, \tilde{t}, i, \tilde{i}} \eta \left[ v\big(w,\, t,\, i\big) + \gamma E \right] \;+\; \tilde{\eta} \left[ v\big(\tilde{w},\, \tilde{t},\, \tilde{i}\big) + \tilde{\gamma} E \right] + \beta V(T, I, \gamma_{+1}), \tag{16}$$

subject to $\gamma_{+1} = \Gamma(E)$, $E = e(t, \tilde{t})$, and couple pecuniary and time investments in kids are $T = t + \tilde{t}$, and $I = i + \tilde{i}$, respectively. Within couple transfer are redundant, so ignored. The value $V(.)$ includes the child's own childhood payoff and the continuation value of the efficient household the child forms in adulthood. Hence, a child solves:

$$V(T_{-1}, I_{-1}, \gamma) = \max_{s} \; u(c_0, 1 - s) + \delta \mathcal{U}\big(w, \gamma\big) \tag{17}$$

subject to wage equation $w = W(h)$ and human capital condition (14).

Now consider a reform that increases the second-earner wage of a couple. This is equivalent to a change in $w$ in the couple problem (16). We take it from the quasi-experimental couple labor-supply literature, in particular the EITC literature, in addition to our own findings, to sign many comparative statics that are theoretically ambiguous. statics that are theoretically ambiguous. To start, we assume that higher net wage leads to higher hours worked and income, $\partial_w l > 0$ and $\partial_w z > 0$, but the spouse does not react, $\partial_w \tilde{l} \approx 0$ and $\partial_w \tilde{z} \approx 0$. A natural conjecture is that time working comes from both time spent on leisure and time invested in kids, $\partial_w t < 0$ and $\partial_w \tilde{t} \approx 0$, so $\partial_w T < 0$. The sign of $\partial_w I$ is theoretically ambiguous. It is positive under weak time-money complementarity or strong income/resource effects, $\partial_w I > 0$, as EITC literature has found. Theoretically ambiguous whether higher time investment or lower money investment prevails, so we take it from EITC or our empirical results that $\partial_w h_{+1} < 0$, which leads to lower wages and lower wage spouse for the next generation.

Applying the same logic to the next generation, they should work less, earn less, and invest less time but more money in their kids,

$$\partial_w t_{+1} > 0 > \partial_w l_{+1}, \partial_w z_{+1}.$$

and $0 > \partial_w I_{+1}$

$\partial_w E > 0$ implies $\partial_w \gamma_{+1} > 0$, means

$$\begin{aligned} \partial_w t_{+1} > 0 > \partial_w l_{+1}, \partial_w z_{+1} &\text{for Sons} \\ \partial_w t_{+1} < 0 < \partial_w l_{+1}, \partial_w z_{+1} &\text{for D} \end{aligned} \tag{18}$$

The clearest evidence comes from the EITC, which is well established to draw low-income single mothers into employment Eissa and Liebman (1996); Meyer and Rosenbaum (2001). Caveat is that this is about low-income mothers. Mothers' added earnings are often used for bills, debt, gro-

ceries, restaurant meals, and work-enabling durables such as vehicles Goodman-Bacon and McGranahan (2008); Aladangady et al. (2018); Hamad et al. (2023). The time-use evidence is more nuanced. By increasing employment, the EITC lowers total time with children Bastian and Lochner (2022). Yet this decline appears concentrated in passive or non-investment time rather than developmentally intensive activities. Consistent with this interpretation, the child-outcome literature finds that the EITC improves infant health, including reductions in low birth weight Hoynes et al. (2015), raises children's test scores Dahl and Lochner (2012), and improves longer-run educational and labor-market outcomes Bastian and Michelmore (2018).

# 5 Empirical Results

## 5.1 Average and Distributional Effect on Lifetime Income

Figure 6 summarizes the first-stage and instrumental variables estimates for children's lifetime income by age at exposure. We construct the age-specific estimates using overlapping two-year age windows centered on the age shown. Thus, the estimates plotted at ages 1, 2, and 3 use children between ages 0 and 2, 1 and 3, and 2 and 4, respectively. Panels (a) and (b) show the first-stage coefficients for sons and daughters, respectively. To be precise, we use the following specification:

$$\Delta a \, \Delta L_f^m = \pi Z_f + \lambda_{a(i),a(i')} + X_f' \rho + \nu_f. \tag{19}$$

The magnitude of these coefficients is consistent with the labor-supply response documented earlier. Panel (c) of Figure 4 shows that a one-percentage-point increase in the marginal net-of-tax rate raised mothers' weekly working hours by 0.23. Given the average sibling age gap of approximately 3.5 years, this estimate implies a first-stage coefficient of about 8.1 ($0.23 \times 3.5 \times 100/10$) in the units of equation (19).[8] This magnitude reflects how the reform-induced increase in maternal hours translates into differential exposure across siblings of different ages. This implied magnitude closely matches the age-specific first-stage coefficients in panels (a) and (b) of Figure 6.

In both cases, the reform increased maternal labor supply throughout childhood: the first stage is smaller at the very youngest ages, rises through early childhood, peaks around school entry, and then declines somewhat at older ages. The similarity across panels is important, as it implies that any gender differences in IV estimates cannot be attributed to weaker first stages for daughters.

Panels (c) and (d) show the corresponding instrumental-variables estimates of the effects on lifetime income, and reveal sharp heterogeneity by both age at exposure and child gender. For sons, Panel (c) shows that the negative effects are concentrated in the preschool years: a one-year earlier exposure to a persistent 10-hour increase in maternal weekly working hours lowers sons' lifetime income by about 4.8 percent on average, while estimates after school entry are close to zero (we can reject effects larger than 1%). For daughters, Panel (d) tells a strikingly different

[8] $100/10$ is a unit conversion. Multiplying by 100 converts the MNTR change from a 0–1 scale into percentage points, while dividing by 10 expresses maternal working hours in 10-hour units.

story: preschool-age coefficients are centered near zero, and while estimates become somewhat more negative at older ages, the average effect is 1.2%, they remain smaller in magnitude but more precisely estimated than the preschool effects for sons.

Panels (e) and (f) examine the distributional effects for preschool-exposed sons. Panel (e) uses the cumulative distribution function and shows that the largest negative effects appear around thresholds in the middle of the income distribution, meaning that exposed sons are less likely to attain earnings near or slightly above the median. The effects are considerably smaller in the upper tail, providing little evidence that maternal employment reduced the probability of reaching very high earnings. Panel (f) confirms this pattern using the probability density function: probability mass shifts away from middle-income levels and toward somewhat lower earnings. The negative average effect for preschool sons thus operates mainly through a movement from average to below-average lifetime income, rather than through a sharp increase in very low earnings or a reduction in the chances of reaching the top of the distribution.

Panels (e) and (f) examine the distributional effects for preschool-exposed sons (For similar analysis of other age-gender groups, see Appendix Figures XXX). The two panels show that the probability mass is unaffected, except for a shift away from the 80-90 percentile range (9th decile) toward p30-p80 (4th to 8th deciles), a lower likelihood of becoming part of the lower upper class, and an increase in the likelihood of appearing in the middle class. The effects are considerably smaller in the tails; maternal employment does not affect the probability of reaching the top decile or bottom three deciles.

### 5.2 Family Formation and Household Income

Table 1 examines whether exposure to maternal employment affects family formation in adulthood. We define a spouse as the first partner observed between ages 30 and 50, either through marriage or through cohabitation with a common child. The estimates provide little evidence that exposure changes whether individuals form families: effects on the probability of having a spouse, the probability of having a child, and completed fertility are small and statistically insignificant for both sons and daughters. There is also no meaningful effect on spouse age for sons, although exposed daughters have spouses who are slightly younger. The clearest change is in educational sorting. Exposed daughters are 1.5 percentage points less likely to have a spouse with post-compulsory education, whereas the corresponding estimate for sons is close to zero. Exposed sons also have their first child approximately 0.13 years earlier, a modest shift of about one and a half months. Taken together, the results suggest that exposure has limited effects on the incidence or size of families, while modestly affecting partner selection and the timing of parenthood.

Figure 9 shows that the effects on children's own lifetime income do not map one-for-one into household income, because exposure also changes the outcomes of their realized spouses. All outcomes are expressed as within-family sibling differences, defined as younger child minus older child. For spouse outcomes, the corresponding difference is between the corresponding spouses. In the extended samples used in panels (b) and (c), outcomes are the matched and av-

eraged sibling-pair measures constructed for the analysis sample. All specifications include fixed effects for the interaction of the siblings' birth years, the interaction of the siblings' genders, the mother's education category, and six bins of the mother's year of birth.

For sons exposed during preschool ages, the negative effect on own lifetime income of about 4.8 percent is partially attenuated at the household level: their wives earn about 2.5 percent more, leaving total household income down roughly 3.5 percent. For daughters, the pattern reflects the mirror image. Despite essentially no change in their own income, their husbands earn about 3.2 percent less, resulting in a household income fall of about 1.5 percent. For sons exposed at primary school age, there is no effect on their own or their spouses' incomes, whereas daughters experience declines in both their own and their spouses' earnings. The patterns of effects on spousal earnings anticipate the reallocation of childcare we will document later.

Spousal outcomes should be interpreted as outcomes of the realized spouses of exposed adult children, not as direct effects on a fixed spouse. They may therefore capture both marriage-market sorting and post-marriage household labor-supply responses. To shed some light on the issue of selection we look at own and spousal human capital.

### 5.3 Heterogeneity by Quality of Childcare

To understand the mechanisms behind our main results, we examine how the effects on children vary with the quality of care that substitutes for maternal time. Table 2 reports the results.

We first ask whether the effects reflect a particular cost of mothers entering the labor market, or a more general effect of any increase in maternal hours. Column (1) shows that the negative effect of an additional maternal work hour is similar regardless of whether the mother was employed prior to the reform, suggesting that what matters is the reallocation of maternal time rather than anything specific to the transition into work itself.

Next, we use maternal education as a proxy for the quality of home care foregone. Column (2) shows that the negative effects are substantially larger for children of more educated mothers—roughly three times as large—consistent with higher-educated mothers providing higher-quality home care prior to the reform.

We then turn to the quality of available childcare alternatives. Since we do not observe which type of care working mothers used, we proxy for availability using two sources of variation. First, we measure access to formal and informal childcare using the share of preschool teachers and childcare workers per preschool-age child across municipalities in 1970. Formal preschool was rare at the time, covering just 6 percent of preschool-age children in 1966 (SOU, 1967), while private family childcare was more common—covering 31 percent of children compared to 5 percent in municipal family childcare (SOU, 1967). Second, we measure grandparent availability as an alternative care source, defined as having at least one grandparent residing in the same municipality and either not employed in 1970 or working at least part time (25 hours or more) in childcare. We then split children by whether grandparents are accessible, and separately by whether they live in a municipality with above- or below-median availability of formal and informal childcare.

The results point to meaningful differences in care quality across these alternatives. Children with access to grandparents experience smaller and statistically insignificant negative effects, suggesting that grandparent care is of higher quality than the formal and informal options available at the time. Among children without nearby grandparents, the negative effects are substantially larger in municipalities with greater availability of formal and informal childcare—a counterintuitive pattern that suggests these alternatives were of relatively low quality, particularly compared to care provided by parents or grandparents.

### 5.4 Human Capital: Skills and Education

Figures 7 and 8 quantify the role of human capital in mediating the long-run effects on income. We begin with educational attainment and then turn to skills, which are available only for sons.

Panel (a) of Figure 7 summarizes the effects of the full set of educational measures for children and their spouses on a common lifetime-income scale. The predicted effects are negative for all four groups. The loss is largest for sons, at approximately 1.8 percent; it is about half as large for sons' wives and daughters' husbands and smaller for daughters. Thus, exposure reduces education-related human capital not only among children but also among the spouses with whom they subsequently match.

Panels (b) and (c) decompose these education-based predictions into the contributions of education level, field of study, and upper-secondary exit grades for sons and daughters, respectively. For sons, the predicted loss of approximately 1.8 percent amounts to nearly one-half of the 3.9 percent decline in realized lifetime income. Field of study accounts for the largest component, followed by education level, while grades contribute less. For daughters, realized lifetime income is essentially unchanged, but the education-based predicted effect is negative at approximately 0.5 percent. Lower education levels and grades contribute to this loss, partly offset by a positive contribution from field of study. Daughters therefore experience measurable losses in education-related human capital even though these losses do not translate into a comparable decline in their own lifetime income.

Panel (c) applies the same income-scale transformation to the full set of educational measures for children and their spouses. The predicted effects are negative for all four groups. The effect is largest for sons, approximately half as large for both groups of spouses, and smaller for daughters. Together with Panel (a), these results show that a single indicator for post-compulsory education understates the broader changes in educational attainment, particularly for daughters and spouses.

Figure 8 turns to skills. Panel (a) reports effects on cognitive and non-cognitive skills measured at age 18 through military enlistment testing. The estimates are broadly negative across both domains. Among cognitive skills, inductive reasoning, spatial ability, and technical understanding decline, whereas the estimate for verbal comprehension is close to zero. The non-cognitive effects are larger, with particularly pronounced declines in intensity and social maturity and a smaller decline in psychological energy. The effects of increased maternal labor supply on sons' skills are

therefore broad-based but most pronounced for non-cognitive attributes.

Panel (b) expresses cognitive, non-cognitive, and physical skills on the lifetime-income scale. Skill losses predict a 1.3 percent decline in lifetime income, accounting for approximately one-third of the total 3.9 percent loss. Each of the three skill domains contributes to the predicted decline, although the physical component is less precisely estimated. Panel (c) combines education and skills. Together, these measures predict a 2.4 percent decline in lifetime income, or approximately three-fifths of the total effect. Field of study and non-cognitive skills are the largest components, followed by education level and grades. Cognitive skills contribute little conditional on education, consistent with cognitive skills affecting earnings primarily through educational attainment and field choice rather than through an additional direct channel. Overall, the two figures indicate that education and skills account for a substantial share, but not all, of sons' loss in lifetime income. For daughters, the divergence between the negative education-based prediction and the near-zero realized income effect points to offsetting forces, to which we return below.

### 5.5 Working Hours

The working-hours estimates reveal a striking gender pattern. Sons work 2.5 percent fewer hours, while their wives' hours remain essentially unchanged; daughters work 1.0 percent more, whereas their husbands work 1.3 percent fewer hours. The point estimates therefore align more closely by gender than by whether the individual is the exposed child or the child's spouse: market hours fall among men, while they are stable or increase among women. In both types of couples, this pattern narrows the gender gap in paid work and is consistent with a less specialized division of labor in the next generation.

The human-capital results provide a natural benchmark for interpreting the labor-supply responses. We find lower human capital among both husbands and wives, which we interpret as a loss of market productivity. In a standard model of couples' labor supply, lower own productivity reduces market hours through the substitution effect, whereas lower spousal productivity reduces household income and thereby increases one's own labor supply. Because women's labor supply is generally more responsive to their own productivity than men's, while the cross-spouse income response is smaller, the combined productivity losses should reduce women's hours and generate, at most, a modest increase in men's hours.

The observed responses are the opposite: women increase their market hours, while men reduce theirs substantially. The human-capital channel alone therefore cannot account for the reallocation of market work within couples. This sign reversal motivates an intergenerational norm channel. By exposing children to greater maternal employment and a less gender-specialized division of work, the reform may have shifted preferences or beliefs toward a more equal allocation of paid work and caregiving. Such a shift would increase women's market hours and reduce men's, offsetting—and ultimately overturning—the labor-supply effects predicted by the productivity losses. This interpretation is consistent with the literature documenting the intergenerational transmission of gender-role attitudes and labor-supply behavior (Fernández et al., 2004; Bütikofer,

2013; Farré and Vella, 2013; Johnston et al., 2014; McGinn et al., 2019).

### 5.6 Allocation of Childcare

Figure 10 turns specialization within the families that adult children subsequently form. Panel (a) reports effects on the component of income not explained by the measures of human capital analyzed in Figure 7. This residual income falls by 2.0 percent for exposed sons and rises by 1.1 percent for their wives. The pattern is reversed in the households of exposed daughters: their own residual income is essentially unchanged, while their husbands' residual income falls by 2.2 percent. Because these outcomes net out the contribution of measured education and skills, they point to changes in labor supply and within-household specialization, rather than human capital alone.

The remaining panels provide direct evidence on the allocation of childcare. Panel (b) shows that exposed sons take 3.2 additional days of parental leave per child, while their wives take 5.4 fewer days. Exposed daughters take 3.2 fewer days, whereas their husbands take 3.8 additional days. Panel (c) shows a parallel reallocation of leave to care for sick children: the exposed sons' within-couple share rises by approximately 1 percentage point and their wives' share falls correspondingly, while the pattern is reversed for exposed daughters and their husbands.

Panel (d) shows that these changes extend beyond recorded leave-taking to the longer-run earnings consequences of parenthood. We define a large child penalty as a proportional change in earnings from before to after childbirth that falls below the gender-specific median. Exposure raises the probability of such a penalty by 2.2 percentage points for sons and by 1.8 percentage points for the husbands of exposed daughters. The corresponding estimates for exposed daughters and sons' wives are small and negative. Thus, in both types of households, men assume a larger share of childcare and become more likely to experience an associated earnings penalty, while women assume a smaller share.

Overall, the evidence indicates that exposure to a working mother reshapes the allocation of market work and childcare in the next generation without materially changing family size. The response is symmetric across the two types of couples: exposed sons shift toward caregiving relative to their wives, while exposed daughters shift toward market work relative to their husbands. The consistency of the effects across residual income, parental leave, sick-child leave, and child penalties supports an intergenerational gender-norm channel through which maternal employment leads adult children to adopt a more gender-equal division of paid work and childcare.

### 5.7 Transmission to the Third Generation

Figure 11 provides suggestive evidence that the consequences of maternal labor supply persist into the third generation, i.e., grandchildren. Because much of this generation is still too young to have completed education or entered the labor market, we study school performance, measured using results from national exams taken at the end of compulsory schooling. All Swedish students complete these exams in the spring term of 9th grade, at approximately age 16. These exams are

high-stakes: GPA determines eligibility for oversubscribed secondary programs and ultimately affects college admission (Ramstedt, 2005).

Children of the generation exposed to maternal labor supply at preschool ages perform worse in school, with the adverse average effect concentrated among children of exposed sons. Panel (a) shows that their standardized grade-point score falls by approximately 2.5 percent of a standard deviation, whereas the corresponding estimate for children of exposed daughters is close to zero. Panels (b) and (c) reveal that these averages mask distinct changes across the grade distribution. Among children of exposed sons, the dominant pattern is a deterioration at the lower end: the probability of receiving a D falls by 1.3 percentage points, while the probabilities of receiving an E or an F rise by 0.3 and 0.6 percentage points, respectively. Thus, the negative average effect appears to be driven primarily by children moving from D into lower grade categories.

For children of exposed daughters, the absence of an average effect conceals substantial heterogeneity. The probability of receiving a D falls by 1.0 percentage point, but the displaced mass moves in both directions: the probabilities of receiving an E or an F increase at the lower end, while the probabilities of receiving a C or a B increase higher in the distribution. Exposure therefore appears to widen the distribution of achievement among these grandchildren, pushing some children with grades around D downward while moving others upward. This pattern echoes the distributional effect on exposed daughters' own lifetime income in Figure 6, Panel (f), where limited movement in the mean similarly masks offsetting effects across the distribution. Overall, the results indicate that the consequences of maternal employment under childcare scarcity extend beyond the directly exposed cohort, taking the form of a lower mean among children of sons and greater dispersion among children of daughters.

The weaker school performance of grandchildren is naturally related to the lower human capital of the parent generation. Exposure to maternal employment reduces the education and skills of sons and daughters who later become parents, potentially lowering both the quality of the cognitive and non-cognitive inputs they provide to their own children and the material resources available in the home. This mechanism is consistent with the lower average achievement among children of exposed sons. For children of exposed daughters, the offsetting movements across the grade distribution suggest that changes in parental human capital and household resources generate heterogeneous effects rather than a uniform decline in achievement, paralleling the distributional effects on exposed daughters' own lifetime income in Figure 6, Panel (f). This mechanism also connects our findings to the literature on intergenerational returns to schooling: Oreopoulos et al. (2006) show that additional parental schooling reduces grade repetition and dropout among children, Currie and Moretti (2003) find that higher maternal education improves parental behavior, Carneiro et al. (2013) show that maternal education improves children's cognitive achievement partly through the home environment, and Lundborg et al. (2014) find positive effects of maternal education on sons' skills in Sweden.

Since schooling also has well-documented labor-market returns (Heckman et al., 2006) and family resources can themselves improve children's outcomes (Page, 2024), lower parental human

capital is likely to reduce both investments and household resources available to the third generation. This interpretation is reinforced by the fact that affected households have lower total income even though parental leave is reallocated in a more gender-equal way, suggesting that the gains from a more equal division of childcare do not fully offset the losses in parental human capital and family resources. At the same time, the heterogeneous response among children of exposed daughters indicates that these changes need not affect all grandchildren in the same direction. Because the broader evidence on causal intergenerational effects of parental schooling is mixed, however, our results are best interpreted as consistent with—rather than a direct estimate of—this transmission mechanism (Holmlund et al., 2011).

### 5.8 Placebo Evidence on Instrument Validity

We use a placebo reduced form to assess the conditional-independence assumption in equation (13). The simulated tax instrument is constructed from parental earnings and, under joint taxation, varies mechanically with the father's income. It is therefore correlated with family socioeconomic status by construction. The identifying assumption does not require otherwise. Rather, conditional on the siblings' ages at the reform and the baseline controls, the instrument must be unrelated to unobserved determinants of the sibling outcome gap. A threat to this condition would arise if paternal income were systematically related to birth-order effects, age-specific parental investments, or other sibling-specific outcome gradients that remain after family differencing.

To examine this possibility, we construct a placebo sample of sibling pairs born between 1972 and 1982, whose childhoods occurred entirely after the transition to individual taxation. We assign 1982 as a hypothetical reform date and define each sibling's age relative to that date, thereby reproducing the age structure of the baseline design. We then apply the 1970 joint-tax and 1975 individual-tax schedules to parental earnings measured in 1982 and construct the resulting simulated change in the mother's marginal net-of-tax rate using the same procedure as in the main analysis. The placebo instrument thus retains the mapping from paternal income into simulated tax exposure, but it is not associated with an actual change in the tax regime or, consequently, with reform-induced variation in maternal labor supply.

We estimate the sibling-difference reduced form in this placebo sample, replacing age at the 1971 reform and the baseline instrument with their placebo counterparts. Under conditional independence, stable relationships between paternal income and sibling-specific outcome gradients should not generate a reduced-form association. Conversely, if the baseline estimates reflect such gradients rather than reform-induced maternal labor supply, the same earnings-based construction should tend to predict sibling outcome gaps in the placebo cohorts.

Table A.3 provides no evidence of such a relationship. In the baseline preschool sample, the reduced-form estimate is $-0.158$ with a standard error of $0.0488$ in the specification with mother-education fixed effects. The corresponding placebo estimate is $0.00494$ with a standard error of $0.129$; without mother-education fixed effects, it is $-0.0624$ with a standard error of $0.127$. Thus, the negative reduced-form relationship in the reform-exposed cohorts is not reproduced when the

same simulated-tax construction is applied to cohorts for whom no reform occurred.

The scope of this placebo test is limited. It does not rule out channels specific to the 1971 reform itself, such as contemporaneous policy changes, divorce, fertility, or migration responses. Rather, it addresses the specific concern that the simulated tax instrument proxies for paternal-income-related sibling outcome gradients. The absence of a corresponding placebo reduced form therefore provides little support for the view that the main estimates are driven by such gradients.

# 6 Policy Implications

Our findings have implications for both the design and the sequencing of family taxation and childcare policy. Although individual taxation is now widespread, several major economies—including the United States, Germany, and France—retain important elements of couple-based taxation, whether through joint filing, income splitting, or household assessment. These systems differ in whether couple-based assessment is mandatory, automatic, or elective. Their common feature is that the tax liability associated with a secondary earner's income depends on the income of the primary earner. Proposals to weaken this link or move to individual taxation therefore remain relevant well beyond the Swedish setting.

By increasing the return to secondary-earner employment, individual taxation may reduce household specialization and promote what Goldin (2021) calls *couple equity*: a more equal allocation of career opportunities and caregiving responsibilities between partners. Our finding that exposure to maternal employment leads adult children to divide childcare more equally with their partners—such as parental leave or staying home when children are sick—is consistent with such changes being transmitted across generations. Tax reforms like these may therefore affect not only the couples directly exposed to it, but also the household behavior and gender-role norms of the next generation.

Such a reform may also affect the human capital of future generations. At the same time, our findings show that these effects depend critically on the childcare environment. In Sweden, the transition to individual taxation substantially increased maternal employment before childcare capacity expanded. The resulting human-capital losses were concentrated among children exposed at young ages and were attenuated when formal care or grandparents were available as a substitute source of care. Thus, a reform that improves labor-market incentives and promotes greater equality within currently exposed couples may nevertheless reduce the human capital and later earnings of their children when adequate substitutes for parental care are unavailable.

There is, therefore, a complementarity between policies that increase parental employment and access to childcare of sufficient quality. Tax and childcare policies should consequently not be evaluated in isolation. A conventional evaluation of individual taxation may capture changes in employment, earnings, tax revenue, and household disposable income while omitting induced childcare demand and effects on children's human-capital accumulation. Conversely, the utilization and labor-supply effects of childcare expansion depend partly on whether the tax system

rewards or penalizes secondary-earner employment.

The Swedish experience illustrates the transitional incidence of this coordination. The prevalence of dual-earner households increased considerably faster than the provision and use of formal childcare. Our results suggest that children exposed during this mismatch bore persistent costs through lower skills, education, and adult earnings.

One sequencing strategy is to expand childcare capacity before introducing reforms that substantially increase secondary-earner employment. This expansion could take the form of direct public provision, subsidies for private provision, or policies that facilitate provider entry while maintaining quality standards. Such sequencing would allow childcare supply to adjust before parental employment rises and would protect children in the transitional cohorts. It would, however, require governments to finance or facilitate substantial capacity before the corresponding demand had fully materialized.

An alternative is to reform couple-based taxation first and expand childcare provision as the number of dual-earner households increases. This approach may reduce the initial fiscal commitment and reveal the magnitude, location, and composition of childcare demand before large public investments are undertaken. Increased demand may also encourage private provider entry by creating a sufficiently large and predictable market to overcome fixed costs and coordination problems.

In addition, the growth of dual-earner households may create a politically influential constituency in favor of expanded public childcare. The Swedish sequence is consistent with this political-economy mechanism: by making childcare increasingly essential to a growing share of households, the transition toward dual earning may have contributed to the political support for the subsequent expansion of public provision. Our research design does not, however, identify this mechanism, and contemporaneous changes in gender norms, labor demand, and the broader political environment may also have contributed.

The disadvantage of reforming taxation first is that childcare supply may adjust only gradually. Even a substantial increase in demand need not generate an adequate childcare market when entry is constrained by fixed costs, credit constraints, shortages of qualified workers, regulation, or uncertainty about future demand. Informational asymmetries may also make it difficult for parents to assess childcare quality, while providers may have insufficient incentives to account for the longer-run social returns to children's human-capital development. Where these constraints are important, childcare subsidies, workforce investments, quality regulation, or direct public provision may be justified on efficiency grounds. Where the main obstacle is instead temporary uncertainty about demand, transitional public investment or capacity guarantees may be sufficient until private provision expands.

The resulting policy trade-off is inherently dynamic. Expanding childcare before reforming household taxation may protect the human capital of the transitional generation but impose higher up-front fiscal and administrative costs. Reforming taxation first may generate the demand, information, and political support needed for childcare expansion, but only after some children have

been exposed to inadequate substitute care. The appropriate sequence therefore depends not only on the long-run complementarity between the policies, but also on the speed with which childcare supply, political support, and public-sector capacity respond to the increase in demand.

More generally, the Swedish sequence should not be interpreted as a universally optimal or universally undesirable model. Its relevance to other countries depends on the size of the secondary-earner tax wedge, the elasticity and quality of private childcare supply, the government's fiscal and administrative capacity, prevailing gender norms, the quality of available informal care, and the responsiveness of the political system to the demands of dual-earner households. The externally relevant conclusion is therefore conditional: reforms that strengthen incentives for secondary-earner employment and policies that expand access to high-quality childcare are complements, but their optimal timing and sequencing depend on the institutional environment. Without adequate childcare capacity, gains in labor supply and *couple equity* may come at the cost of lower human-capital formation and earnings in the next generation.

# 7 Conclusion

This paper studies the long-run consequences of a large and permanent increase in maternal labor supply for children and the next generation's households. In Sweden in the early 1970s, the move to individual taxation substantially raised mothers' labor supply at a time when public childcare was scarce. Our estimates show that the effects on children are large, persistent, and highly heterogeneous: the main costs were borne by sons exposed during the preschool years, who experienced lower lifetime earnings, weaker cognitive and non-cognitive skills, and reduced educational attainment. These losses were attenuated when grandparents were available as an alternative source of care, and were larger in municipalities with greater availability of formal and informal childcare — a pattern consistent with the quality of substitute care being lower than that provided by parents or grandparents. At the same time, exposure to maternal employment shifted the within-household division of childcare toward greater gender equality in the next generation, raising paternal leave-taking among sons and the husbands of daughters.

The central lesson is that the child welfare consequences of policies that raise maternal employment depend critically on the care environment that replaces maternal time. When high-quality substitute care is unavailable, expansions in maternal employment can generate persistent losses in child human capital that offset or outweigh the household income and gender-equality gains. This finding speaks directly to a broader and ongoing policy debate. Boneva et al. (2025) document that individuals perceive childcare availability and quality as among the most important determinants of maternal labor supply decisions, and that labor supply intentions respond strongly to hypothetical improvements in childcare access. Our results provide the other side of that equation: the returns to maternal employment for children are themselves contingent on childcare quality, so that expansions in female labor supply and investments in high-quality childcare are complements rather than substitutes from a child welfare perspective.

Several important questions remain open and cannot be resolved with the present data. Because we do not observe the exact childcare arrangement that replaced maternal time, nor the quality of those arrangements, we cannot fully separate the roles of formal care, private home-based care, grandparental care, and changes in fathers' caregiving. This gap is especially important in light of recent evidence that grandparents materially shape the multigenerational incidence of childcare policy (Karademir et al., 2026), that childcare quality matters for both take-up and mothers' labor-market responses (Cobb-Clark et al., 2025), and that beliefs about post-birth work and childcare can diverge systematically from realized paths (Caplin et al., 2025). We also observe the third generation only early in life, so we can study school performance but not yet adult education, earnings, health, or family formation. Future work should therefore link labor-supply reforms to direct measures of childcare use, parental time investments, beliefs, and work schedules, and should test whether father-targeted leave reforms and other norm-shifting policies can offset the human-capital losses documented here, as suggested by recent evidence on earmarked paternal leave and cultural change (Albrecht et al., 2024).

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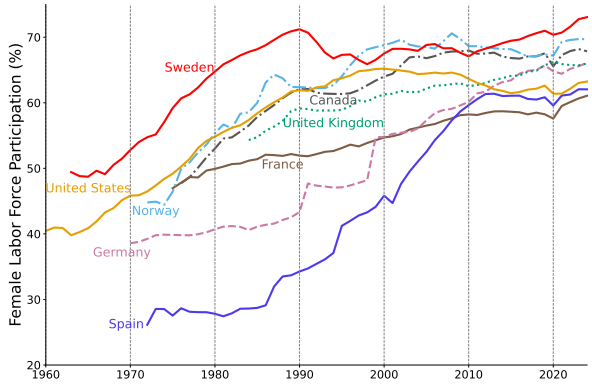
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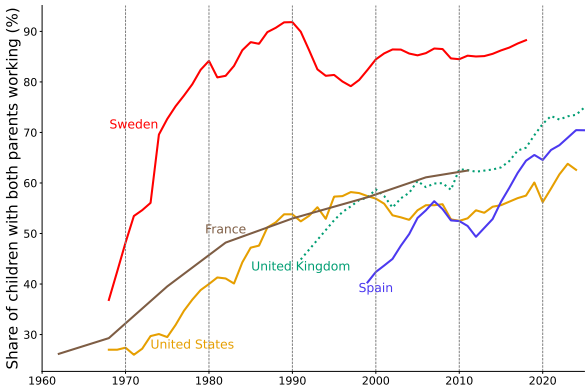
# 8 Figures and Tables

Figure 2: Female Labor Supply, Dual-Earner Families, and Childcare, 1960-2020

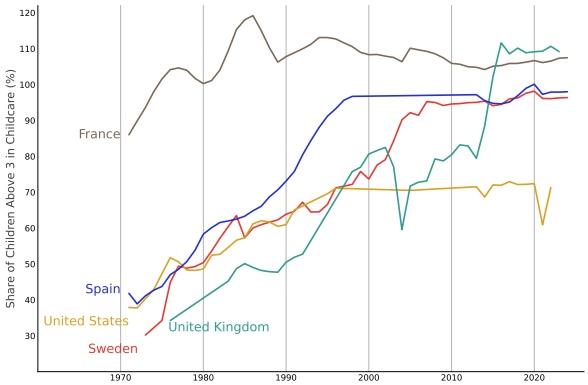
(a) Female Labor Force Participation by Country



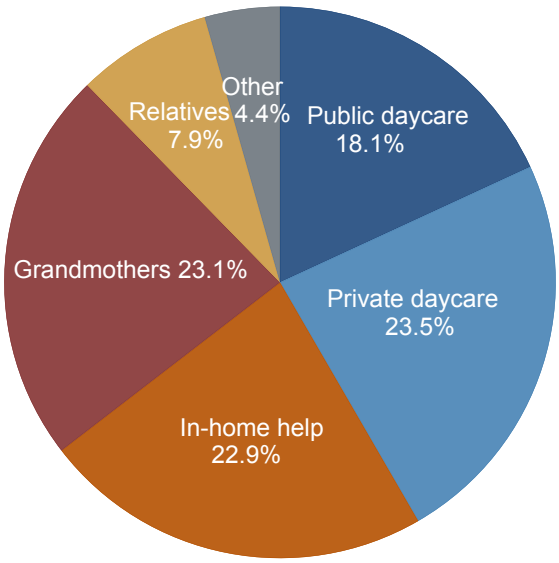

(b) 0-6 years old in Dual-Earners Household



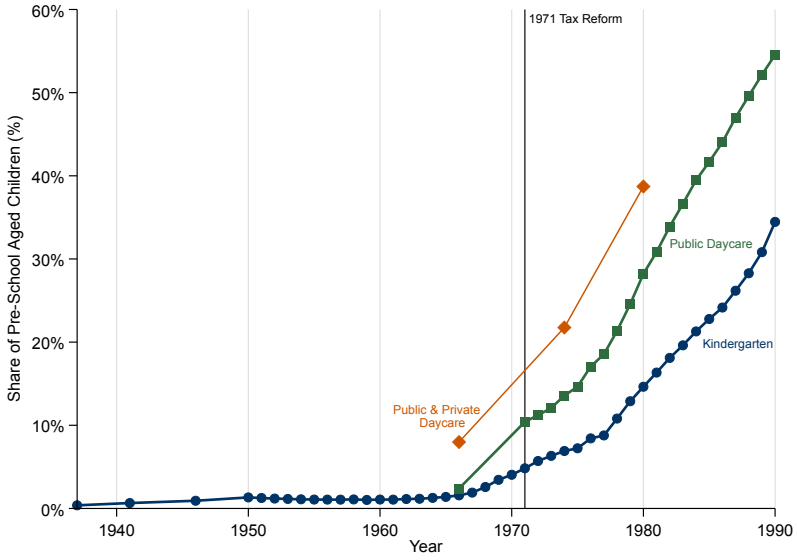

(c) 4-7 years old in childcare




*Notes:* **Panel (a)** plots labor force participation rates of women aged 15–74; data are from the OECD. **Panel (b)** plots the share of children aged 0–6 with both parents employed full- or part-time. Swedish data are from the Income and Taxation Register (IoT) linked to the Multigenerational Register; UK data are from microdata accessed via the UK Data Service, supplemented by IPUMS International; French data are from IPUMS International; Spanish data are from the Encuesta de Población Activa (EPA); and US data are from the IPUMS Current Population Survey (CPS). **Panel (c)** plots the share of children above age 3 enrolled in childcare, measured as the gross enrollment ratio in pre-primary education (ISCED level 02): total enrollment at that level as a percentage of the population in the official age group. In all five countries the official entrance age is 3, and the age range extends to the official entrance age for primary education. Because the numerator includes children enrolled outside this age range, ratios may exceed 100 percent. Data are from the UNESCO Institute for Statistics ("Gross enrollment ratio, pre-primary, both sexes (%)," Bulk Data Download Service).




(a) Care Arrangements in 1966



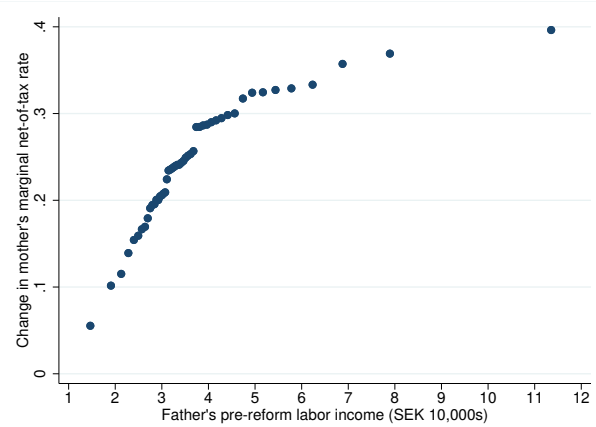

(b) Formal Childcare Coverage, 1937–1990

Figure 3: Pre-School Childcare in Sweden

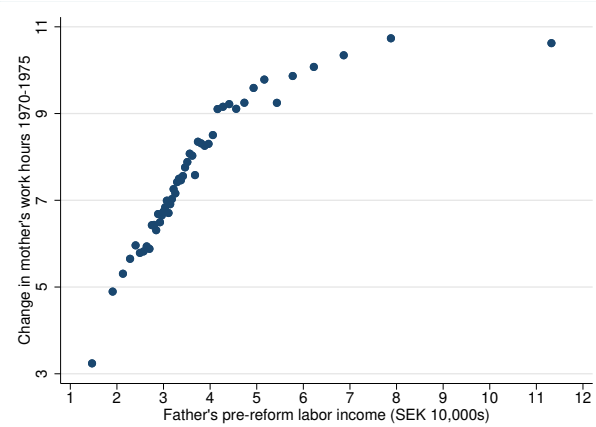
*Notes:* **Panel (a)** shows the primary care arrangements for pre-school-aged (0–7) children whose parents both worked over 15 hours per week in 1966—the year with the most accurate childcare data around the 1971 reform. *Public childcare* is organized, monitored, and subsidized by municipalities; *Private childcare* is outside municipal oversight or subsidy. *In-home help* refers to nannies or domestic staff in the child's home. *Grandmothers* means care by a grandmother, while *Relatives* covers other family members, including siblings. *Other* includes neighbors, friends, or children left on their own. For further details and survey breakdowns, see Section 1.2 and Appendix C. **Panel (b)** plots three measures of formal childcare coverage as a share of children aged 0–7. The first series (solid) covers kindergarten, or public day care centers (daghem/förskola), the second (dashed) adds municipal family daycare (kommunala familjedaghem), and the third includes private family daycare (privata familjedaghem). Data for 1937–1989 are drawn from Statistics Sweden's annual statistical reports; data from 1990 onwards are from Skolverket. Prior to 1968 the series records licensed places; from 1968 it records enrolled children. The dashed series is unavailable between 1966 and 1971: the 1966 data point is from a nationally representative survey conducted by SCB on behalf of Familjedaghemsutredningen, and the segment connecting it to the continuous series from 1971 is linearly interpolated. The two 1966 observations for the dashed and dotted series record children of parents working more than 15 hours per week. Private family daycare is observed at three isolated points (1966, 1974, 1980) connected by a dashed line; the 1966 observation uses children of working parents as the denominator while the 1974 and 1980 observations cover all preschool-age children (Familjestödsutredningen, 1978, 1981). Private family daycare is otherwise unobserved as it was not captured in administrative registers.

Figure 4: Mothers' Labor Supply Responses to Reform-induced Tax Cuts

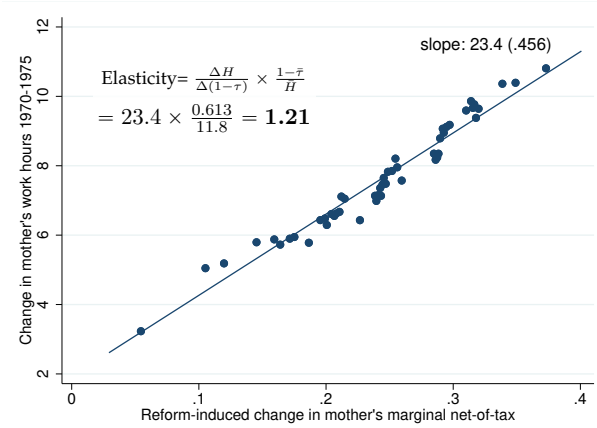
(a) Change in Net-of-Tax Rate



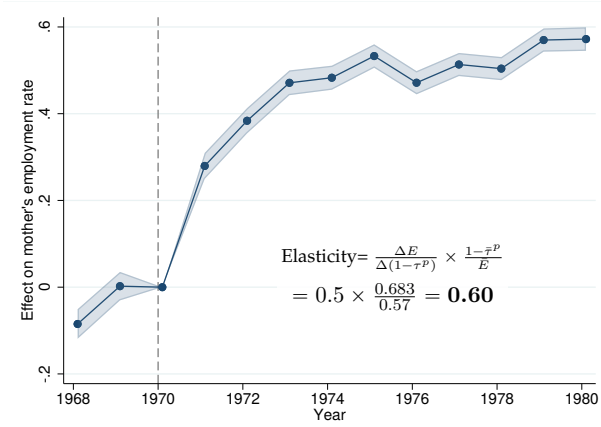

(b) Change in Mothers' Labor Supply




(c) Mothers' Labor Supply Response to Tax Reform

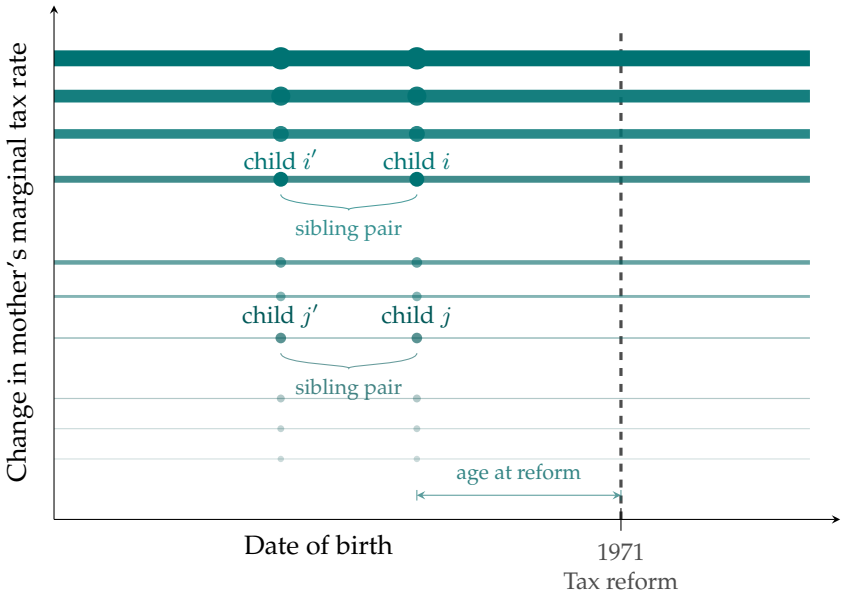



(d) Event Study: Employment Effect of Tax Reform




*Notes:* **Panel (a) and (b)** plot the simulated change in mothers' marginal net-of-tax rate (MNTR) and weekly working hours (including zeros) between 1970 and 1975 against fathers' predicted pre-reform labor income. To compute MNTR, we construct a tax calculator (Appendix B). The post-reform MNTR is computed at pre-reform income to isolate tax changes from behavioral responses, following the simulated tax instrument approach of Gruber and Saez (2002). Fathers' 1970 income is predicted using 1968–1969 income to reduce the influence of transitory income shocks and mean reversion. **Panel (c)** plots the change in hours from panel (b) against the reform-induced change in MNTR—the fitted value from a non-parametric projection of the simulated MNTR change on fathers' predicted pre-reform income from panel (a). This projection isolates the component of reform-induced mothers' tax cuts generated by fathers' pre-reform income. The estimated slope implies that a 1 percentage point increase in the MNTR raised weekly hours by 0.23, corresponding to labor supply elasticities of 1.29 for hours and 0.60 for employment. **Panel (d)** reports an annual employment event study to test the parallel-trend assumptions, i.e., whether those with larger reform-induced tax cuts have been on the same labor supply trends pre-reform. We estimate year-specific effects of the reform-induced MNTR change, with mother fixed effects and year-by-mother-age-bin controls, instrumenting the year-by-MNTR terms with year interactions of fathers' 1970 earnings deciles; 1970 is omitted (See specificatoin 3). We use employment because hours are observed every five years, whereas employment is measured annually. Appendix Figure A.9 shows analogous results for fathers: hours fall slightly on average, but the drop is not systematically related to pre-reform income.

Figure 5: Research Design, IV Construction using Between and Within Family Variations

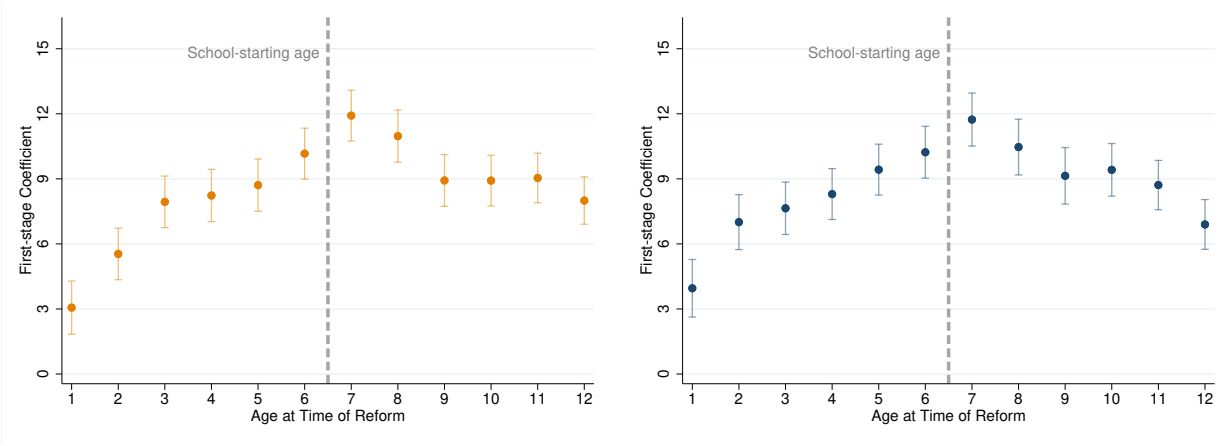



*Notes:* This figure illustrates how two sources of variation jointly determine our instrumental variable: (i) *between-family variation in treatment intensity* (y-axis): the reform affects mothers' marginal tax rate as a function of husbands' income (see Figure A.3); (ii) *within-family variation in age at reform* (x-axis), where a younger sibling is exposed to a working mother over a longer share of childhood.

Figure 6: Effects of Mother's Labor Supply on Next Generation's Lifetime Income

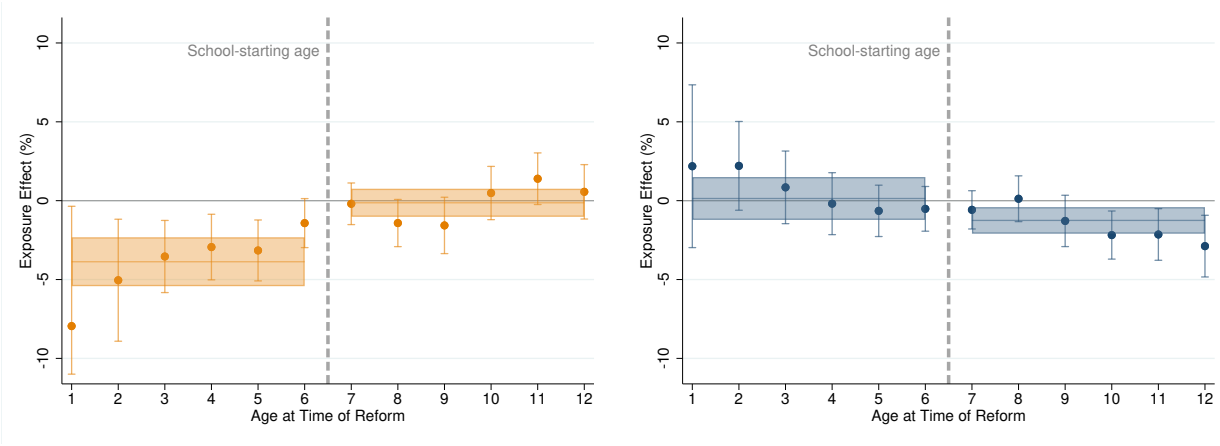
(a) First-stage: Sons

(b) First-stage: Daughters



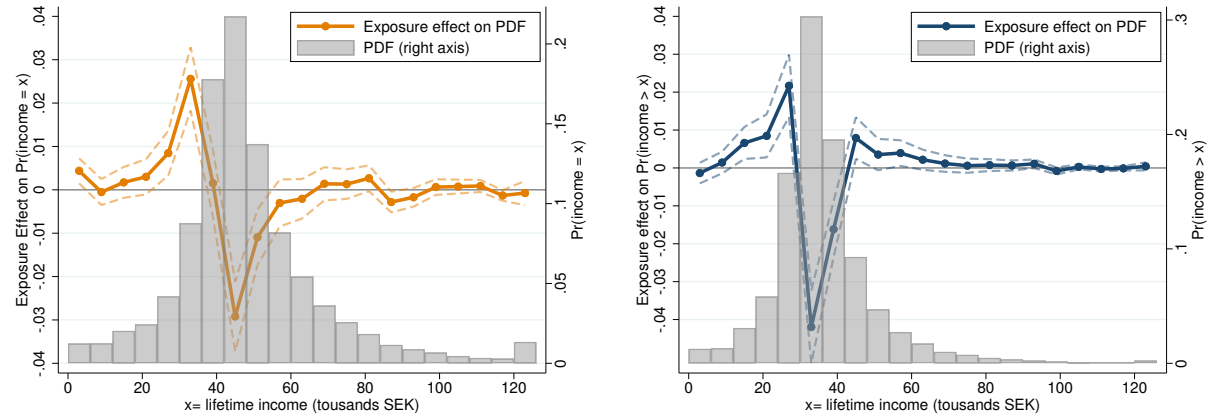

(c) Average Effect: Sons

(d) Average Effect: Daughters




(e) Distributional Effect: Preschool Sons

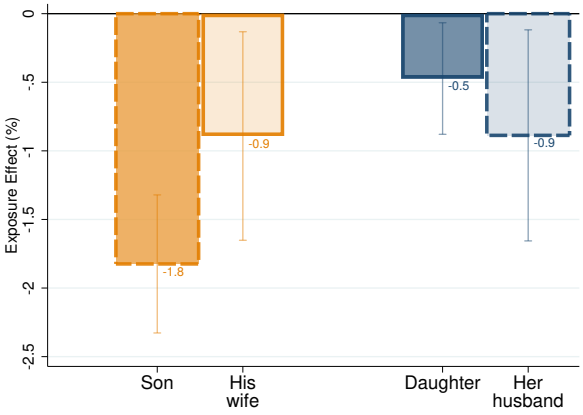
(f) Distributional Effect: Preschool daughters



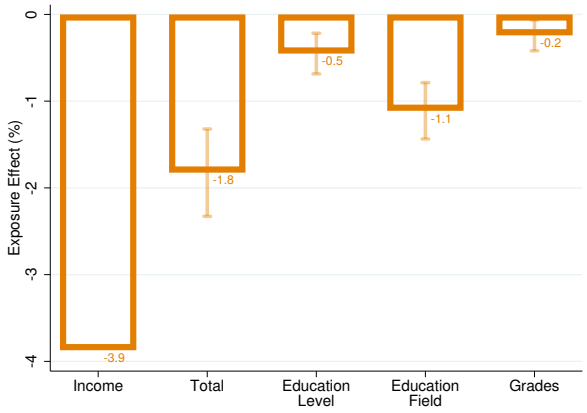

*Notes:* **Panels (a)-(b)** shows the mothers' labor supply increases by child age (first stage). Other panels present the IV estimates of the effect of a one-year earlier exposure to a 10-hour increase in maternal weekly working hours, $\beta_a$ in equation (8). They are estimated using the sibling-difference IV specification in equation (10): effect by gender and overlapping two-year age groups in **Panels (c)-(d)** and effect on the probability distribution of lifetime income for sons and daughters exposed at preschool ages in **Panels (e)-(f)**. See Figure A.8 for the cumulative distribution in the next generation. 95-percent Confidence intervals are based on robust standard errors.

Figure 7: Quantifying the Effect on Human Capital I: Education

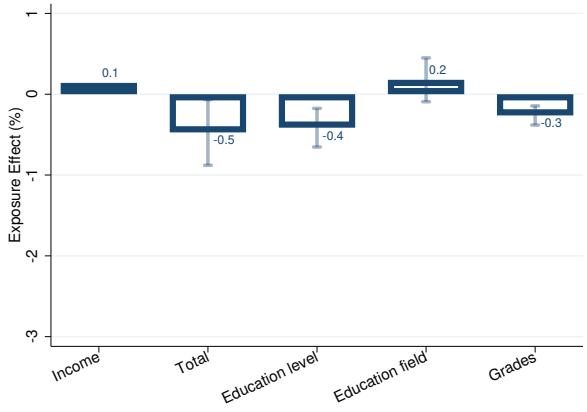
(a) Education: Income Scale




(b) Decomposition: Sons



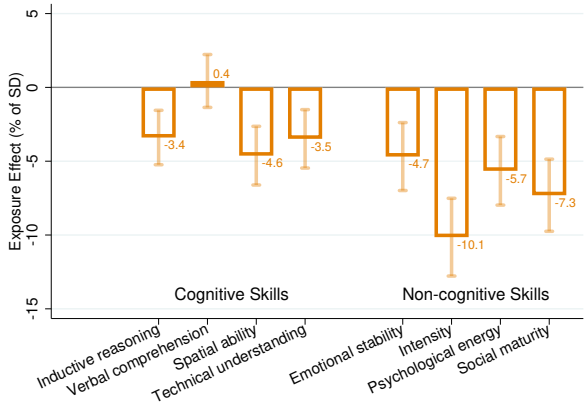

(c) Decomposition: Daughters



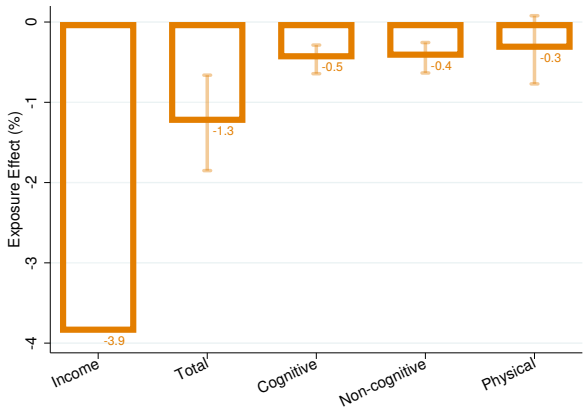

*Notes:* **Panel (a)** reports the effect on income predicted from these educational measures for children and their spouses. Predicted-income measures are obtained from Mincer regressions described in Section 5.4. **Panel (b)** and **(c)** expresses the effect on lifetime income and the component associated with educational attainment on an income scale, separately for sons and daughters. The educational measures are level (12 categories based on two-digit education codes; e.g., nine-year compulsory education and post-secondary education shorter than two years), field (342 categories; e.g., law and electrical engineering), and upper-secondary exit grades (deciles plus a category for missing grades). All panels present IV estimates of the effect of one additional year of exposure at preschool age to a 10-hour increase in maternal weekly working hours, corresponding to $\gamma_a$ in equation (8) and estimated using the sibling-difference specification design described in equation (10)). Whiskers show 95 percent confidence intervals based on robust standard errors.

Figure 8: Quantifying the Effect on Human Capital II: Skills (Only Sons)

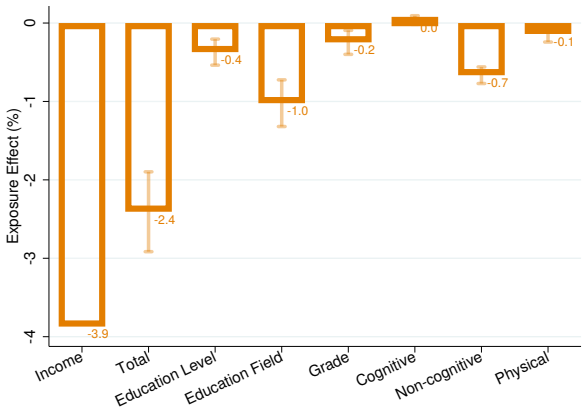
(a) Skills




(b) Skills: Income Scale



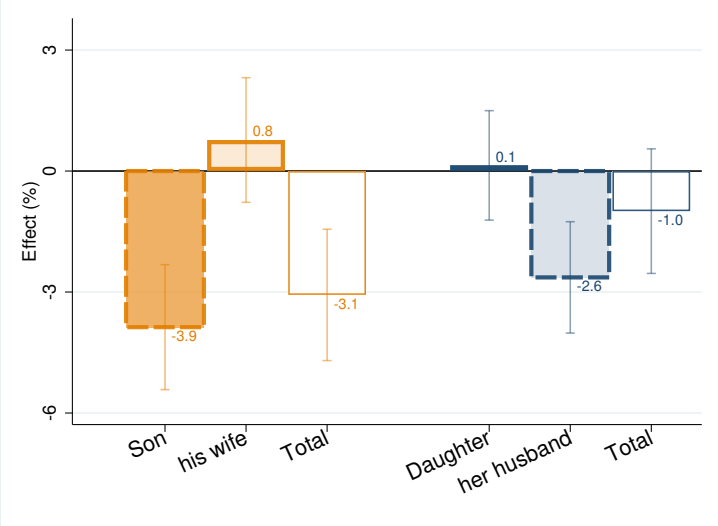

(c) Education & Skills: Income Scale



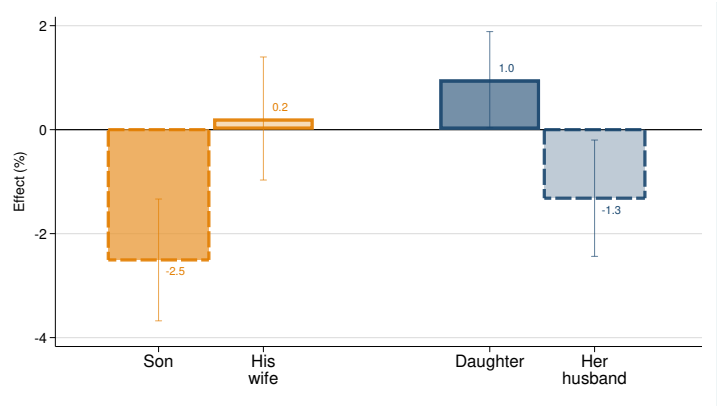

*Notes:* **Panel (a)** reports effects on sons' cognitive and non-cognitive skills, expressed as percentages of a standard deviation. **Panel (b)** expresses the effect on lifetime income and the components associated with cognitive, non-cognitive, and physical skills on an income scale. **Panel (c)** expresses the effect on lifetime income and the components associated with educational attainment and skills on an income scale. The educational measures are defined in the notes to Figure 7. Skill- and education-based predicted-income measures are obtained from Mincer regressions described in Section 5.4. The sample is restricted to sons because the skill measures are available only for men. All panels present IV estimates of the effect of one additional year of exposure at preschool age to a 10-hour increase in maternal weekly working hours, corresponding to $\gamma_a$ in equation (8) and estimated using the sibling-difference specification described in equation (10). Whiskers show 95 percent confidence intervals based on robust standard errors.

Figure 9: Effects of Mother's Labor Supply on Next Generation: Individual vs. household levels

(a) Lifetime Income



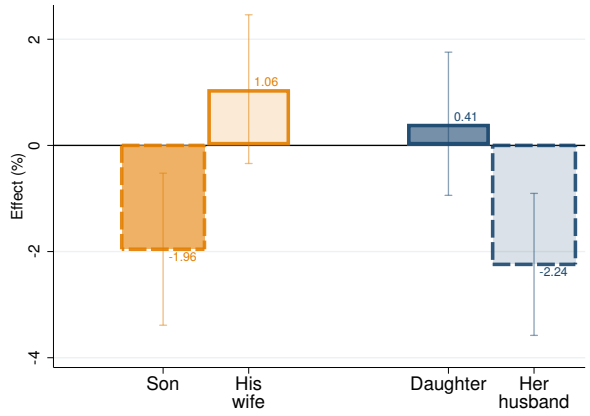

(b) Working Hours



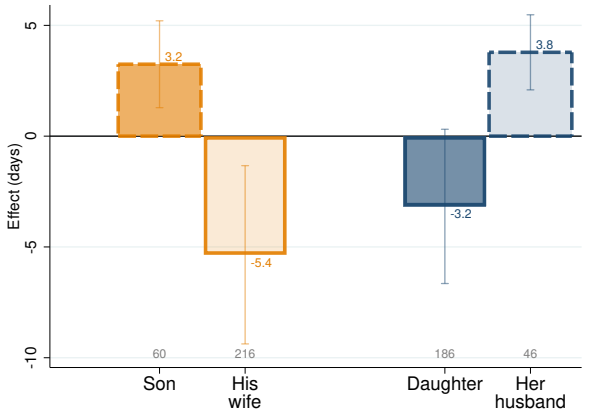

*Notes:* This figure presents IV estimates of the effect of a one-year exposure at *preschool age* to a 10-hour increase in maternal weekly working hours, $\gamma_a$ in equation (8) and estimated using specification (10). Whiskers show 95 percent confidence intervals based on robust standard errors.

Figure 10: Effects of Mother's Labor Supply on Next Generation's Within-Couple Decisions

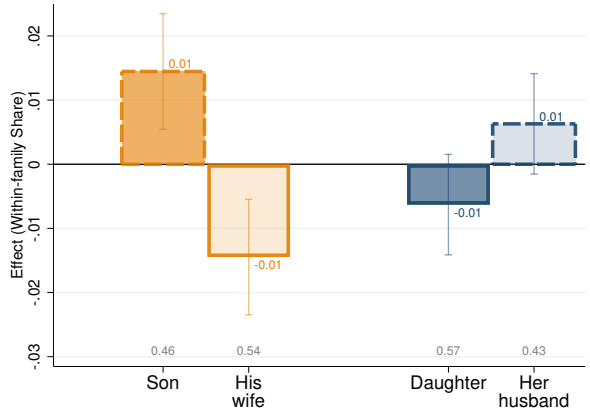
(a) Income residual of human capital



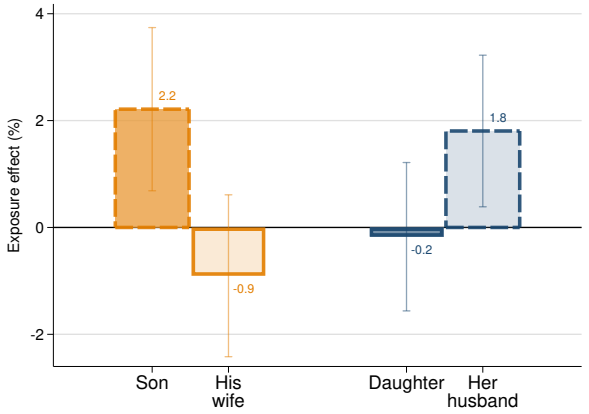

(b) Parental Leave per Child




(c) Sick-child Leave per Child



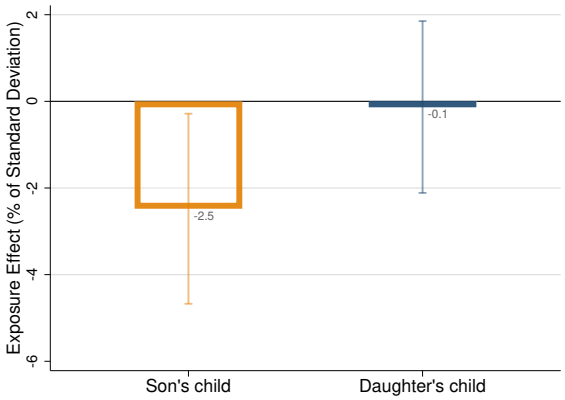

(d) Child Penalty



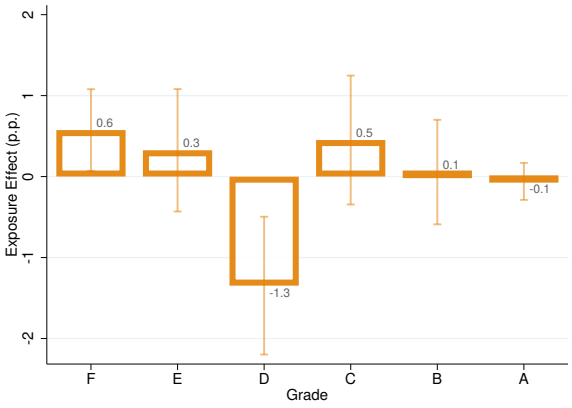

*Notes:* This figure presents IV estimates of the effect of a one-year exposure at *preschool age* to a 10-hour increase in maternal weekly working hours, $\gamma_a$ in equation (8) and estimated using specification (10). Panel (a) outcome is income unexplained by human capital, residuals from Figure 7, and time spent per child on leave to take care of a newborn or a sick child in Panels (b) and (c). Panel (d) is the probability of a large child penalty, defined as a proportional drop in earnings from pre-birth to post-birth below the gender-specific median. Whiskers show 95 percent confidence intervals based on robust standard errors. Numbers reported below bars denote sample means of the corresponding outcomes.

Figure 11: Effect on School Performance of in the Third Generation

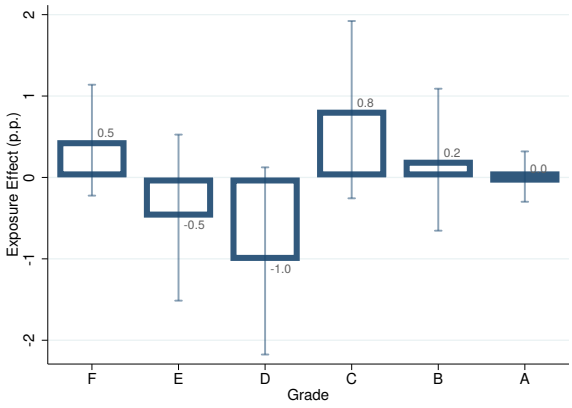
(a) Standardized Score




(b) Distributional Effect: Son' Child




(c) Distributional Effect: Daughter's Child




*Notes:* This figure shows the effect on the school performance of children of individuals exposed to a working mother at preschool ages. School performance is measured by grades on exams in the spring term of 9th grade, at age 16. **Panel (a)** reports effects on the grade-point score, standardized by birth cohort and gender, separately for children of exposed sons and exposed daughters. **Panels (b) and (c)** report effects on the probability of receiving each average letter grade, from F to A, for children of exposed sons and exposed daughters, respectively; estimates are expressed in percentage points. All panels present IV estimates of the effect of one additional year of exposure at preschool age to a 10-hour increase in maternal weekly working hours, corresponding to $\gamma_a$ in equation (8) and estimated using the sibling-difference specification design described in equation (10)). Vertical lines show the 95 percent confidence intervals based on robust standard errors. The sample includes the first child of each sibling and is restricted to sibling pairs in which both siblings have at least one child.

Table 1: Effect of Mother's Labor Supply on Family Formation and Spousal Characteristics

| | (1) Having Spouse | (2) Spouse Age | (3) Spouse Education | (4) Any Child | (5) Completed Fertility | (6) Age First Child |
|---|---|---|---|---|---|---|
| Effect on sons | -0.00319 | -0.0606 | 0.000467 | -0.00519 | -0.00686 | -0.126* |
| | (0.00379) | (0.0468) | (0.00248) | (0.00397) | (0.0114) | (0.0649) |
| Effect on daughters | 0.00430 | -0.0768* | -0.0149*** | 0.00223 | -0.00121 | 0.0769 |
| | (0.00336) | (0.0433) | (0.00313) | (0.00325) | (0.0104) | (0.0492) |
| Mean dep. var. sons | 0.797 | 50.12 | 0.970 | 0.846 | 1.712 | 30.08 |
| STD dep. var. sons | 0.347 | 3.793 | 0.171 | 0.361 | 1.025 | 4.912 |
| Mean dep. var. daughters | 0.846 | 54.33 | 0.931 | 0.904 | 1.908 | 27.60 |
| STD dep. var. daughters | 0.313 | 4.055 | 0.254 | 0.295 | 0.939 | 4.546 |
| Siblings' Age FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Siblings' Gender FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Education FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Age FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| F-stat sons | 440.5 | 303.0 | 305.7 | 440.5 | 440.5 | 297.4 |
| F-stat daughters | 464.0 | 414.0 | 409.2 | 464.0 | 464.0 | 383.7 |
| Sample Size | 269948 | 208310 | 207854 | 269948 | 269948 | 208113 |

*Notes:* This table reports the effect of maternal labor supply on family formation and spousal characteristics for those expose at preschool ages, estimated separately for sons and daughters. Each column reports IV estimates from the sibling-difference design described in equation (10), where estimates are effects of a one-year earlier exposure to a 10-hour increase in maternal weekly working hours. A Spouse is defined as the first partner (either married or cohabiting with a child) observed between ages 30 and 50. The dependent variables are as follows. **Column (1)**: an indicator for having a spouse; individuals without a spouse are those with no observed partner between ages 30 and 50. **Column (2)**: spouse age in 2020, conditional on having a spouse. **Column (3)**: indicator for whether spouse has post-compulsory education, conditional on having a spouse. **Column (4)**: an indicator for having any child. **Column (5)**: completed fertility, measured as the total number of children. **Column (6)**: age at first childbirth, conditional on having any. All fertility outcomes are measured through 2020. Standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 2: Heterogeneity of the Effect of Mother's Labor Supply on Lifetime Income

| | (1) Mother education | (2) Grandmother availability (GM) | (3) Childcare availability (CC) | (4) GM \| low CC | (5) GM \| high CC |
|---|---|---|---|---|---|
| Low | -0.014** | -0.028*** | -0.037*** | -0.049*** | -0.006 |
| | (0.007) | (0.007) | (0.009) | (0.010) | (0.010) |
| High | -0.039*** | -0.005 | -0.006 | -0.013 | 0.004 |
| | (0.010) | (0.009) | (0.010) | (0.013) | (0.013) |
| F-stat | 182 | 261 | 259 | 138 | 128 |
| p-value | 0.012 | 0.027 | 0.029 | 0.018 | 0.527 |
| Sample size | 266718 | 266718 | 266718 | 132874 | 133841 |

*Notes:* This table presents IV estimates of the effect of mother's labor supply on children's log lifetime income by the heterogeneity dimension in the column header. The sample ispreschool children pooled across genders. Each column reports estimates from the sibling-difference IV specification described in equation (10). Heterogeneity in **Column (1)** by mother's education (High refers to mothers with at least a high school degree); **Column (2)** by grandparent (GM) availability (High if in 1970 at least one grandmother resides in the same municipality as the child, is age 60 or younger, and is not employed; **Column (3)** by childcare (CC) availability (High in municipalities above the sample median); **Columns (4)** and **(5)** examine whether grandparent availability matters differently depending on formal childcare availability. They estimate heterogeneity by GM availability among children in low-CC municipalities and high-CC municipalities, respectively. CC availability is measured at the municipality level using the number of childcare workers per preschool-aged child with an employed mother, where employed mothers are defined as working at least 25 hours per week in 1970. The median child in the sample lives in a municipality with about 2 childcare workers per 10 preschool-aged children with working mothers. Standard errors are reported in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

# Online Appendix

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Monday 27th July, 2026

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# A Data, Tax Reform, etc

## A.1 Data

Labor Force Participation

Figure , panel (a). For 1963–1984, all series draw on Gustafsson and Lantz (1985). For 1984–2004, the series for married women, single women, and men are constructed as weighted indices using age-specific labor force participation rates from *Statistisk årsbok för Sverige* (Statistical Yearbook of Sweden) combined with population data from Statistics Sweden (SCB). For men, the series is extended through 2020 using SCB data. The series for women with pre-school-age children is drawn from Gustafsson and Lantz (1985) for 1963–1984 and from SCB for 2004–2024.

The Gross Enrollment Ratio is defined as total enrollment at the pre-primary level (ISCED level 02), expressed as a percentage of the population of the official age group for this level of education. For all five countries shown—Sweden, France, Spain, the United States, and the United Kingdom—the official entrance age is 3 years, with the age range extending to the official entrance age of primary education (based on the ISCED 02 official entrance age table). Because the numerator includes children enrolled outside the official age range (e.g., under- or over-age enrollment), the values may exceed 100 percent. The variable use is described as "Gross enrollment ratio, pre-primary, both sexes (%)" from the UNESCO Institute for Statistics, Stat Bulk Data Download Service.

## A.2

Many countries adopted major reforms moving joint or family-based personal income taxation toward mandatory individual taxation, including Japan 1950, New Zealand 1963, Denmark 1970, Sweden 1970 (effective 1971), Austria 1972 (effective 1973), the Netherlands 1973—with broader individualization completed in 2001—Finland 1975 (effective 1976), Italy 1976 (implemented in 1977), Iceland 1978 (effective 1979), Belgium 1988 (effective 1989), the United Kingdom 1988 (effective 1990), South Africa 1991, the Philippines 1992, the Republic of Korea 2003, Singapore 2004 (effective 2005), the Czech Republic 2007 (effective 2008), Estonia 2016 (effective 2017), Norway 2017 (effective 2018), and Kenya 2023; Switzerland approved a prospective individual-taxation reform in 2026, with implementation required no later than 2032. Here, we report the comparative coding year and identify the first operative year when it differs; because several reforms initially applied only to certain income categories or removed residual joint provisions, these dates should be understood as major individualization milestones rather than homogeneous one-step full reforms.

Some other systems introduced or retain optional or partial separate assessment, including Germany 1957, Ireland 1980, Spain 1988/1989, Hong Kong 1989/90, Malaysia 1991, Poland 1991, Israel 1992, Thailand 2012, Portugal 2014, Luxembourg 2017, Greece 2018, Malta 2019, and the United States; at the U.S. federal level, no comparable reform year should be assigned because married couples may file jointly or separately, while the Revenue Act of 1948 expanded income

splitting for joint returns rather than replacing joint taxation with mandatory individual taxation.

# B Statutory Labor Income Tax Functions

This appendix documents the exact statutory labor income tax schedules used to construct marginal and average tax rates in 1970 (pre-reform) and 1975 (post-reform). All parameters correspond exactly to the tax functions implemented in the replication code.

Throughout, income is measured in SEK. Because municipality-specific tax rates are unavailable prior to 1974, we use the average local tax rate in each year.

## B.1 Pre-Reform Taxation in 1970: Joint Assessment

In 1970, married couples were subject to joint taxation of labor income at the central-government level. Let $y_f$ denote the wife's labor income, $y_m$ the husband's labor income, and $c$ the number of children under age 16.

### B.1.1 Deductions

**Basic deduction.** Each spouse received a basic deduction of $D^{basic} = 2{,}250$.

**Work deduction (*förvärvsavdrag*).** For the wife, the work deduction was:

$$D_f^{work} = \begin{cases} 300 & \text{if } c = 0, \\ 300 + \min(0.25 y_f,\ 2{,}700) & \text{if } c > 0. \end{cases}$$

Thus, for mothers with children, the deduction phased in at 25% of earnings up to an additional maximum of 2,700 SEK.

### B.1.2 Local Tax

Local taxable income for the wife is

$$y_f^{loc} = y_f - D^{basic} - D_f^{work}.$$

The average local tax rate in 1970 was $\tau_{1970}^{loc} = 0.2024$. The husband's local tax is computed analogously using his own income and the same basic deduction.

### B.1.3 Deduction of Local Taxes from the State Base

Under the pre-reform system, local taxes paid were deductible from the state tax base, subject to a cap:

$$D^{loc} = \min(4{,}500,\ T_f^{loc} + T_m^{loc}).$$

### B.1.4 State Tax Base and Progressive Schedule

State taxable income under joint assessment is

$$y^{state} = y_f + y_m - D^{basic} - D_f^{work} - D^{loc}.$$

The 1970 central-government marginal tax schedule for married couples was:

$$\begin{aligned}
10\% \quad & \text{above } 0, \\
15\% \quad & \text{above } 12{,}000, \\
22\% \quad & \text{above } 16{,}000, \\
27\% \quad & \text{above } 20{,}000, \\
34\% \quad & \text{above } 24{,}000, \\
42\% \quad & \text{above } 30{,}000, \\
48\% \quad & \text{above } 40{,}000, \\
54\% \quad & \text{above } 60{,}000, \\
59\% \quad & \text{above } 100{,}000, \\
65\% \quad & \text{above } 150{,}000.
\end{aligned}$$

Total state tax is computed on the joint base. The wife's state tax liability is assigned proportionally:

$$T_f^{state} = \frac{y_f}{y_f + y_m} \cdot T_{family}^{state}.$$

### B.1.5 Total Tax Liability in 1970

$$T_f^{1970} = T_f^{loc} + T_f^{state}.$$

Because the state schedule applies to pooled income, the marginal tax rate on the wife's earnings depends explicitly on $y_m$.

## B.2 Post-Reform Taxation in 1975: Individual Assessment

After 1971, labor income was taxed individually. Let $y$ denote individual labor income.

### B.2.1 Deductions

**Basic deduction.** $D^{basic} = 4{,}500.$

**Work deduction.** $D^{work} = \min(0.20y,\ 2{,}000).$

### B.2.2 Local Tax

Local taxable income:

$$y^{loc} = y - D^{basic} - D^{work}.$$

Average local tax rate in 1975: $\tau^{loc}_{1975} = 0.2523$.
Local tax liability:

$$T^{loc} = \tau^{loc}_{1975} \cdot \max(y^{loc}, 0).$$

Unlike 1970, local taxes are no longer deductible from the state tax base.

### B.2.3 State Tax Base and Progressive Schedule

$$y^{state} = y - D^{basic} - D^{work}.$$

The 1975 marginal tax schedule was:

$$\begin{aligned}
7\% &\quad \text{above } 0,\\
12\% &\quad \text{above } 15{,}000,\\
17\% &\quad \text{above } 20{,}000,\\
22\% &\quad \text{above } 25{,}000,\\
28\% &\quad \text{above } 30{,}000,\\
33\% &\quad \text{above } 40{,}000,\\
38\% &\quad \text{above } 45{,}000,\\
43\% &\quad \text{above } 65{,}000,\\
48\% &\quad \text{above } 70{,}000,\\
52\% &\quad \text{above } 100{,}000,\\
58\% &\quad \text{above } 150{,}000.
\end{aligned}$$

### B.2.4 Special Tax Reduction

A special tax reduction applied at low income levels:

$$STR = \begin{cases} 250 & \text{if } y < 36{,}000, \\ 250 - 0.10(y - 36{,}000) & \text{if } 36{,}000 \leqslant y \leqslant 38{,}500, \\ 0 & \text{otherwise.} \end{cases}$$

### B.2.5 Spousal Tax Reduction

For secondary earners, the spousal tax reduction was:

$$SpousalReduction = \max(0,\ 1{,}800 - 0.4y).$$

### B.2.6 Total Tax Liability in 1975

$$T^{1975} = T^{loc} + T^{state} - STR - SpousalReduction.$$

Tax liability is truncated at zero if negative.

## B.3 Key Mechanical Differences

Relative to 1970, the reform introduced three mechanical changes that are central for our empirical design:

1. Labor income was taxed individually rather than jointly.
2. Local taxes were no longer deductible from the state tax base.
3. New tax reductions (spousal and special reductions) altered the effective schedule at low income levels.

The elimination of joint assessment is the first-order change: under individual taxation, the marginal tax rate on the wife's earnings no longer depends on the husband's income. This generates heterogeneous reductions in participation and marginal tax rates across households that vary systematically with pre-reform husband income.

# C Childcare Institutions and Childcare Use

In the late 1960s and early 1970s, childcare in Sweden operated through a mixed system of formal and informal arrangements. Public provision was limited and highly rationed, and the majority of working parents relied on informal or semi-formal solutions to meet their childcare needs.

### Formal childcare

Formal childcare—referred to collectively as *barnstugor*—comprised several distinct forms that differed in both time coverage and function. First, childcare centers (*daghem*) provided full-day care for children from infancy to school entry (approximately 6 months to 7 years). These centers were typically open 8–10 hours per day and were explicitly intended to support full-time employment.

Second, after-school centers (*fritidshem*) offered care for younger school-age children outside regular school hours, including afternoons, holidays, and school breaks. Third, play schools and part-day preschools (*lekskolor* or *halvdagshem*) provided part-day care—usually around three hours per day—primarily for preschool-aged children. These institutions were not designed to cover a full working day and were often operated by voluntary organizations rather than municipalities. Finally, some municipalities ran combined institutions (*gemensamma barnstugor*) that integrated features of full-day and part-day care to accommodate heterogeneous care needs.

Across settings, children were organized in age-based groups. Typical group sizes ranged from 10–15 children for younger preschoolers to 15–20 children for older preschoolers, with regulated staff-to-child ratios. Municipalities were the dominant providers of formal childcare, though voluntary organizations played an important role in part-day provision. Access to institutional childcare was selective: priority was generally given to children of single parents, households in which both parents worked full time, or families with documented social or medical needs. There was no universal entitlement, and capacity constraints were severe.

### Informal childcare

Given the limited availability of formal care, most working parents relied on informal arrangements. A central component of this system was municipal family childcare (*kommunala familjedaghem*), in which children were cared for in private homes by childminders (*dagbarnvårdare*) under municipal organization or supervision. Municipal family childcare expanded rapidly from the late 1940s onward and served as a flexible complement to institutional childcare, particularly in municipalities with limited childcare capacity. From the perspective of local governments, family childcare was attractive because it required no capital investment and could be scaled quickly. At the same time, it was widely regarded as a second-best alternative to formal care, reflecting concerns about pedagogical quality, continuity, and oversight.

In addition, many families relied on private family childcare (*privata familjedaghem*), which consisted of similar home-based arrangements operating outside municipal oversight or financial support. Family childcare settings—both municipal and private—typically involved small groups of three to five children. Another important alternative was care provided in the child's own home. This category encompassed a wide range of arrangements, including care by parents (often through staggered work schedules), grandparents or other relatives, siblings, domestic workers (*hembiträden*), or other helpers. Finally, a small share of families relied on shared arrangements, such as the so-called "three-family system," in which several households jointly employed a childcare worker.

### Childcare use

To document the prevalence of different childcare arrangements prior to the tax reform, we use data from a nationally representative interview survey conducted in autumn 1966. To our knowledge, this is the best evidence of childcare use close before the tax reform. The childcare survey was implemented as a special supplement to Statistics Sweden's Labour Force Survey, which covered approximately 60,000 individuals, of whom about 50,000 formed the sampling frame for this study. The survey covers children under age 10 whose custodial parent(s) worked at least 15 hours per week and records the main childcare arrangement in use at the time of interview.

The sampling design combined stratified sampling from the Labour Force Survey with administrative sampling of municipal family childcare providers, yielding a final sample of 2,517 children. Data were collected primarily via telephone interviews with parents and, where relevant,

family childcare providers. Reported nonresponse was low (2.3 percent), and a small validation exercise found no evidence of systematic measurement error. Survey weights were used to extrapolate the results to a national population of approximately 317,000 children.

Table A.1 summarizes the distribution of childcare arrangements used by children of working parents, separately for all children aged 0–10 and for preschool children aged 0–7. Childcare is classified into institutional arrangements—full-time and part-time center-based care (*barnstugor*, including *daghem*, *fritidshem*, and *lekskola/halvdagshem*)—and non-institutional arrangements, including municipal and private family childcare (*familjedaghem*) and care provided in the child's own home.

Two patterns stand out. First, institutional childcare played a limited role: even among preschool children, fewer than one in seven were enrolled in full- or part-time center-based care. Second, non-institutional arrangements dominated, reflecting both capacity constraints in municipal provision and the importance of informal solutions. Among children in private family childcare, care was most commonly provided by non-relatives (57 percent), though a substantial share relied on grandparents (37 percent) or other relatives (6 percent). Care in the child's own home was similarly heterogeneous: in 38 percent of cases the child was cared for by the non-working spouse, while other arrangements included paid nannies (25 percent), grandparents (15 percent), siblings (6 percent), and other relatives or helpers (16 percent). Overall, these patterns underscore the extent to which families relied on diverse home-based arrangements in the absence of widely available institutional childcare.

### Availability of childcare across regions

The report emphasizes that the availability of childcare options varied substantially across municipalities, and that the choice of arrangement was often shaped by limited local supply rather than parental preferences. Among families using family childcare (*familjedaghem*), the most frequently cited reason for this choice were logistical considerations, such as proximity to the home, which accounted for the majority of responses. However, capacity constraints in institutional childcare played a central role: lack of available places in childcare centers or after-school centers was reported as one of the main reasons for choosing family childcare. In addition, approximately 10 percent of families using family childcare reported that their working hours were incompatible with the opening hours of institutional childcare, suggesting that family childcare absorbed demand from parents with nonstandard schedules.

Selection into different childcare arrangements also varied systematically across regions. Of all children with working parents, 26 percent lived in metropolitan areas, 22 percent in medium-sized cities, and 52 percent in smaller municipalities. Institutional childcare was substantially more prevalent in metropolitan areas, where 12 percent of children had access to *barnstugor*, compared with 6 percent in medium-sized cities and only 2 percent in smaller municipalities. By contrast, private family childcare was most prevalent outside the largest cities, reaching 25 percent in some regions, compared with 17–18 percent elsewhere.

Table A.1: Childcare use by Care Type and Age Group in 1966

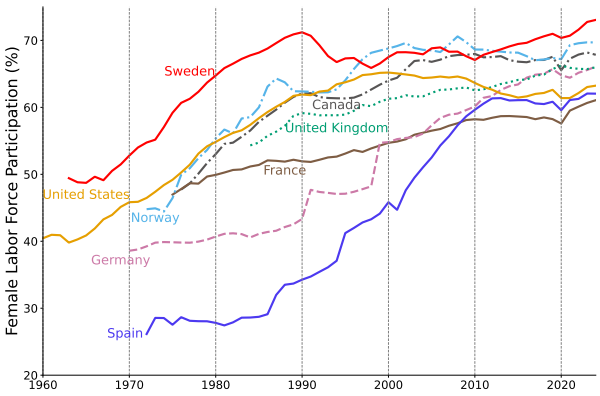
| | **Children aged 0–10** | | | | **Children aged 0–7** | | | |
|---|---|---|---|---|---|---|---|---|
| | At home | Private | Public | Total | At home | Private | Public | Total |
| Grandmothers | 9.2 | 9.3 | 0.4 | 19.0 | 9.6 | 13.4 | 0.2 | 23.1 |
| Other relatives | 5.4 | 1.5 | 0.1 | 3.0 | 5.7 | 2.2 | 0.0 | 4.1 |
| Non-relatives | 23.8 | 15.9 | 10.3 | 54.0 | 24.8 | 23.5 | 18.1 | 70.3 |
| Child on its own | 24.1 | 0.0 | 0.0 | 24.1 | 2.5 | 0.0 | 0.0 | 2.5 |
| Total | 62.5 | 26.7 | 10.8 | 100.0 | 42.6 | 39.0 | 18.4 | 100.0 |

*Notes:* The table reports the distribution of children with parents working more than 15 hours per week in 1966, by care setting and caregiver other than they parent. "At home" refers to care provided in the child's own home; "Private" refers to private family childcare (privata familjedaghem); and "Public" refers to municipally organized childcare, including childcare centers (barnstugor) and municipal family childcare (kommunala familjedaghem). "Other relatives" refers to other relatives than grandmothers, including siblings. Percentages are shares of all children in the relevant age group and sum to 100 within each age group. The category "child on its own" captures children without adult supervision. *Source:* Authors' calculations based on SOU 1967:39.

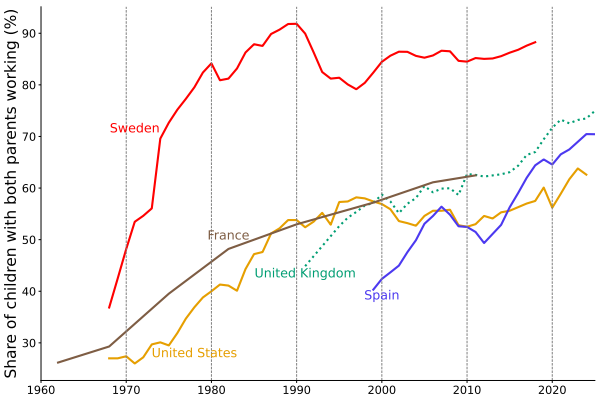
# D Additional Tables and Figures

Figure A.1: Labor Force Participation (LFP) of Women, Mothers, and Men 1960-2020

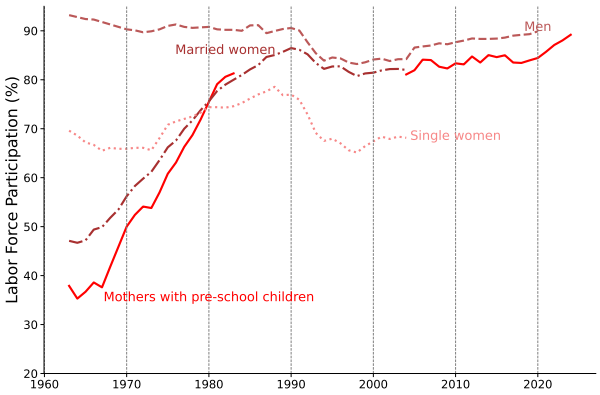
(a) Female LFP by Country



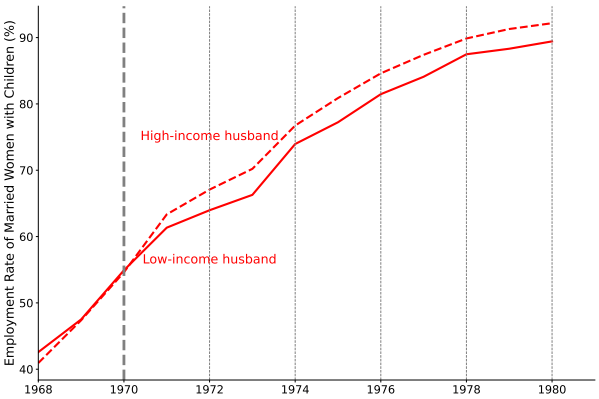

(b) Children with Working Parents by Country




(c) LFP by gender & marital status in Sweden

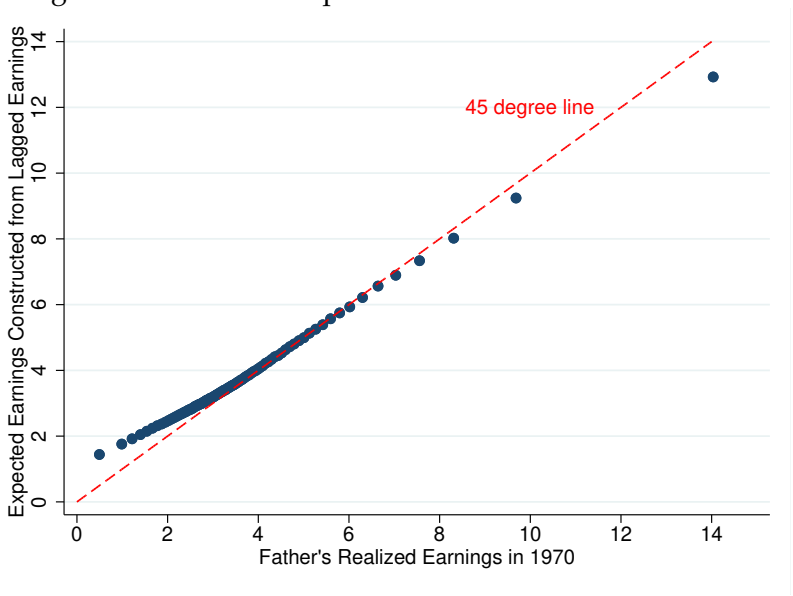



(d) Emp. of Swedish women by husband's income




*Notes:* The figure shows long-run trends in labor force participation (LFP) of women and men, and the rising share of children in dual-earner households. **Panel (a)** plots LFP rates of women aged 15–74 across a selection of countries, illustrating a broad upward trend. Data are from the OECD. **Panel (b)** plots the share of children aged 0–6 with both parents employed full- or part-time across countries. The share rises markedly over the period in all countries, reflecting the increase in labor force participation among mothers. For Sweden, data are drawn from the Income and Taxation Register (IoT) and the Multigenerational register. For the United Kingdom, data come from microdata accessed via the UK Data Service, supplemented by IPUMS International. For France and Spain, data are from IPUMS International and the Encuesta de Población Activa (EPA), respectively. For the United States, data are from the IPUMS Current Population Survey (CPS). **Panel (c)** plots LFP by gender and marital status in Sweden, showing a rapid rise in participation among married women—especially mothers—during the late 1960s and 1970s. The series are constructed from three sources: (Gustafsson and Lantz, 1985), Swedish Statistical Yearbooks published by Statistics Sweden (SCB), and population records from SCB. See Appendix A for details. **Panel (d)** plots employment of married women in Sweden around the 1971 tax reform, showing that the rise in employment was faster after the reform among women married to above-median-earning husbands than among those married to below-median-earning husbands. The series are constructed using the Income and Taxation Register (IoT).

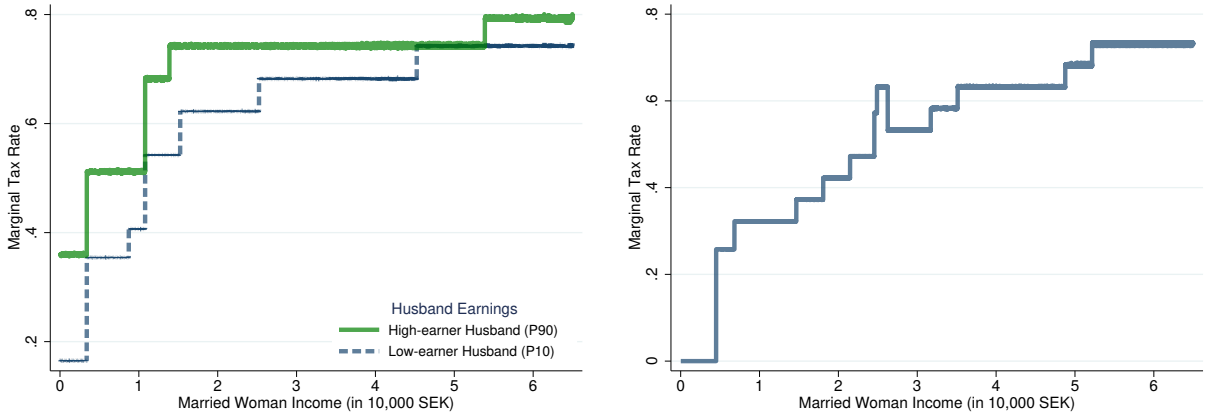
Figure A.2: Father's Expected vs Realized Income in 1970




*Notes:* This figure shows the constructed expected labor income of fathers in 1970 using their lagged earnings against their actual income in 1970. The expected income is constructed in the following way: we first estimate a reduced-form income process in which fathers' earnings in 1970 are regressed on their lagged earnings in 1968 and 1969. To allow for heterogeneous income dynamics, we interact lagged earnings with indicators for the father's education category and year of birth. Then, the resulting fitted value serves as our measure of expected paternal earnings and is used throughout the analysis. For more information refer to Section 1.1.

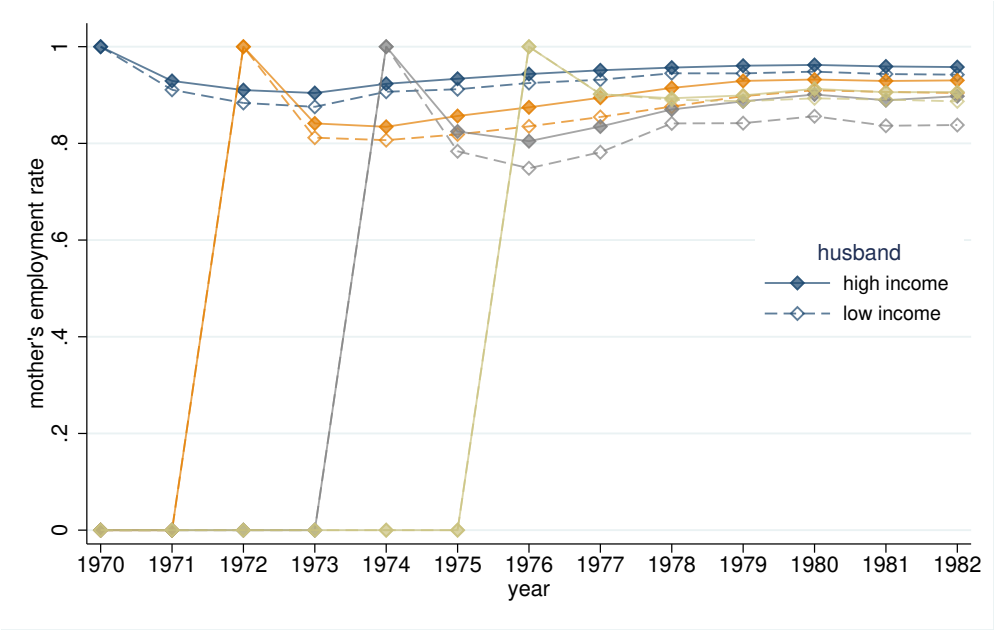
Figure A.3: Tax Schedule of Married Women

(a) Pre-reform in 1970 (b) Post-refrom in 1975



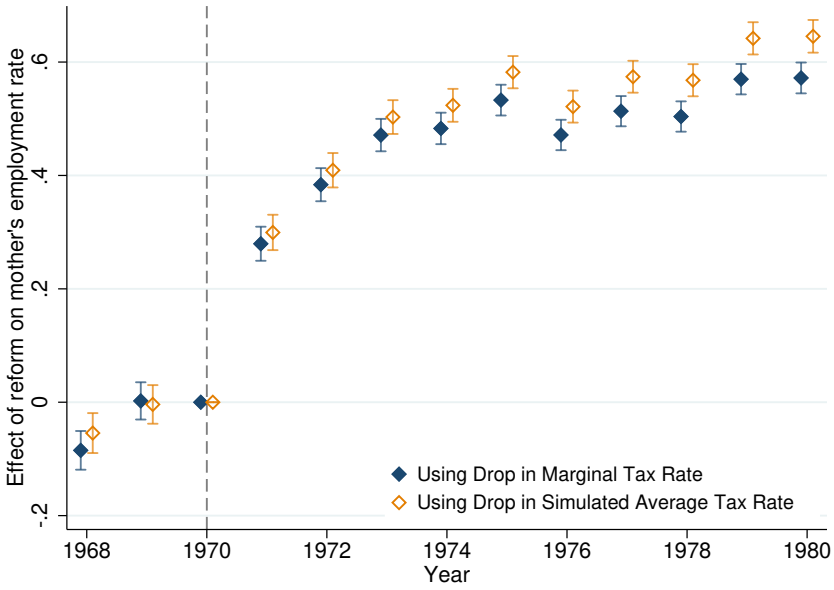

*Notes:* This figure shows the marginal labor income tax for married women in Sweden under joint taxation in 1970 (**Panel (a)**), where married women's marginal tax rate depended on husbands' income, and under individual taxation in 1975—the year we are able to measure mothers' labor supply (**Panel (b)**). Low- and high-earner husbands correspond to the 10th and 90th percentiles ($\approx$ 24 and 55 kSEK).

Figure A.4: Employment Persistence Across Married Women Joining Labor Market




*Notes:* This figure plots mothers' employment rates by calendar year separately by the year in which the mother first enters employment and by whether the husband's 1970 labor income is above or below the sample median. The sample consists of mothers in our main estimation sample, corresponding to families with children at preschool and school ages. Employment is defined as positive annual labor earnings. For each entry cohort, the figure reports average employment rates from 1970 to 1982 for mothers whose first observed employment year is 1970, 1972, 1974, or 1976. Husband income groups are defined using pre-reform paternal labor income in 1970.

Figure A.5: Robustness Check- Employment Effect of Change in Simulated Average Tax Rate

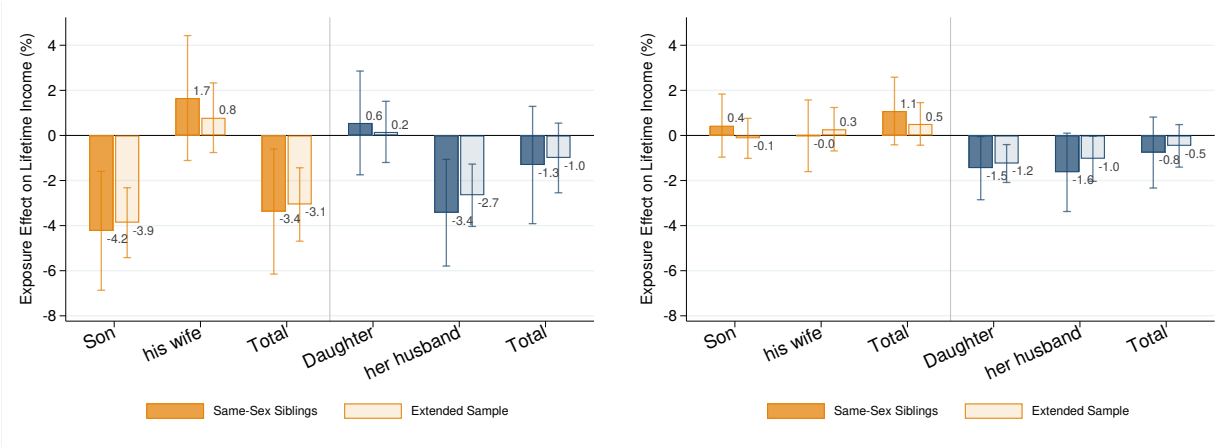



*Notes:* This figure replicates the event-study specification in Figure 4d, estimating the effect of the tax reform on mothers' employment over time. The baseline specification (solid markers) uses the reform-induced change in the marginal tax rate as the source of variation (the same as Figure 4d), while the alternative specification (hollow markers) replaces this measure with the reform-induced change in the average tax rate. Each point corresponds to a year-specific coefficient, with vertical bars indicating confidence intervals. The vertical dashed line marks the year of the reform (1971).

Figure A.6: Robustness 1: Same-sex Siblings Data compared to the Extended Sample

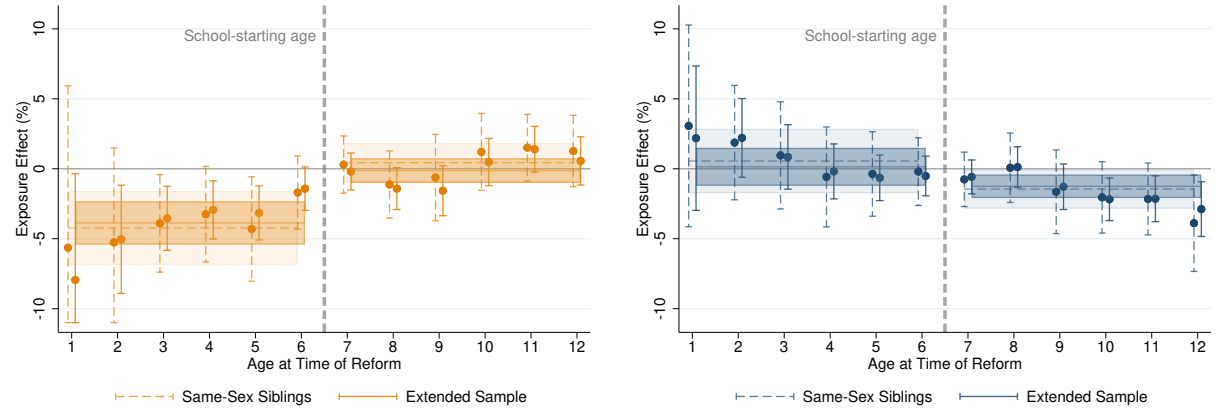
(a) Exposed while in Preschool Age

(b) Exposed while in Primary School Age




(c) Average Effect on Lifetime Income: Sons

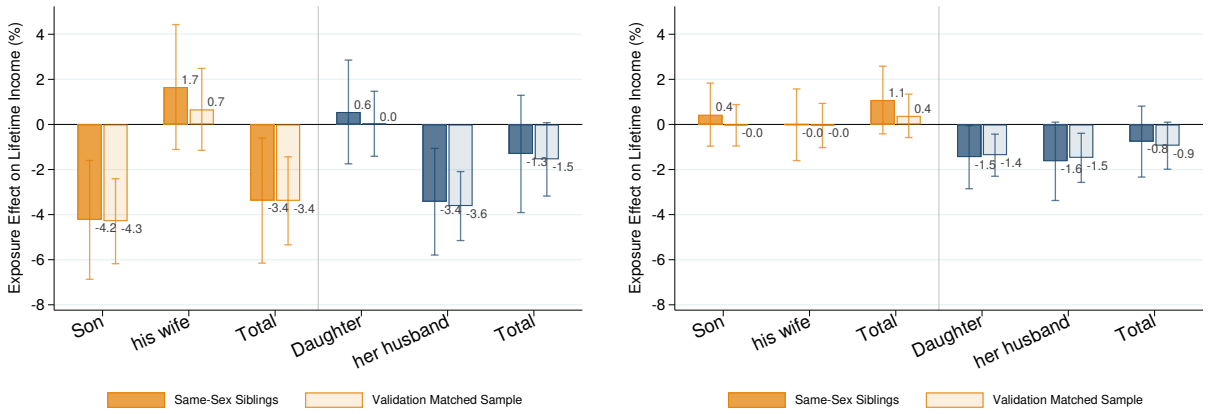
(d) Average Effect on Lifetime Income: Daughters




*Notes:* This figure compares the main estimates using the extended sample to the estimates from the smaller same-sex siblings data.

Figure A.7: Robustness 2: Validating the Matching Method

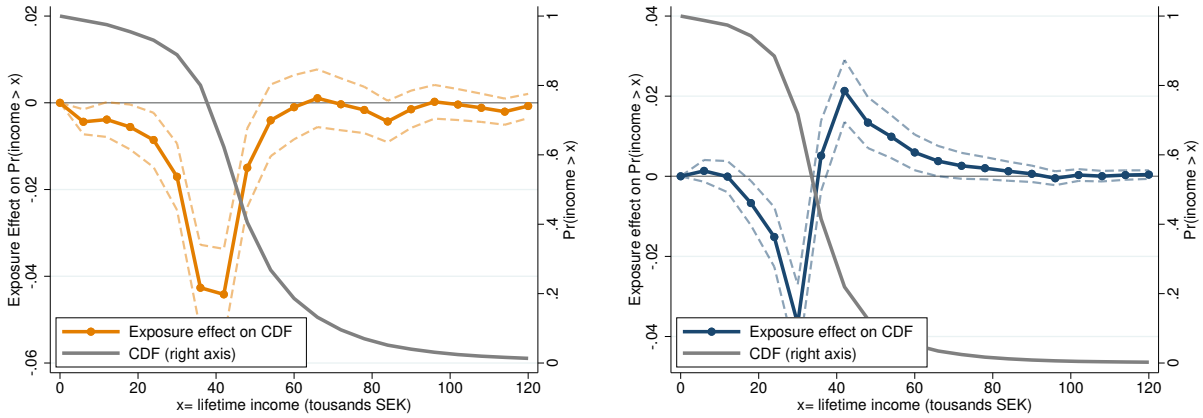
(a) Exposed while in Preschool Age (b) Exposed while in Primary School Age




*Notes:* This figure compares the estimates using the same-sex siblings and a validation sample, constructed using the same matching method from the same-sex siblings data.

Figure A.8: Effects of Mother's Labor Supply Across the Next Generations' lifetime Income

(a) Distributional Effect (CDF): preschool Sons (b) Distributional Effect (CDF): preschool daughters




*Notes:* **Panels (a)-(b)** shows how the mother's labor supply change the cumulative distribution of lifetime income in the next generation. The shaded region reports the average effect separately for pre-school and primary school-aged children. Vertical bars represent 95 percent confidence intervals based on robust standard errors. Lifetime income is defined as the mean of an individual's total labor income between ages 30 and 50, with income measures deflated to 1970 prices. See Figure 6e-f.

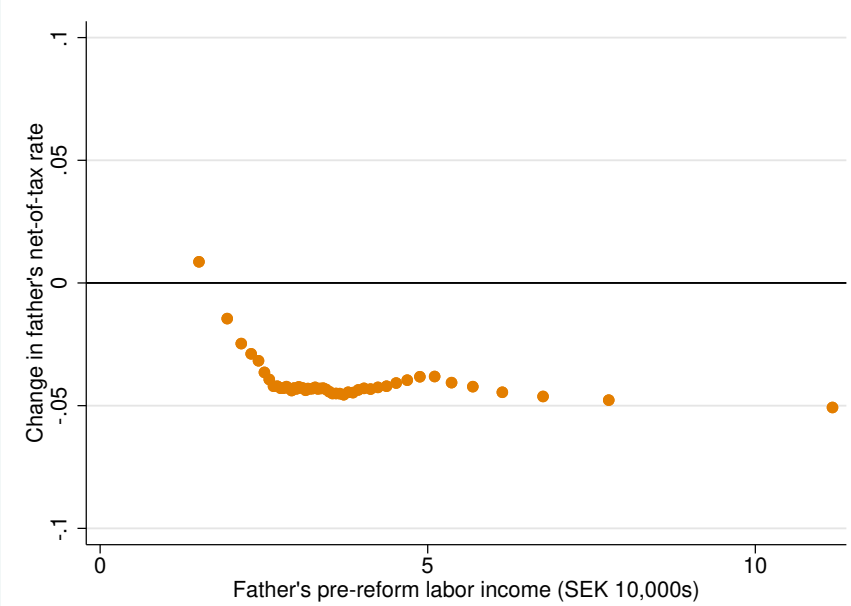
Table A.2: Effect on Long-Run Employment Rate Between Ages 30 and 50

| | Exposed while in Preschool Age | | | Exposed while in Primary School Age | | |
|---|---|---|---|---|---|---|
| | (1) own | (2) spouse | (3) total | (4) own | (5) spouse | (6) total |
| Effect on sons | -0.000130 | 0.00136 | 0.000615 | 0.00346* | 0.00823 | 0.00584* |
| | (0.00316) | (0.00727) | (0.00435) | (0.00207) | (0.00508) | (0.00301) |
| Effect on daughters | -0.000427 | -0.00428 | -0.00235 | -0.00137 | 0.0108* | 0.00472 |
| | (0.00296) | (0.00677) | (0.00405) | (0.00216) | (0.00562) | (0.00324) |
| Mean for sons | 0.957 | 0.567 | 0.762 | 0.960 | 0.573 | 0.766 |
| STD for sons | 0.155 | 0.385 | 0.227 | 0.145 | 0.391 | 0.226 |
| Mean for daughters | 0.959 | 0.620 | 0.789 | 0.965 | 0.631 | 0.798 |
| STD for daughters | 0.158 | 0.378 | 0.226 | 0.141 | 0.383 | 0.220 |
| F-stat boy | 61.39 | 61.39 | 61.39 | 175.7 | 175.7 | 175.7 |
| F-stat girl | 66.90 | 66.90 | 66.90 | 134.2 | 134.2 | 134.2 |
| Siblings' Age FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Siblings' Gender FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Mother Education FE | ✓ | ✓ | ✓ | ✓ | ✓ | ✓ |
| Sample Size | 93781 | 93781 | 93781 | 65937 | 65937 | 65937 |

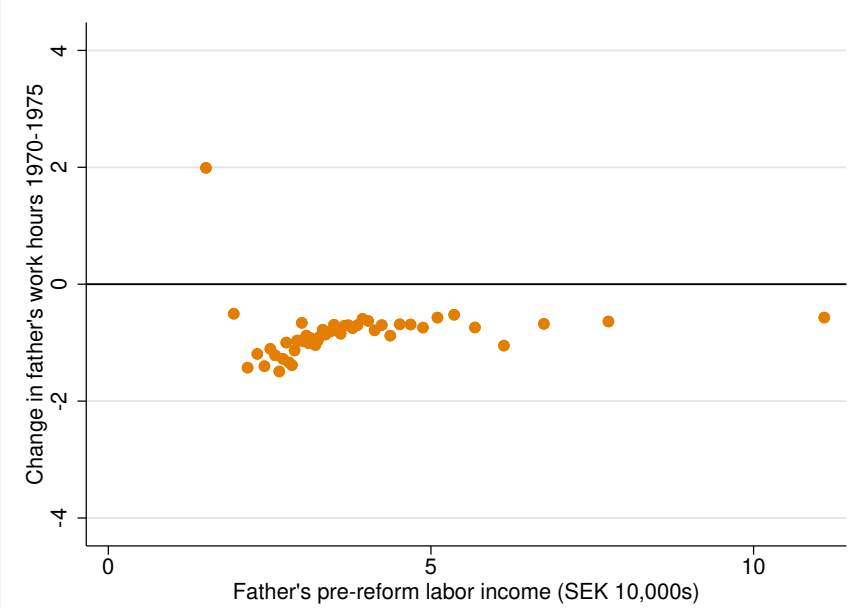
*Notes:* This table reports the effect of maternal labor supply on children's long-run labor market attachment, measured as the share of years with positive earned income between ages 30 and 50 (normalized by the maximum of 21 years). The treatment corresponds to a one-year earlier exposure to a persistent increase in maternal weekly labor supply. Columns (1)–(3) restrict the sample to individuals exposed during preschool ages at the time of the reform, while Columns (4)–(6) include those exposed during primary school ages. Columns (1) and (4) report effects on individuals' own employment, Columns (2) and (5) report effects on their partners' employment, and Columns (3) and (6) report effects on total family employment, defined as the sum of own and partner employment. The partner outcome is measured over the same age range (30–50) and is defined as the share of years in which the individual has a partner and that partner has positive earned income. Standard errors are reported in parentheses.

Figure A.9: Fathers' Tax Changes and Labor Supply Responses by Pre-Reform Income

(a) Change in Fathers' Net-of-Tax Rate




(b) Change in Fathers' Weekly Work Hours




*Notes:* **Panel (a)** plots the change in fathers' net-of-tax rate against pre-reform paternal labor income. **Panel (b)** plots the change in fathers' weekly work hours between 1970 and 1975 against pre-reform paternal labor income. Labor income is measured in 10,000 SEK. The sample consists of fathers in our main estimation sample, corresponding to families with children at preschool and school ages. The binned scatters are constructed analogously to Figure 4b, which presents the corresponding relationship for mothers' labor supply.

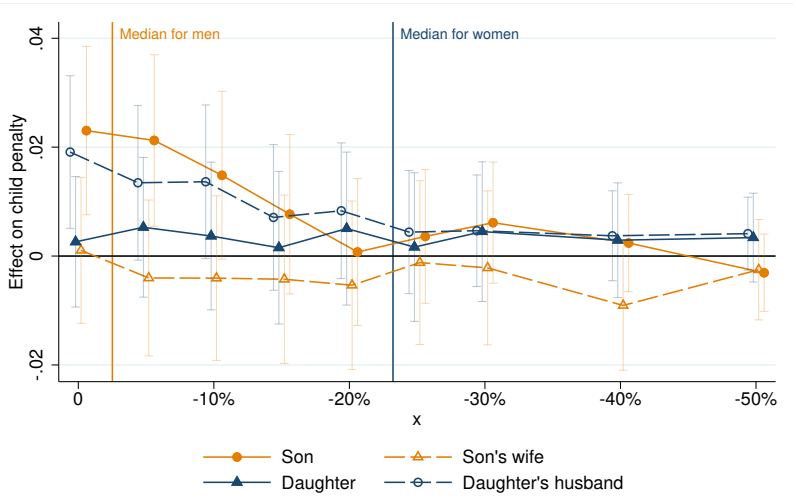
Table A.3: Empirical Test for Exclusion Restriction Assumption Using Placebo Sample

| | Main Sample | | Placebo Sample | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Instrument | -0.201*** | -0.158*** | -0.0624 | 0.00494 |
| | (0.0481) | (0.0488) | (0.127) | (0.129) |
| Constant | 0.108*** | 0.0975*** | 0.00269 | 0.000760 |
| | (0.0115) | (0.0118) | (0.00443) | (0.00453) |
| Siblings' Age FE | ✓ | ✓ | ✓ | ✓ |
| Siblings' Gender FE | ✓ | ✓ | ✓ | ✓ |
| Mother Education FE | | ✓ | | ✓ |
| Sample Size | 92787 | 92711 | 101509 | 101400 |

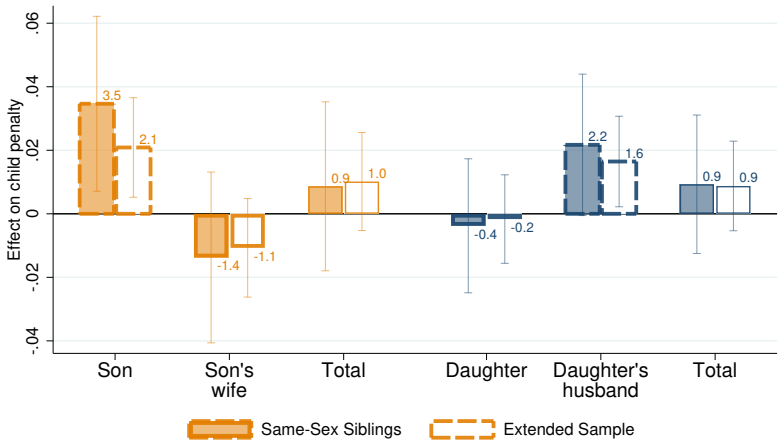
*Notes:* This table reports reduced-form estimates from regressions of sibling differences in log income on the instrument. Columns (1)–(2) present estimates for sibling pairs in which the younger sibling is of preschool age at the time of the 1971 reform. Columns (3)–(4) report estimates for a placebo sample consisting of sibling pairs born between 1972 and 1982, who are not exposed to the reform in their childhood. For this placebo sample, we impose a hypothetical reform in 1982 and define siblings' ages relative to this placebo reform. The instrument is constructed by applying the 1970 and 1975 tax schedules to parental earnings in 1982, generating a predicted change in marginal tax rates that varies with paternal income and captures the change in incentives that would have occurred had the reform taken place in 1982. The specification follows the baseline sibling-difference design and includes the same set of controls and fixed effects as in the main analysis.

Figure A.10: Maternal Employment Exposure and Child Penalties

(a) Penalty Thresholds




(b) Sample Comparison




*Notes:* The figure reports IV estimates of the effect of maternal employment exposure on child penalties at childbirth among adult children and their spouses. The child penalty is defined as the percentage change in residualized earnings from the pre-birth window to the post-birth window, where the pre-birth period is years $-2$ and $-1$ relative to childbirth and the post-birth period is years $+2$, $+3$, and $+4$. More negative values indicate larger child penalties. Panel A reports estimates for the probability that the child penalty is less than or equal to a given threshold, with the threshold varying from 0 to $-50$ percent. Vertical lines indicate the gender-specific median child penalty, where medians are calculated among older siblings in the control group. Panel B compares estimates from the original same-sex sibling sample with estimates from the full extended sample. In Panel B, the outcome is an indicator for having a child penalty at or below the gender-specific median, where medians are calculated among older siblings in the control group. Estimates are shown separately for sons, sons' wives, daughters, and daughters' husbands in Panel A, and for sons, wives, total family earnings, daughters, husbands, and total family earnings in Panel B. The original sample in Panel B is estimated without weights; the extended sample uses the provided sample weights. Regressions use the sibling-pair IV specification and include the baseline controls. Whiskers show 95 percent confidence intervals based on robust standard errors.

The economy is populated by overlapping generations of two-parent households. Each individual lives for two stages: childhood and adulthood. In childhood, the individual receives parental inputs: receives investment in time and money as well as observes equality among their parents $(T_{-1}, I_{-1}, E_{-1})$. Children's consumption decision is fixed, $c_0$, but they choose their study effort $s$ shaping their human capital,

$$h = H(s, T_{-1}, I_{-1}), \tag{20}$$

Childhood time is allocated between leisure and study: $\bar{l}_0 + s = 1$, i.e., studying comes at the cost of leisure. Human capital translates into adult wages through $w = W_g(h)$ for $g \in \{w, m\}$, where $W_g$ is strictly increasing and gender-specific due to labor market discrimination. This is the first dimension of gender differences.

The next generation's equality taste is a function of the couple-equality among their own parents $\gamma = \Gamma(E_{-1})$, where $\Gamma(\cdot)$ is strictly increasing. Importantly, we assume a gender-neutral norm transmission.

The first thing an adult does is to marry. Matching is perfectly assortative in a scalar index of wage (or equivalenltly, human capital) and inherited equality norms. For an individual of gender $g \in \{w, m\}$, the marriage-market index is $y_g = Y(w_g, \gamma_g)$ where $Y$ is strictly increasing in both arguments. Let $F_{Y,g}$ denote the gender-specific distribution of $y_g$, which we assume is continuous atomless and strictly increasing. Perfect assortative matching requires spouses to have the same rank in the index distribution:

$$F_{Y,w}(Y(w_w, \gamma_w)) = F_{Y,m}(Y(w_m, \gamma_m)). \tag{21}$$

A couple then decides their consumption and leisure decisions, as well as investments $(T, I, E)$ for the next generation. We can separate the couple's decision of time and children investment from individual decisions given those. Start with the latter:

Given wage $w$ and time spent elsewhere (devoted to studying in childhood or children in adulthood $t$), money spent elsewhere $x$ (net within family transfer, minus of standrad unreaned income), each individual choses consumption, leisure and labor income $(c, l, z)$ leading to the indirect utility:

$$v(w,\, t,\, x) \;:=\; \max_{c,l,z}\; u(c,\, l) \tag{22}$$

subject to budget and time constraints $c + c_0 + x = z$, and $l + \frac{z}{w} + t = 1$.

Adult household allocations are selected by fixed Pareto weights $(\eta_w, \eta_m)$, where $\eta_w + \eta_m = 1$. This is the second dimension of gender differences. Denote by $V_g(T, I, \gamma)$ the value associated with a child with gender $g$ raised under parental inputs and norm $(T, I, \gamma)$. The adult problem can then

be written compactly as:

$$\begin{aligned}\mathcal{U}(w_w, \gamma_w, w_m, \gamma_m) \;:=\; \max_{t_w, t_m, i_w, i_m} \; & \eta_w \left[v\big(w_w,\, t_w,\, i_w\big) + \gamma_w E\right] \\ & + \; \eta_m \left[v\big(w_m,\, t_m,\, i_m\big) + \gamma_m E\right] \\ & + \beta \left[V_w(T, I, \gamma) + V_m(T, I, \gamma)\right],\end{aligned} \tag{23}$$

subject to $\gamma = \Gamma(E)$, $E = e(t_w, t_m)$, and couple investments in kids are $T = t_w + t_m$, and $I = i_w + i_m$. Within couple transfer are redundent so ignored.

The spouse state $(w_{-g}, \gamma_{-g})$ is generally drawn from the conditional distribution of opposite-gender agents with the same index rank. Let $\Pi_{-g}(\cdot \mid p)$ denote this conditional distribution. Then the expected adult utility for a woman is

$$\mathcal{U}^e_w\big(w_w, \gamma_w\big) = \int \mathcal{U}\big(w_w, \gamma_w, w_m, \gamma_m\big)\, d\Pi_m\big(w_m, \gamma_m \mid F_{Y,w}\big(Y(w_w, \gamma_w)\big)\big)\big) \tag{24}$$

$$\mathcal{U}^e_m\big(w_m, \gamma_m\big) = \int \mathcal{U}\big(w_w, \gamma_w, w_m, \gamma_m\big)\, d\Pi_w\big(w_w, \gamma_w \mid F_{Y,m}\big(Y(w_m, \gamma_m)\big)\big)\big) \tag{25}$$

The value $V_g(.)$ includes the child's own childhood payoff and the continuation value of the efficient household the child forms in adulthood. Hence, the child of gender $g$ solves:

$$V_g(T_{-1}, I_{-1}, \gamma_g) = \max_s \; u(c_0, 1 - s) + \delta \mathcal{U}^e_g\big(w_g, \gamma_g\big) \tag{26}$$

subject to wage equation $w = W_g(h)$ and human capital condition (14).