Online Appendix

Do Family Policies Reduce Gender Inequality? Evidence from 60 Years of Policy Experimentation

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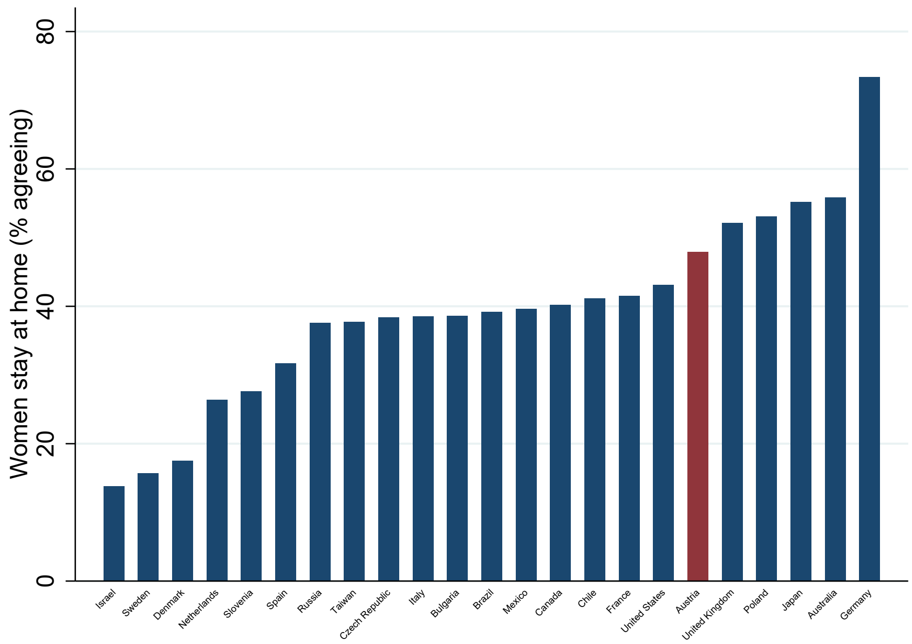
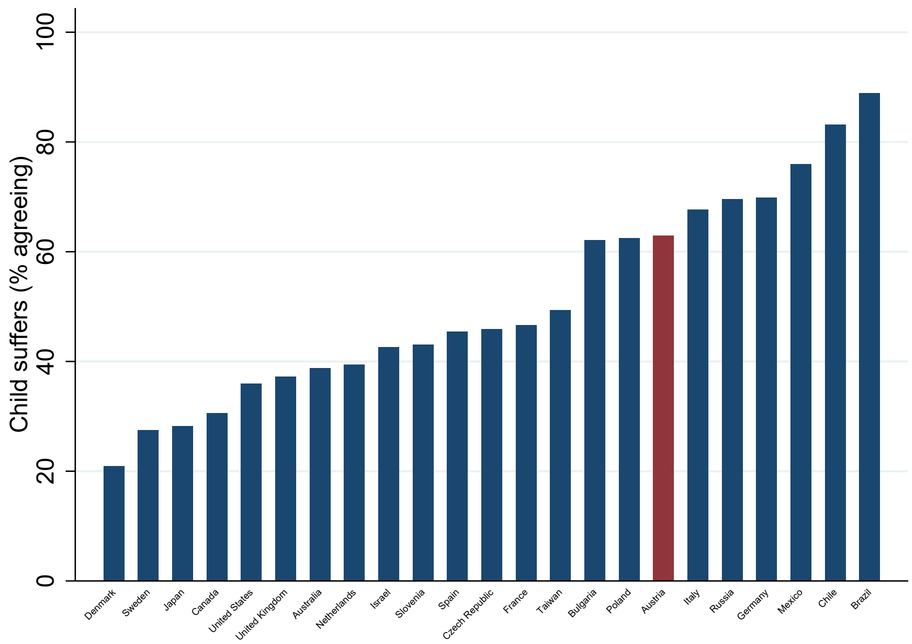
A Supplementary Figures: Context

Figure A.I: Agreement with Statements

(a) "A Woman Should Stay Home when She Has a Child under School Age"



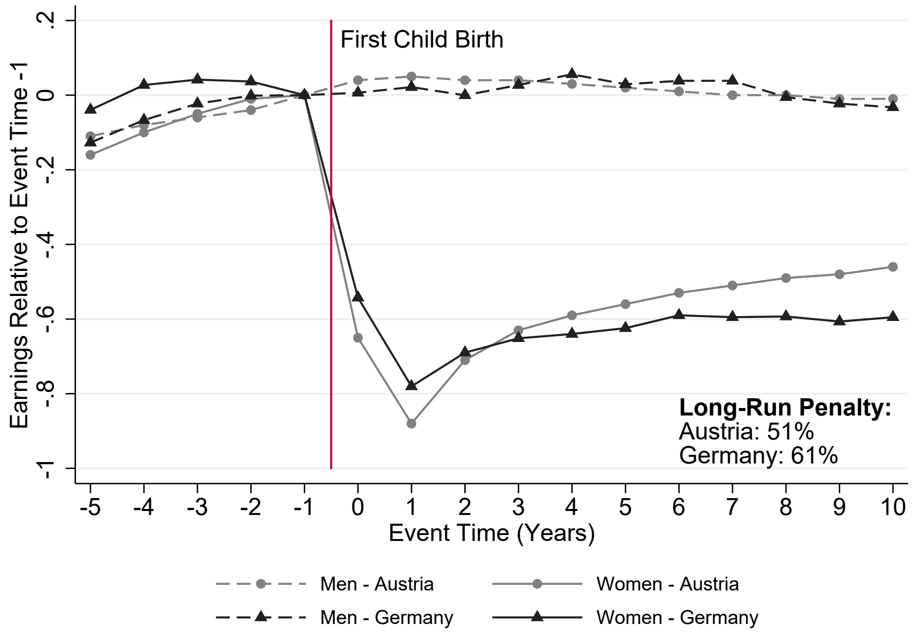
(b) "When a Mother Works for Pay, Her Children Suffer"



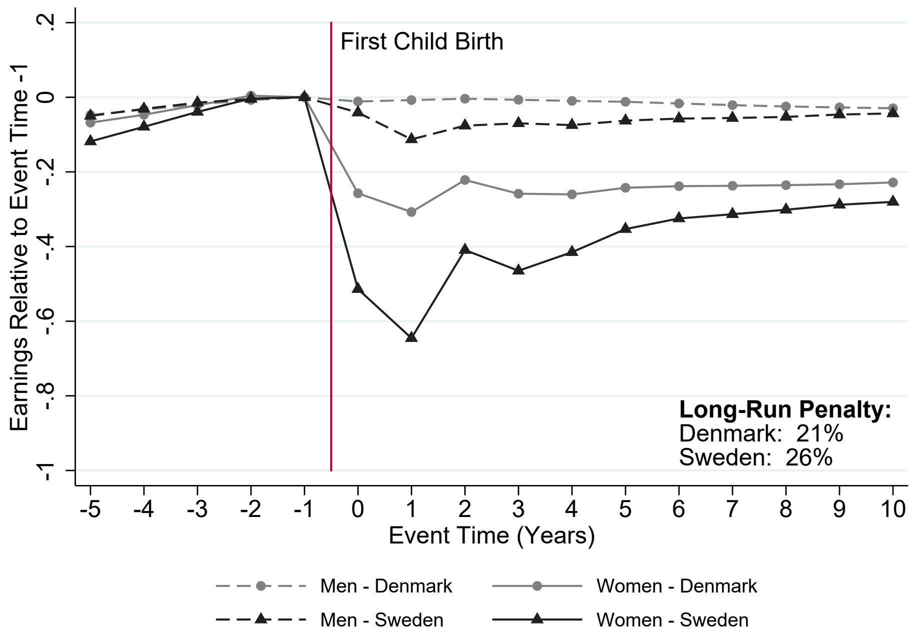
Notes: The figure is based on data from the 2012 wave of the International Social Survey Program (ISSP).

Figure A.II: Child Penalties - Other Countries

(a) German-Speaking Countries



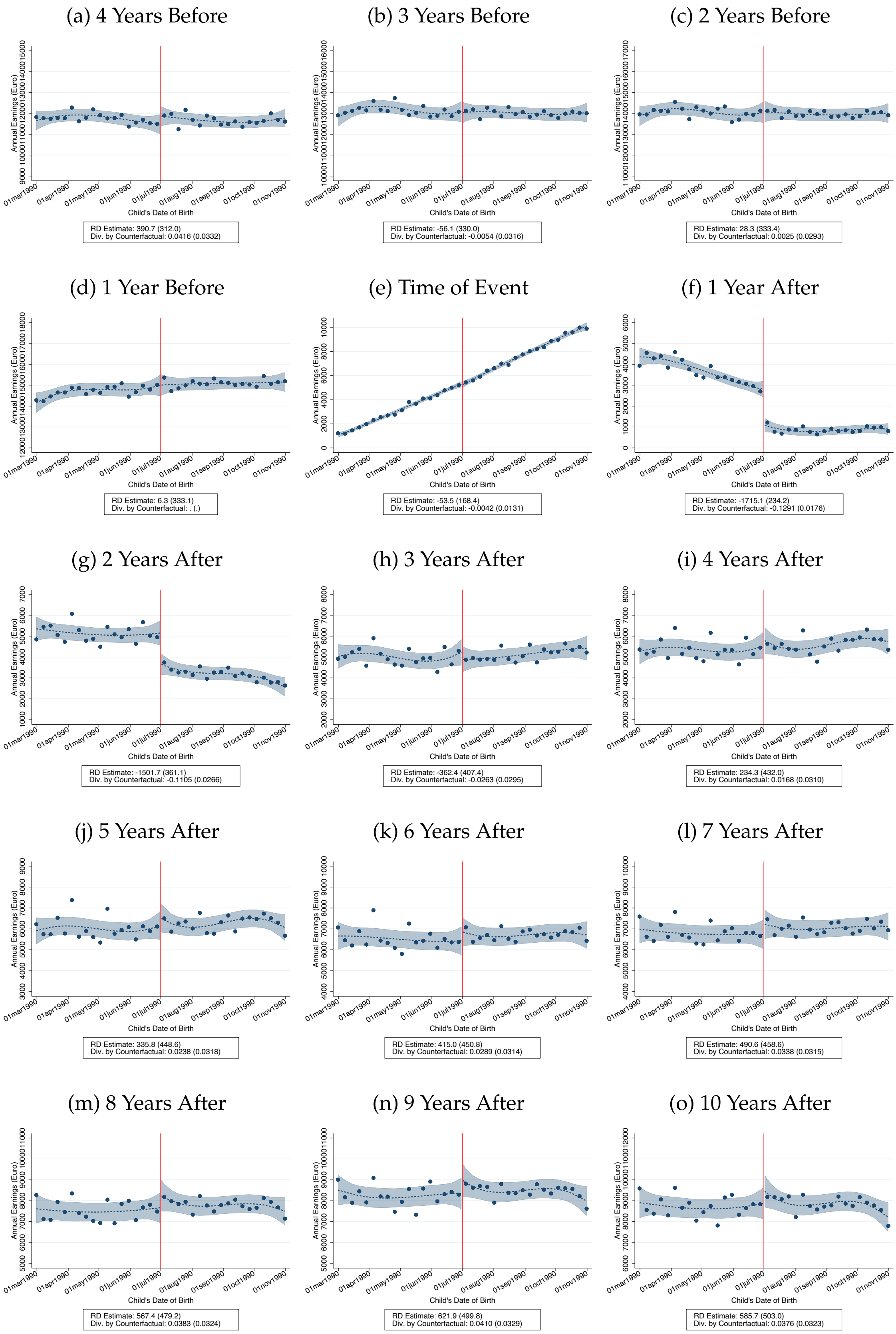
(b) Scandinavian Countries



Notes: Figures reproduced from Kleven, Landais, Posch, Steinhauer and Zweimuller (2019). We plot child penalties P_t^g as defined in equation (2) for each gender g by event time t (years from birth of first child) for a set of countries. Annual earnings are zero if an individual is not working in a given year. Long-run penalties are defined as $P_{10}^m - P_{10}^w$. For details on data sources and methodology see Kleven, Landais, Posch, Steinhauer and Zweimuller (2019).

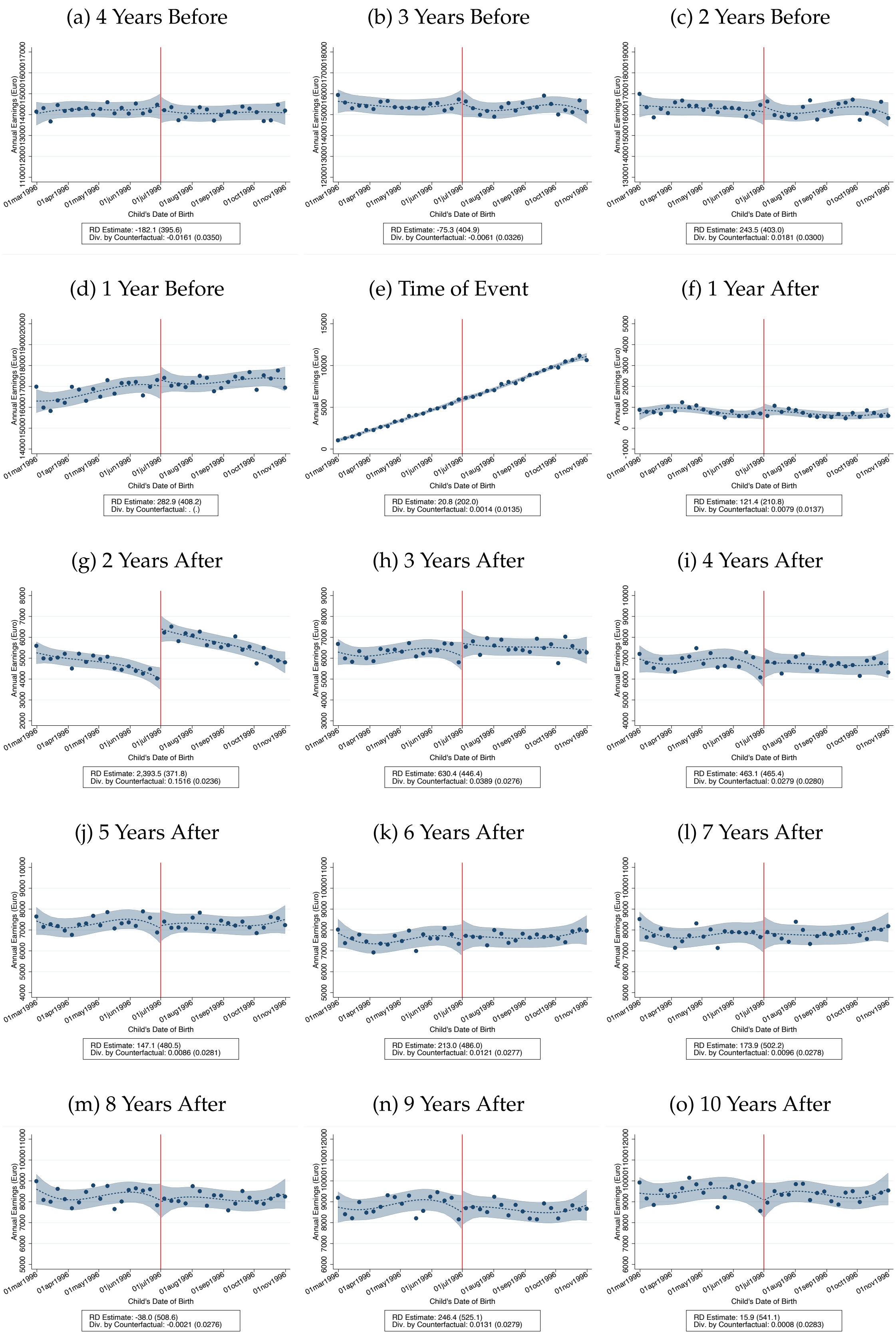
B Supplementary Figures: Parental Leave

Figure B.I: RDD Evidence on Maternal Earnings: 1990 Parental Leave Reform



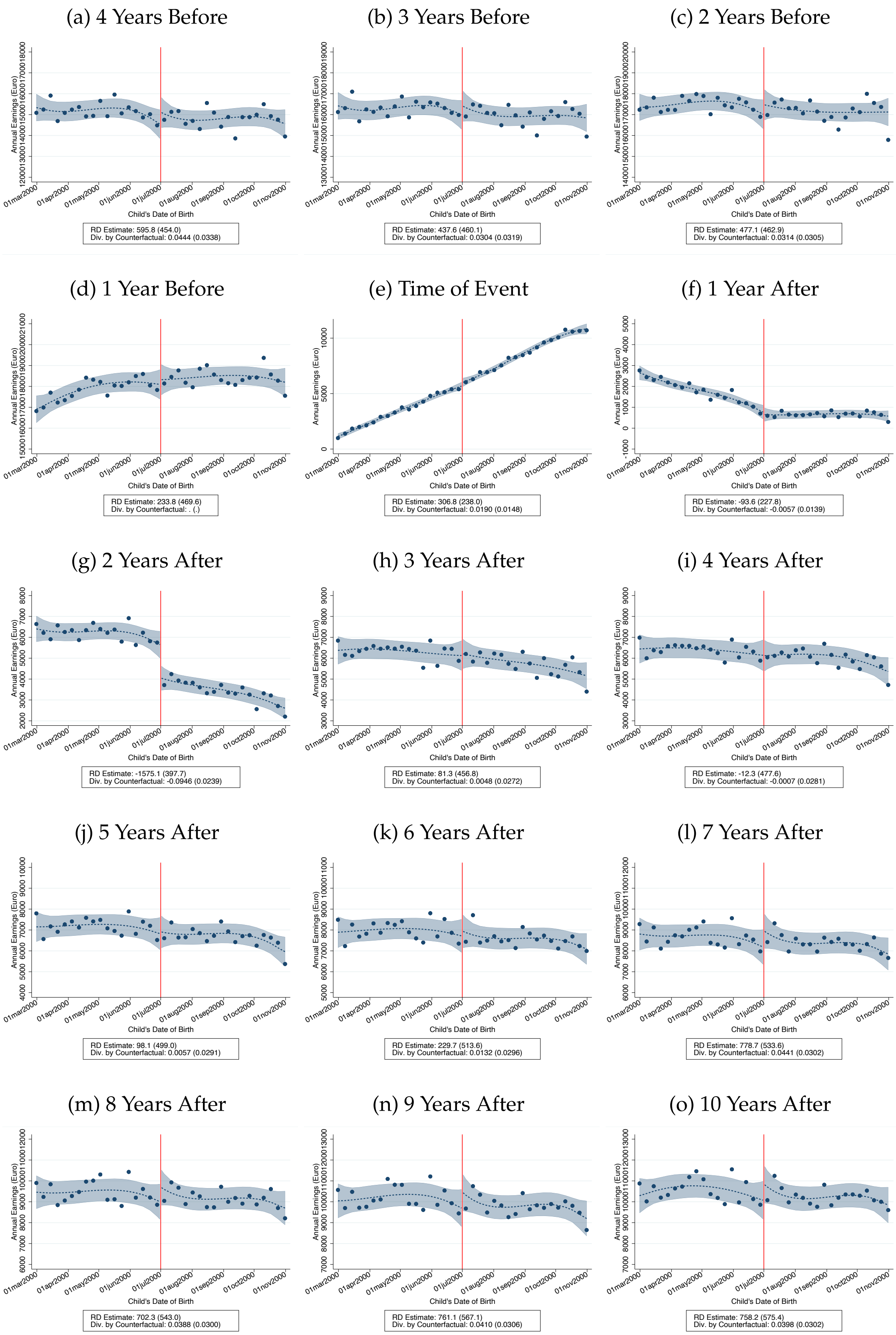
Notes: Figures plot the average annual earnings of mothers by week of first birth for each event time relative to first birth. We adjust earnings by CPI (base year 2000) and correct for topcoding (see Appendix D.1). Earnings are zero if the individual did not work in a particular year. We include first births in a 4-month window around the reform cut-off dates (July 1st 1990, 1996, and 2000). We exclude women ineligible for parental leave (less than 52 weeks of work in the two years preceding birth). For each event time separately we regress annual earnings on age dummies and a cubic in distance to the cut-off, allowing for separate trends on each side. We plot cubic trends from this regression including 95% confidence intervals. We report the discontinuity estimate and standard error as well as the estimate scaled by average counterfactual earnings at the same event time from child penalty regressions estimated on pre-reform samples.

Figure B.II: RDD Evidence on Maternal Earnings: 1996 Parental Leave Reform



Notes: See notes to Figure B.I.

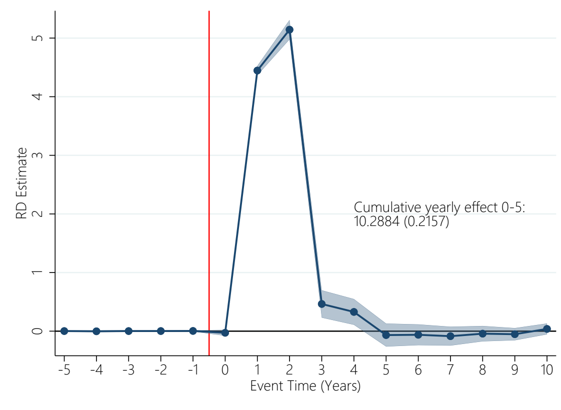
Figure B.III: RDD Evidence on Maternal Earnings: 2000 Parental Leave Reform



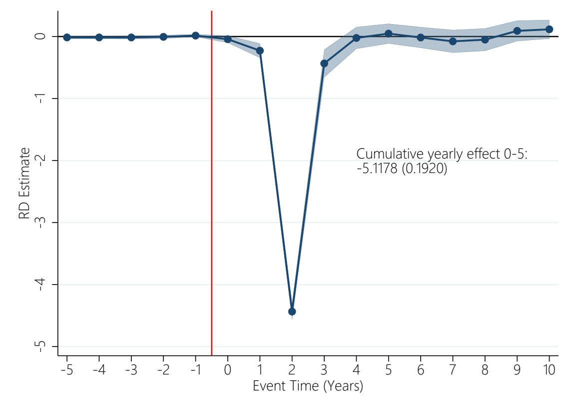
Notes: See notes to Figure B.I.

Figure B.IV: Effect of Parental Leave Reforms on Maternal Take-Up

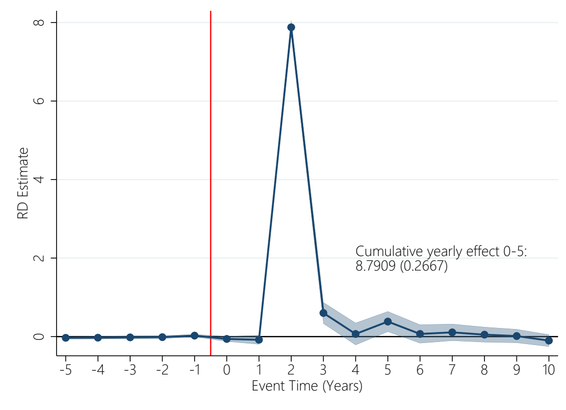
(a) 1990 Reform



(b) 1996 Reform



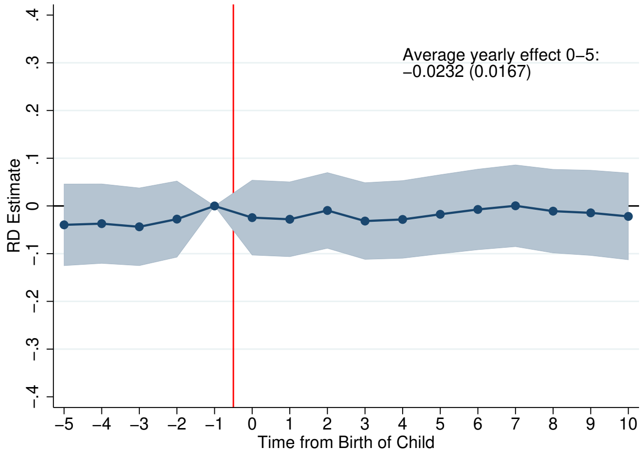
(c) 2000 Reform



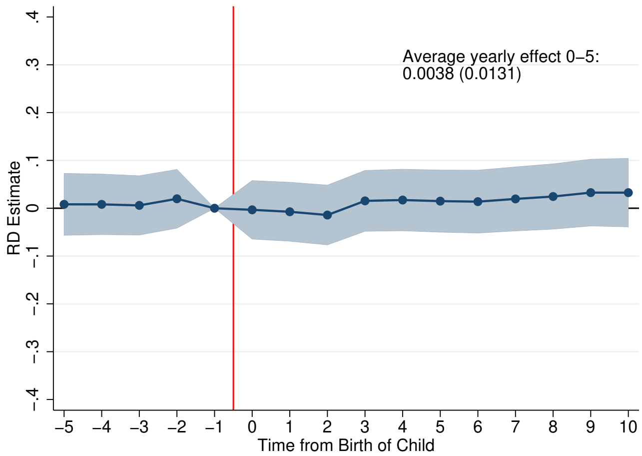
Notes: The figure shows the take-up of parental leave around the reform. The y-axis shows the parental leave duration in months. The sample includes all mothers eligible to parental leave who gave birth to their first child between May and August in the reform year or the year before. Each panel displays the RD estimate for the number of months spent by mothers on parental leave for each event year with respect to the birth of the first child. We also display the cumulated effect over the first five years. This cumulated effect allows us to compute the overall take-up rate of the extra maternal leave granted by each of the three reform. The 1990 reform is associated with a cumulated increase of 10.3 months of maternal leave among treated mothers, out of 12 extra months granted by the reform, which amounts to a take-up rate of 10.3/12=86%. The corresponding take-up rates for the 1996 and 2000 reforms are 5.1/6=85%, and 8.3/12=73% respectively. This evidence confirms that the 2000 reform, starting from a much higher baseline, is associated with a lower take-up rate.

Figure B.V: Effect on Paternal Earnings: 1990, 1996 and 2000 Parental Leave Reforms

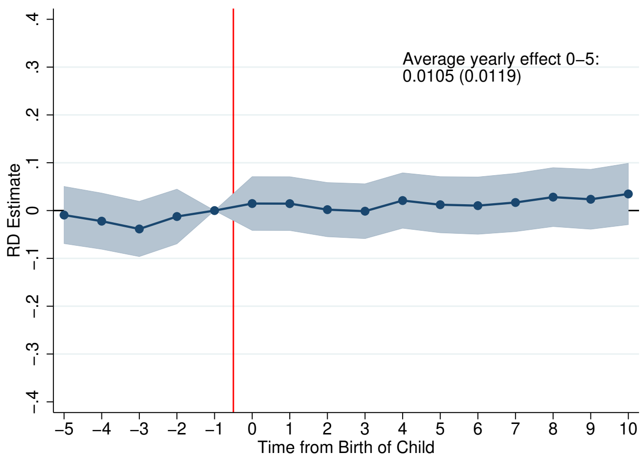
(a) 1990 Reform



(b) 1996 Reform



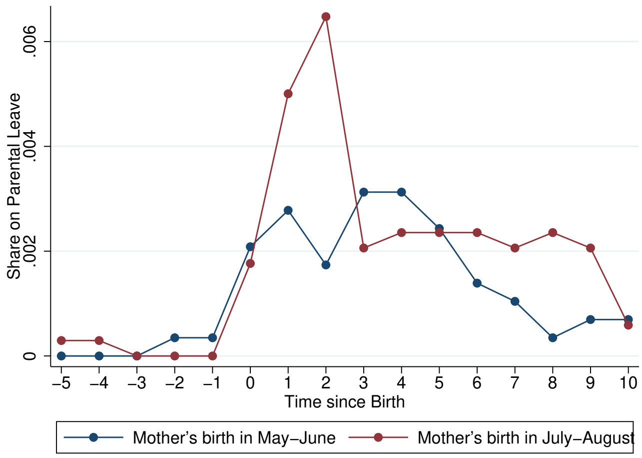
(c) 2000 Reform



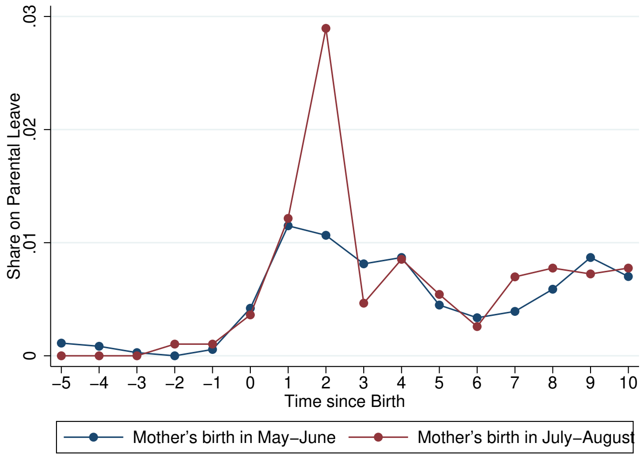
Notes: The figure replicates Figure 3, but on the sample of first time fathers. It displays estimates of the effects of three parental leave reforms on the dynamic earnings of fathers. Fathers are matched to first births based on administrative data on applications for child benefits. See notes to Figure 3 for further details.

Figure B.VI: Take-Up of Paternal Leave Around 1990, 1996 and 2000 Parental Leave Reforms

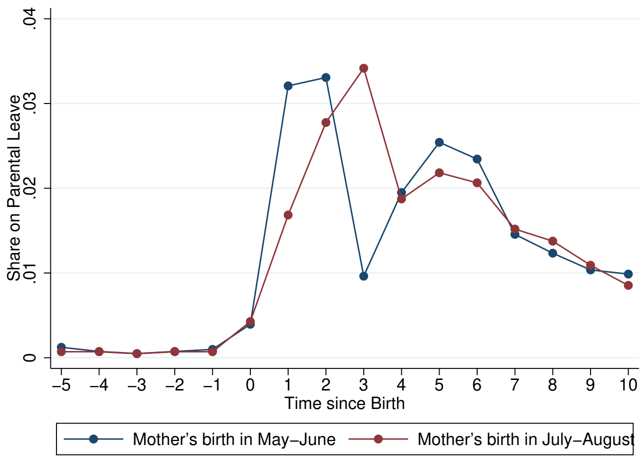
(a) 1990 Reform



(b) 1996 Reform

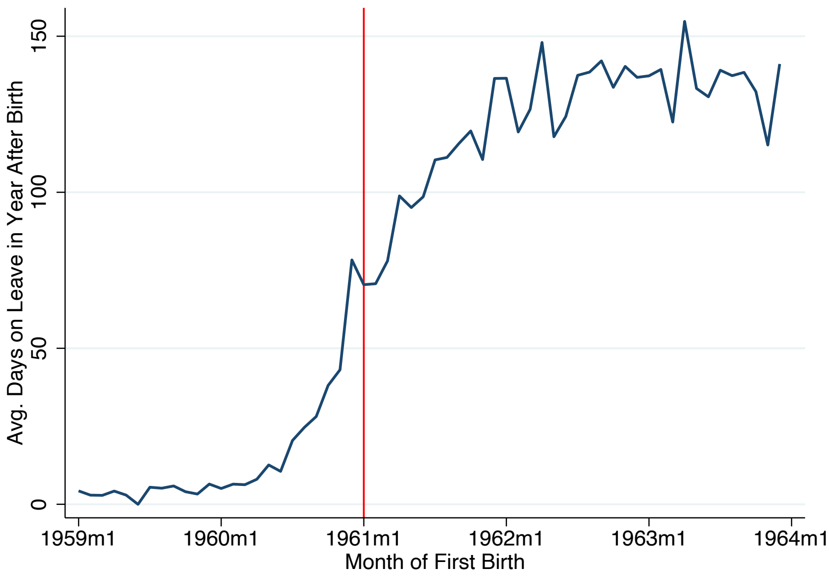


(c) 2000 Reform



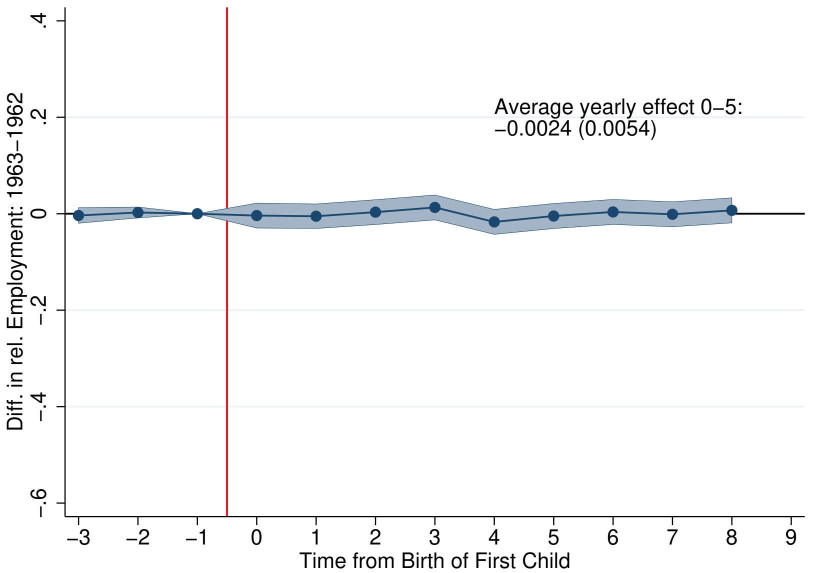
Notes: The figure shows, for the 1990, 1996 and 2000 reforms, the evolution of parental leave take up by fathers as a function of time in years since birth of the first child. In each panel, the blue series represent the fathers of children born in May to June, and not eligible to the new parental leave regime, while the red series depict the fathers of children born in July and August, eligible to the new regime.

Figure B.VII: 1961 Reform: Take-Up



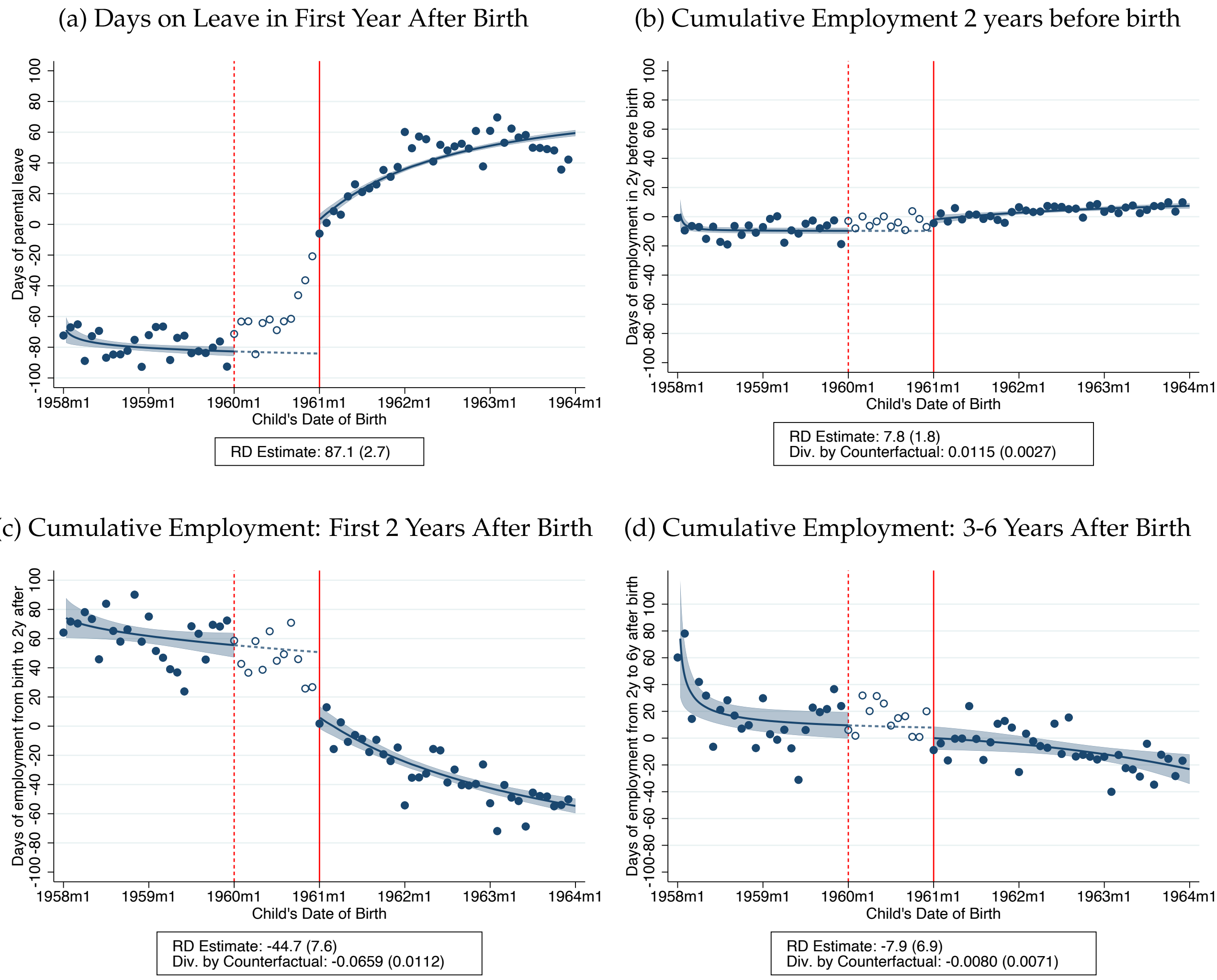
Notes: The figure shows the evolution of maternity leave take up around the 1961 reform which introduced parental leave. We plot average days on parental leave in the year following birth of the first child, by month of first birth (based on REV data). As the reform was grandfathered, women who gave birth in 1960 became eligible, hence the observed gradual increase in take up.

Figure B.VIII: 1961 Reform: Robustness to Trends



Notes: The figure investigates the presence of trends that may confound the estimated effects of the 1961 reform. The graph shows placebo results from specification 4 estimated on a sample of mothers who gave birth in 1962 (T = 0) or 1963 (T = 1). See notes to Figure 4.

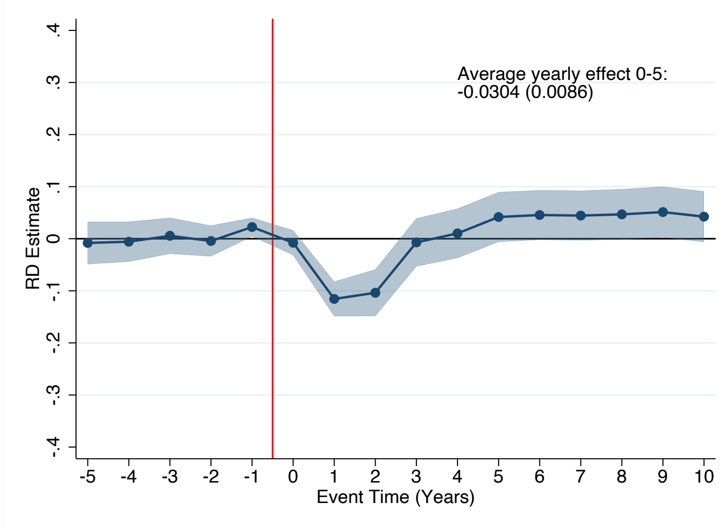
Figure B.IX: Doughnut RD Estimates of the Effects of the 1961 Parental Leave Reform



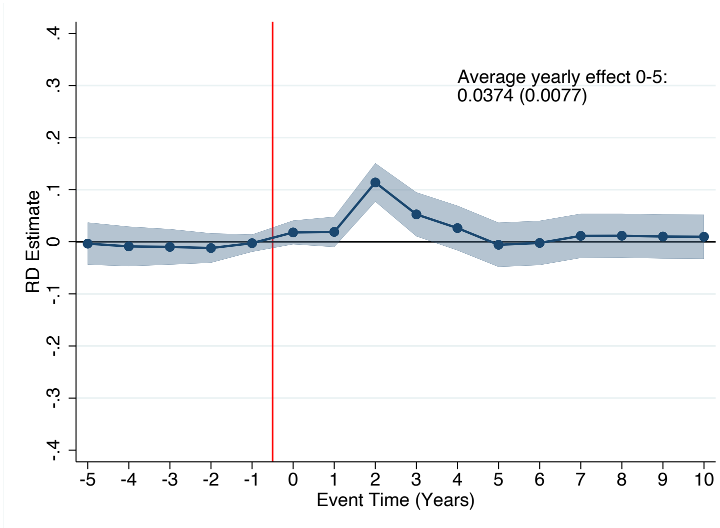
Notes: The figure shows doughnut RD estimates of the effects of the 1961 PL reform. The reform was grandfathered, implying that women who gave birth between January 1960 and December 1960 are partially treated by the reform. We adopt the visualization of Gray et al. (2022): in all panels, we estimate the relationship between date of birth and the outcome using fractional polynomial regressions, separately for (i) the segment of women who gave birth between January 1958 and December 1959 (untreated), (ii) the segment of women who gave birth between January 1960 and December 1960 (partially treated, hollow circles) and (iii) the segment of women who gave birth between January 1961 and December 1963 (treated). The doughnut RD estimate is obtained as the difference between the predicted outcome for the treated estimated at birth date January 1st 1961, and the predicted outcome for the untreated estimated on the same birth date. Panel (a) shows the first-stage, that is the number of days on leave in the first year after child birth. Panel (b) shows pre-child birth employment and establishes the absence of pre-trend. Panel (c) focuses on short-run employment responses in the first two years after birth, while panel (d) looks at longer run employment effects three to six years after birth.

Figure B.X: Earnings Effect of Parental Leave Reforms on Mothers in Top Quartile of Pre-Birth Earnings Distribution

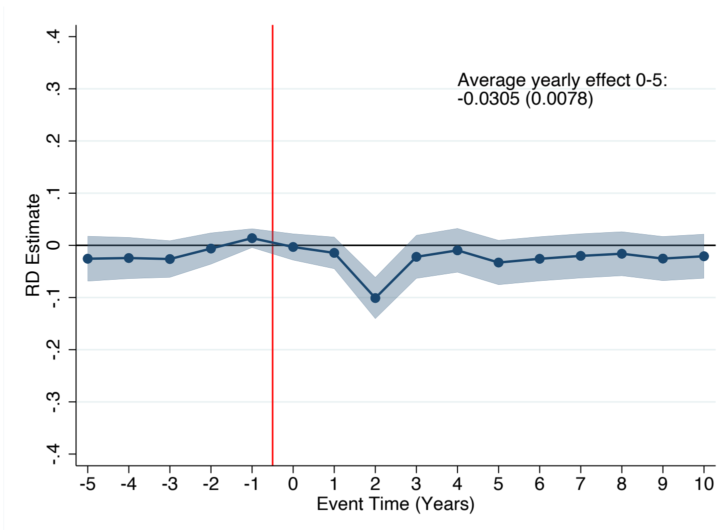
(a) 1990 Reform



(b) 1996 Reform



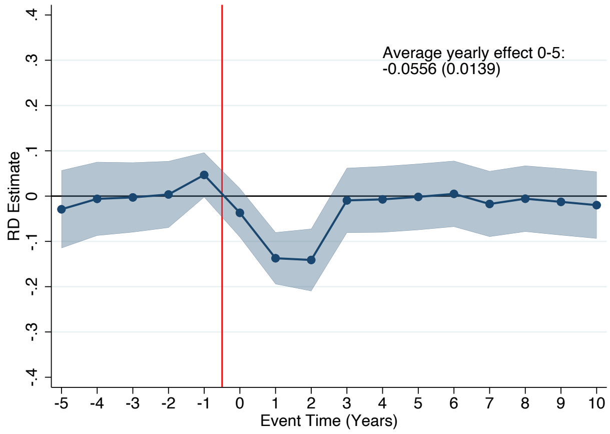
(c) 2000 Reform



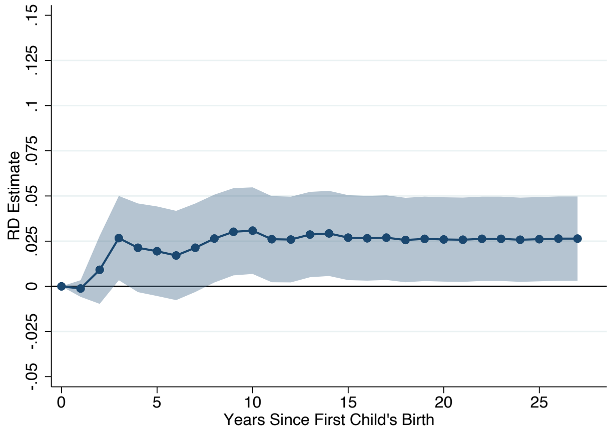
Notes: This figure investigates the presence of heterogeneous responses to parental leave reforms by mothers' earnings level. We reproduce estimates from Figure 3 on women at the top end of the earnings distribution. To this effect, we split our reform samples into four quartiles by annual earnings one year before birth. We then run exactly the same procedure as in Figure 3 keeping only mothers in the top pre-birth earnings quartile.

Figure B.XI: Effects of Parental Leave: Mediating Role of Fertility

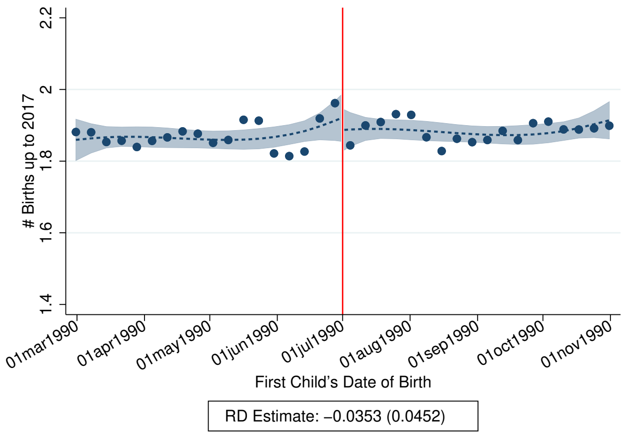
(a) Dynamic Effects of the 1990 Reform - 1 Child Only



(b) 1990 Reform - Probability of Second Birth



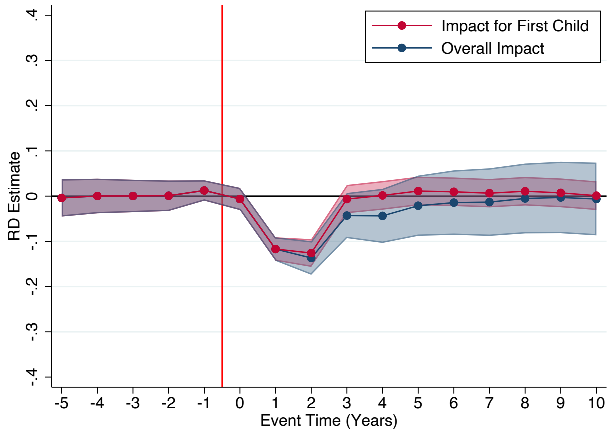
(c) Effect of the 1990 Reform on Completed Fertility



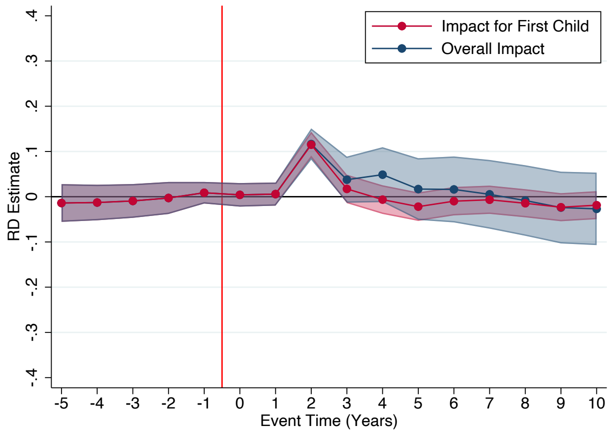
Notes: Panel (a) plots the 1990 reform effect (see notes to Figure 3) for the subsample of mothers who only have one child in the 1989-2017 period. Panel (b) plots the 1990 reform effect of the probability of a second birth by years since the first child's birth. Panel (c) plots average number of births by week of birth of the first child for mothers who gave birth to their first child between March-November 1990. Average births includes all observed births up to 2017. Dashed lines are fitted values based on a cubic polynomial separately fitted at each side of the cut off with 95% confidence interval.

Figure B.XII: Effects of Parental Leave on Child Penalties Acccounting For Subsequent Births

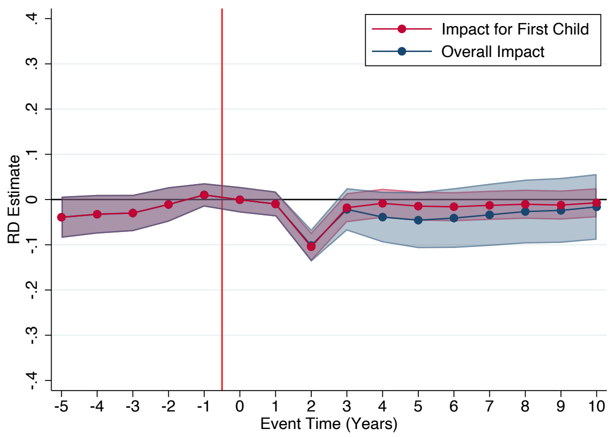
(a) 1990 Reform



(b) 1996 Reform



(c) 2000 Reform

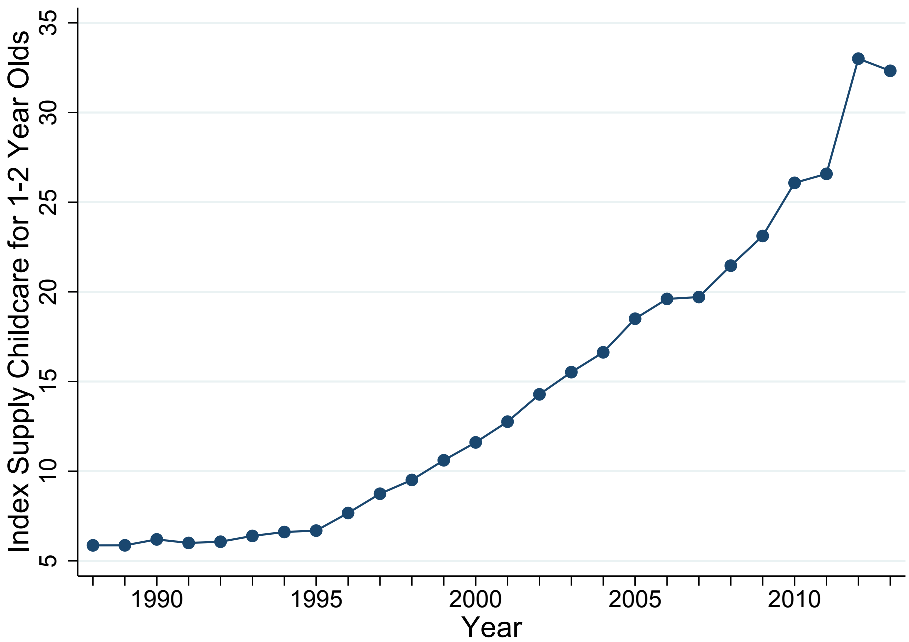


Notes: Our baseline estimates in Figure 3 identify how child penalties respond to the PL regime applicable to the first child. To compute the total effect on child penalties of moving, in steady state, from one PL regime to another, this figure accounts for the effect of PL extensions for births of higher parity. To this effect, we turn to mothers giving birth to their second, third and fourth child, and replicate, for each parity, our dynamic estimates of the impact of PL reforms on earnings. We then construct the total steady state impact of a reform by adding dynamic estimates at all parities, weighted by the average completed fertility, and average timing between parities, of women who had their first birth around the time of each reform.

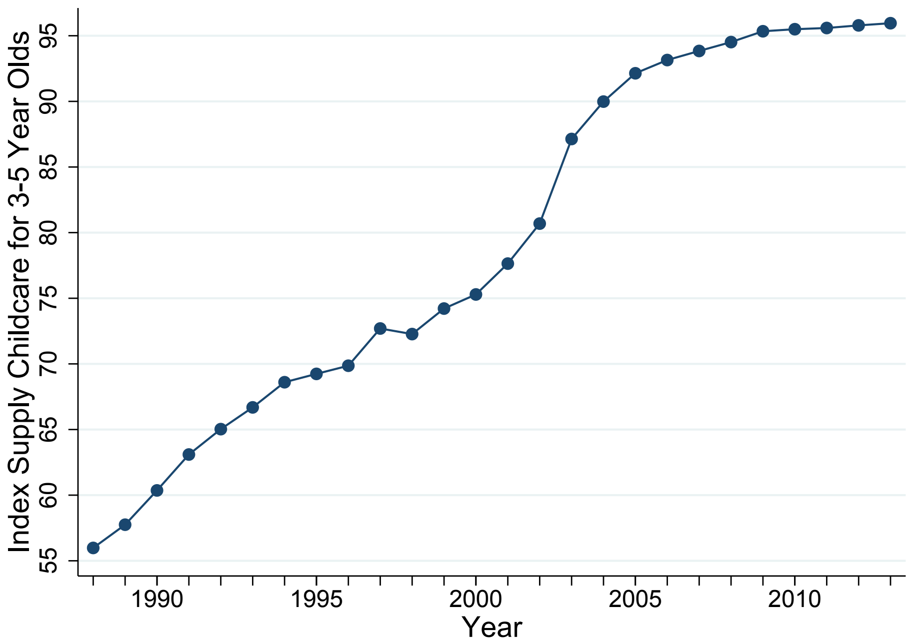
C Supplementary Figures: Child Care Provision

Figure C.I: Index of Child Care Provision over Time

(a) Child Care Index 1-2

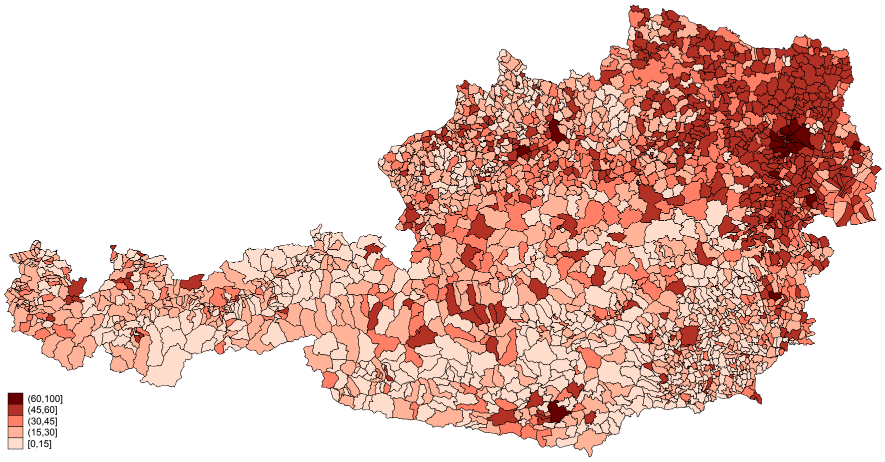


(b) Child Care Index 3-5



Notes: This figure plots our child care indices over time. Each index is calculated at the municipality level based on child care availability and the number of children in the municipality. We then calculate weighted averages by year weighting by the total number of children age 1-2 (panel a) and children age 3-5 (panel b) in the municipality.

Figure C.II: Spatial Variation in Child Care Provision - Index 1-5 - 1990



Notes: This figure plots our child care index 1-5 by municipality using Austrian GIS maps with municipality borders (administrative boundaries). We shade each municipality according to the level of the index. Darker colors indicate a higher index in 1990.

Table C.I: Descriptive Statistics of Municipalities By Treatment Status

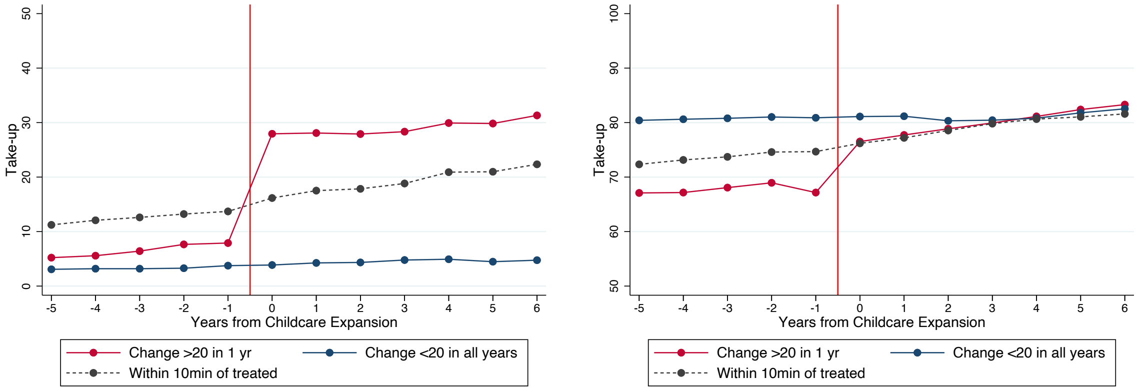
	Index 1-2		Index 3-5		
Treatment Status	Control	Treatment	Control	Treatment	All
Panel A. 1991 Census Characteristics					
Number of Municipalities	1,554	284	253	1,507	1,887
Total Population	4,797,414	1,323,339	2,938,329	3,817,840	7,795,786
% Urban	0.39	2.11	9.49	0.07	1.48
% Towns/Suburbs	15.51	32.04	28.46	15.59	18.76
% Rural	84.11	65.85	62.06	84.34	79.76
Average Population	3,087	4,660	11,614	2,533	4,131
Empl. Rate Men	58.63	58.18	57.91	58.68	58.53
Empl. Rate Women	35.18	35.10	37.54	34.88	35.25
Female Earnings at t=-1	17,317	17,300	17,896	17,228	17,308
Panel B. Child Care in 1991					
% Municip. with Nursery	0.77	4.93	12.65	0.46	2.33
% Municip. with All-Day Pre-School	40.93	40.85	83.79	33.84	41.92
% of Children (1-2) in Nursery	0.00	0.01	0.02	0.00	0.00
% of Children (3-5) in Pre-School	0.57	0.59	0.82	0.53	0.57
Index 1-2	0.16	0.99	2.95	0.08	0.48
Index 3-5	50.54	49.97	85.86	45.25	51.01
Index 1-5	30.39	30.38	52.69	27.18	30.80

Notes: Panel A shows municipality characteristics in our treatment and control municipalities drawn from the 1991 census and earnings information from the ASSD. For earnings, we focus on women just about to give birth (i.e. in event time -1 with respect to the birth of their first child).
Panel B shows average child care availability according to the child care availability dataset provided by Statistics Austria. We calculate the average share with a nursery (children age 1-2), with all-day pre-school (children age 3-5) and the share of children enrolled in each of these types of child care facilities. We also show the averages of our child care indices age 1-2, 3-5 and 1-5.

Figure C.III: Local spillovers: Take-up of Child Care in Neighboring Municipalities

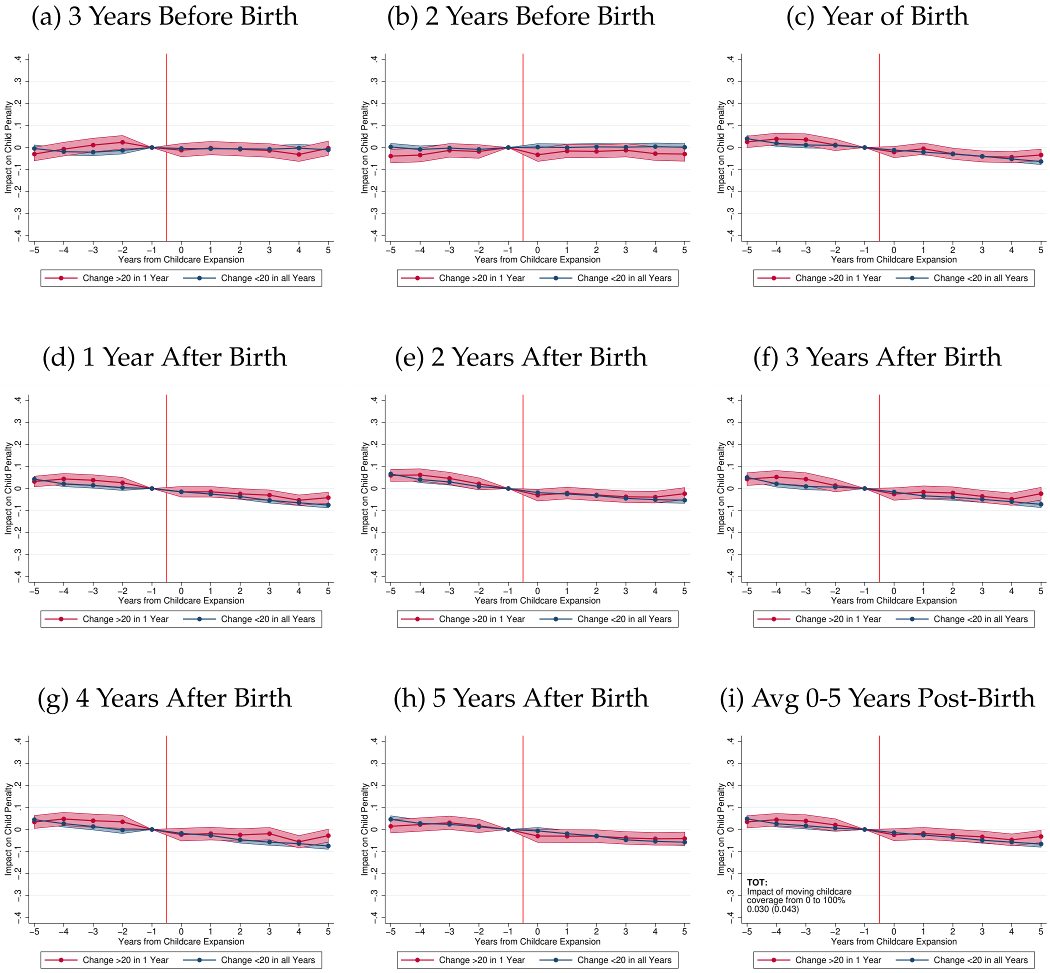
(a) Take-Up of Nursery Care

(b) Take-Up of Pre-School Care



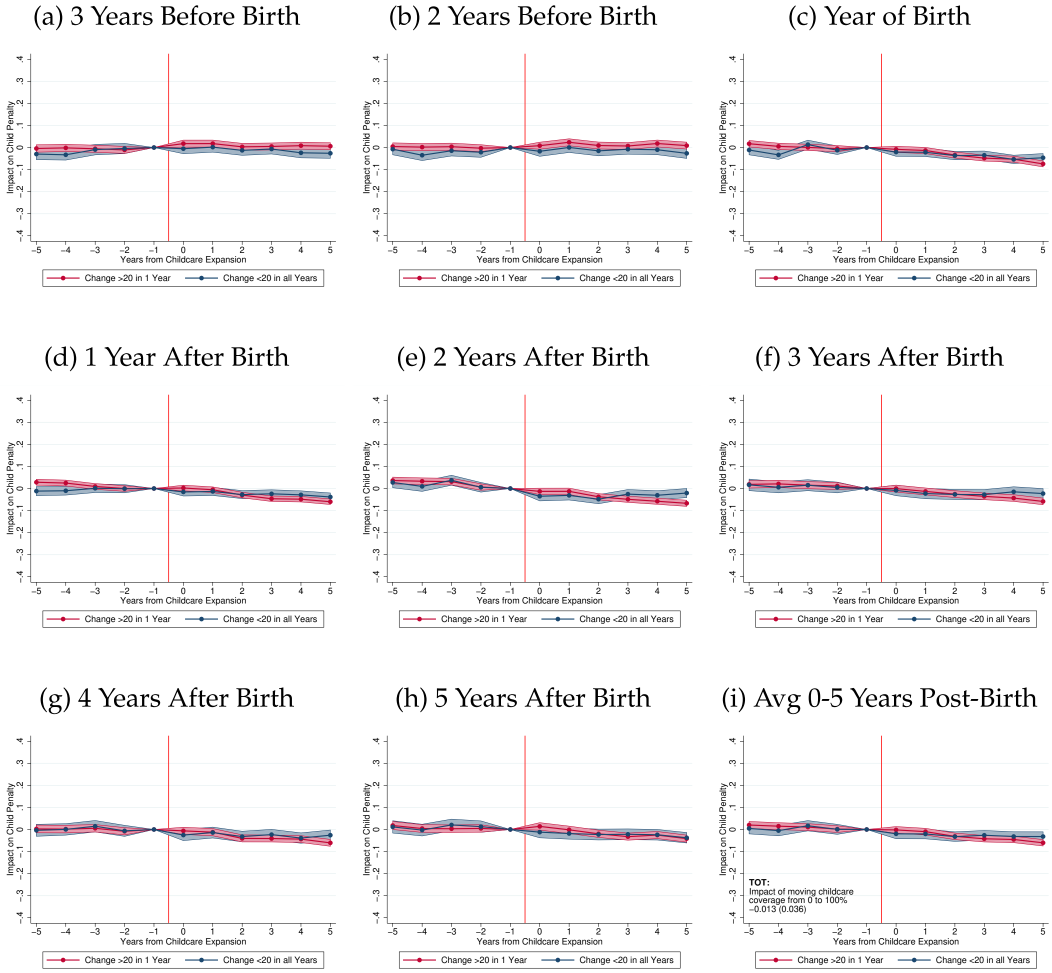
Notes: The figure displays the evolution of nursery care take up (panel a) and pre-school care take up (panel b) in treated municipalities (red) and control municipalities around the time of a local child care expansion in treated municipalities (event year 0). We break down control municipalities between neighboring municipalities, that are less than 10 minutes driving distance from treated municipalities (dashed black line) and other control municipalities, to investigate local spillovers in treatment.

Figure C.IV: Nursery Care



Notes: The figure plots difference-in-differences estimates from specification 6 for the effects of nursery care expansions on the full dynamics of female earnings. Each panel corresponds to different event time with respect to the birth of the first child. In panel (a) we plot the evolution of earnings at event time t=-3, that is for women who are three years before giving birth to their first child, in treated vs control municipalities, around the event of a nursery care expansion. In panel (b), we do the same for women at at event time t=-2. Etc.

Figure C.V: Pre-School Care

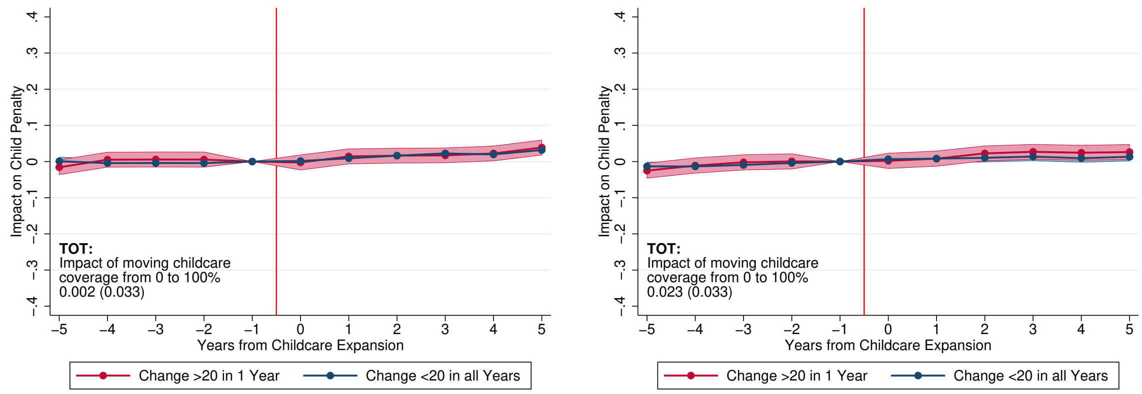


Notes: The figure plots difference-in-differences estimates from specification 6 for the effects of pre-school care expansions on the full dynamics of female earnings. Each panel corresponds to different event time with respect to the birth of the first child. In panel (a) we plot the evolution of earnings at event time t=-3, that is for women who are three years before giving birth to their first child, in treated vs control municipalities, around the event of a pre-school care expansion. In panel (b), we do the same for women at at event time t=-2. Etc.

Figure C.VI: Estimated Effects of Child Care Expansions on Employment Penalty of Mothers

(a) Effect of Nursery Care Expansion on Female Employment in $t = 1 - 2$

(b) Effect of Pre-School Care Expansion on on Female Employment in $t = 3 - 5$

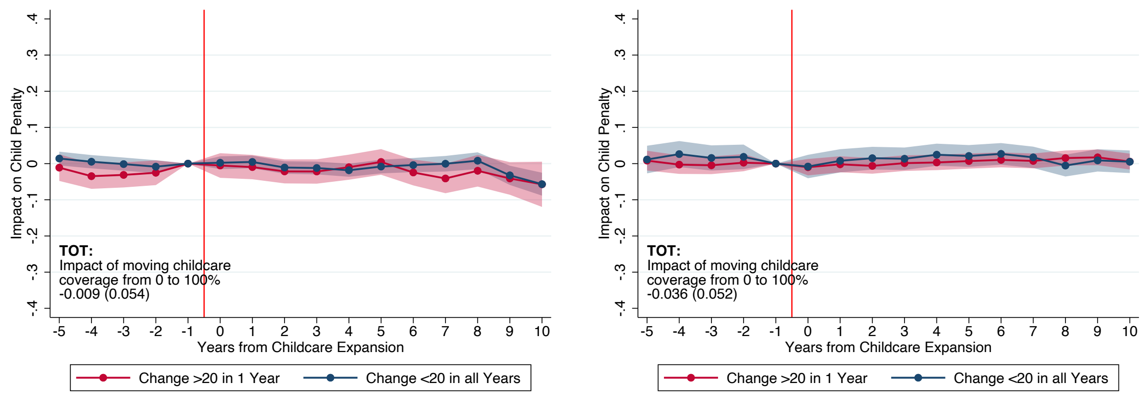


Notes: The figure replicates the analysis of the effects of for nursery care and pre-school care expansions events on the employment penalty of mothers. It shows the DiD estimates of the effects of nursery care (panel (a)) and pre-school care (panel (b)) expansions on the dynamics of female employment. In panel (a) (resp. panel (b)), we plot the evolution of employment at event time $t \in [1,2])$ (resp. $t \in [3,5]$), that is for mothers who are between one and two years (resp. three to five years) after the birth to their first child, in treated vs control municipalities, around the event of a child care expansion. We also report the TOT effect, which corresponds to the DiD estimate $\hat{\alpha}_{st}^{T}$ for earnings scaled by the first-stage, i.e. the equivalent DiD estimate for the change in the child care index.

Figure C.VII: Estimated Effects of Child Care Expansions on the Earnings of Fathers

(a) Effect of Nursery Care Expansion on Earnings in $t = 1 - 2$

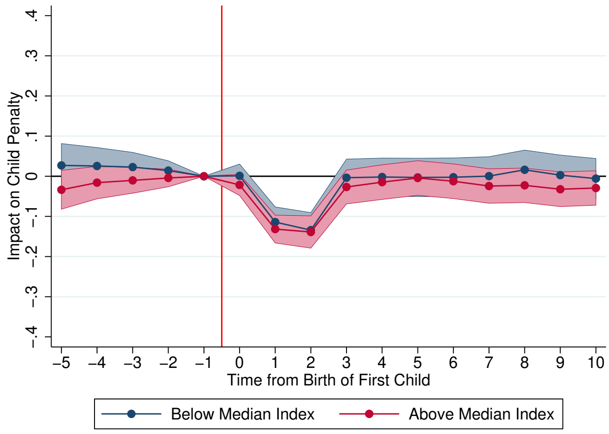
(b) Effect of Pre-School Care Expansion on Earnings in $t = 3 - 5$



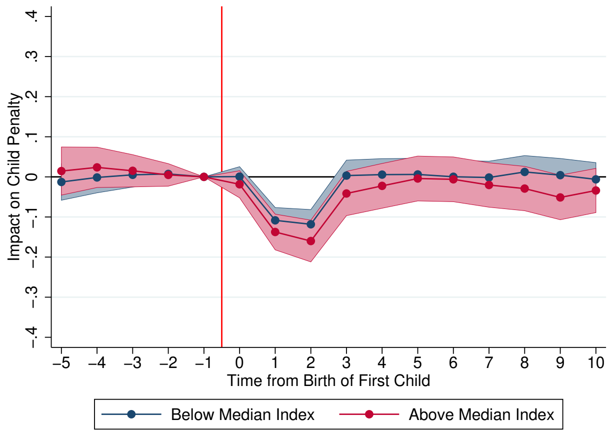
Notes: The figure replicates the analysis of the effects of for nursery care and pre-school care expansions events on the earnings of fathers. It shows the DiD estimates of the effects of nursery care (panel (a)) and pre-school care (panel (b)) expansions on the dynamics of male earnings. In panel (a) (resp. panel (b)), we plot the evolution of earnings at event time $t \in [1,2])$ (resp. $t \in [3,5]$), that is for men who are between one and two years (resp. three to five years) after the birth to their first child, in treated vs control municipalities, around the event of a child care expansion. We also report the TOT effect, which corresponds to the DiD estimate $\hat{\alpha}_{st}^{T}$ for earnings scaled by the first-stage, i.e. the equivalent DiD estimate for the change in child care index.

Figure C.VIII: Effects of 1990 Parental Leave Reform by Level of Child Care Provision

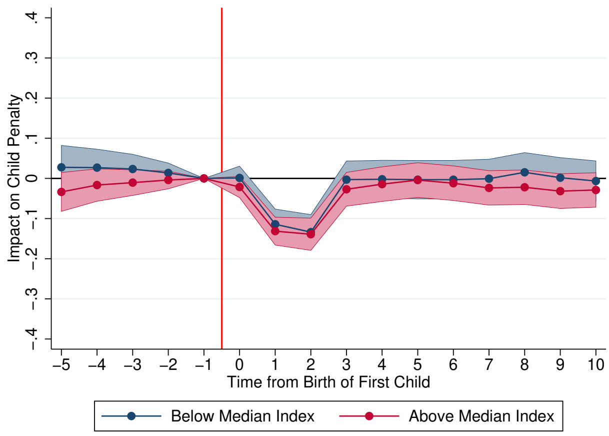
(a) Index 1-5



(b) Index 1-2



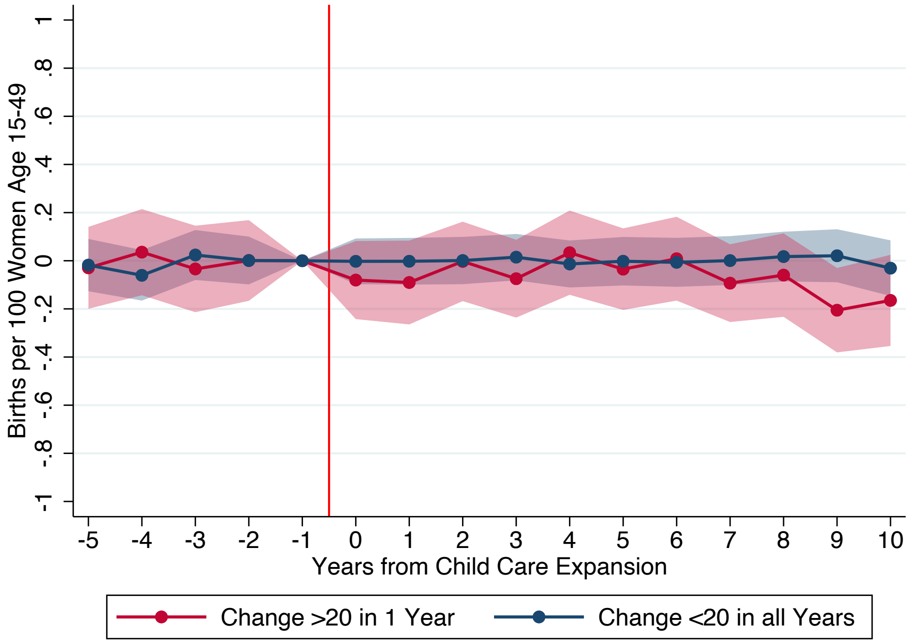
(c) Index 3-5



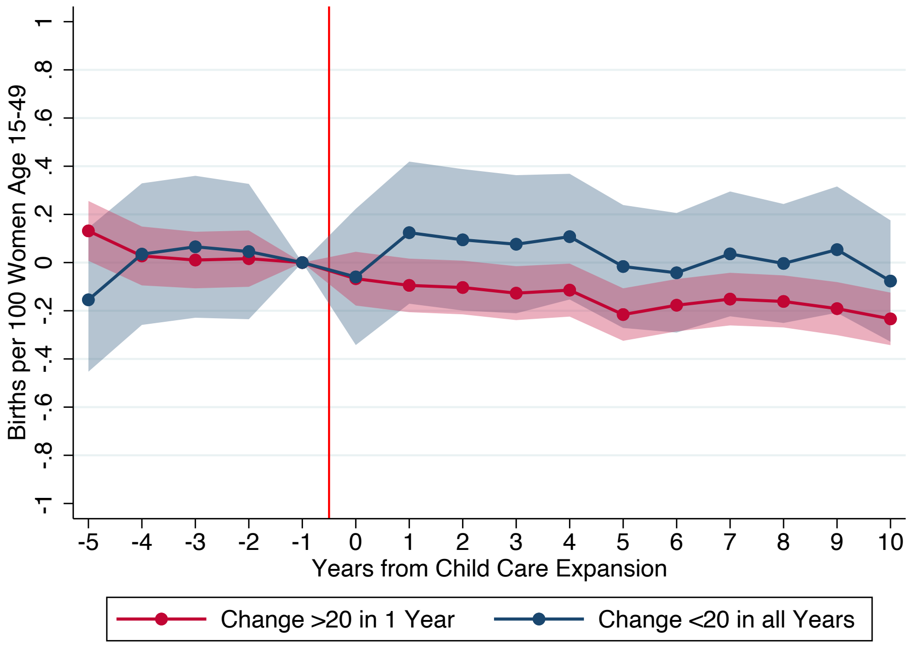
Notes: The figure plots our reform impact estimates by whether the municipality of residence at the time of birth was below or above the median child care index at the time of birth. The reform impact estimates use IPW to weight mothers according to their pre-birth characteristics. Specifically, we run a probit regression of the treatment variable (above median index) on logs of annual earnings in years 3-5 before birth (interacted with year of first birth), employment status in the year before birth and age dummies. We then construct adjusted inverse probability weights based on predicted treatment probabilities and perform the same regressions as in Figure 3.

Figure C.IX: Fertility Effects of Local Child Care Expansions

(a) Nursery Care Expansions



(b) Pre-School Care Expansions



Notes: The figure plots event study estimates of a municipality-level regression of the fertility rate on event time relative to child care expansion interacted with treatment dummy and calendar year dummies. The fertility rate is measured as the number of births in a municipality from the birth register divided by the number of women age 15-49 residing in that municipality in the same year. We obtained the number of women age 15-49 by municipality and year from Statistik Austria register data. Shaded areas are 95% confidence intervals.

D Decomposing Gender Gaps: Data & Methodology

D.1 Measuring Gender Gaps in Earnings Over the Long Run

D.1.1 Using ASSD Data: 1972-2017

Our main data come from the Austrian Social Security Database. This is a matched employer-employee data set which contains detailed information on employment spells and earnings by calendar year over the period 1972 to 2017. The ASSD register covers the universe of paid employment spells, with the notable exclusion of civil servants, self-employed and farmers. This means that about 85% of the Austrian population is covered by ASSD, and this coverage rate has been stable over time. Our analysis therefore focuses on the long run evolution of the earnings gap on the ASSD population. [45]

Earnings in the ASSD (and REV) are topcoded at the maximum contribution base, which changes from year to year. The fraction of topcoded individuals is quite stable over the period 1972-2017: around 8% of men and 2% of women have topcoded earnings. During the years 1994-2012 we can check how much topcoding affects earnings gaps by comparing the earnings gaps in the ASSD and in the income tax data (for workers in both datasets). In Figure D.I, panel (c) we plot the earnings gaps calculated from both sources for workers age 15-64, restricting both samples to individual with positive earnings only. Topcoding has mainly a level effect. The ASSD gap is about 12 percentage points below the income tax gap.

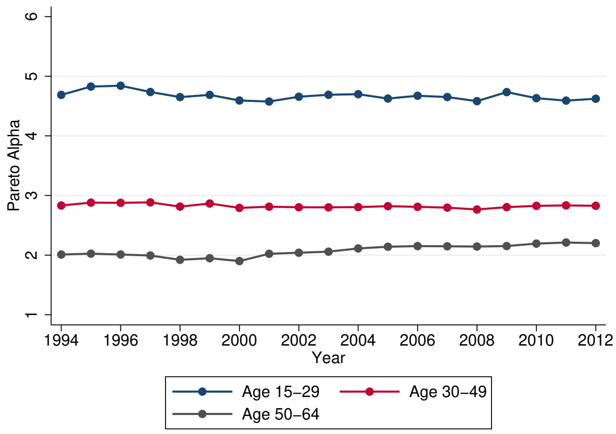
To adjust for topcoding in the ASSD data, we impute earnings above the top coding thresholds, using Pareto estimates of the earnings distribution computed using the income tax data. To be precise, we fit, in the tax data, separate Pareto distributions of earnings by gender and age groups. For each age group a and gender g, we compute Pareto coefficients α_a^g. In practice, we use three age groups: 15-29 years old, 30-49 years old, and 50-64 years old. Figure D.I panel (a) reports the Pareto coefficient of these distributions for each year for men, while panel (b) does the same for women. We see that these Pareto coefficients are very stable over time. In panel (c), we impute earnings above the threshold using these estimated Pareto parameters and compute gender gaps adjusted for top coding. The grey dots report estimated gender gaps when we use the yearly Pareto coefficients for each group, while the yellow series shows the gender gaps when we use the average Pareto coefficients over the period 1994-2012. We see that the ASSD series adjusted for top coding match very closely the gender gaps estimated in

[45] We note that our estimated gender gaps likely understates the earnings gap in the entire Austrian population because men are more often working in the non-ASSD sectors (self employed, civil servants,...) than women and workers in those sectors earn slightly higher wages on average.

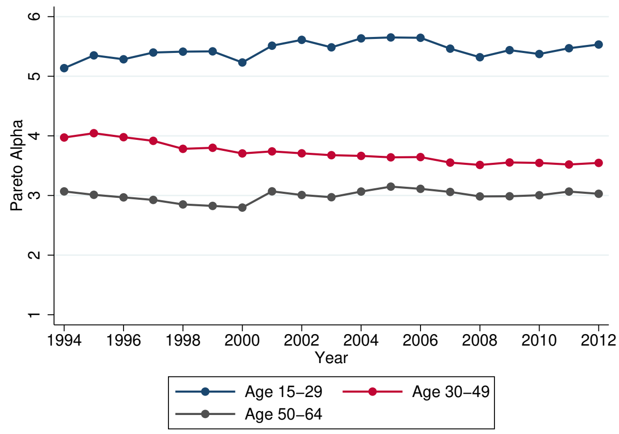
the non-top coded tax data, and that using yearly or average Pareto coefficients does not make a significant difference.

Figure D.I: Correcting for Top Coding of Earnings in the ASSD Data

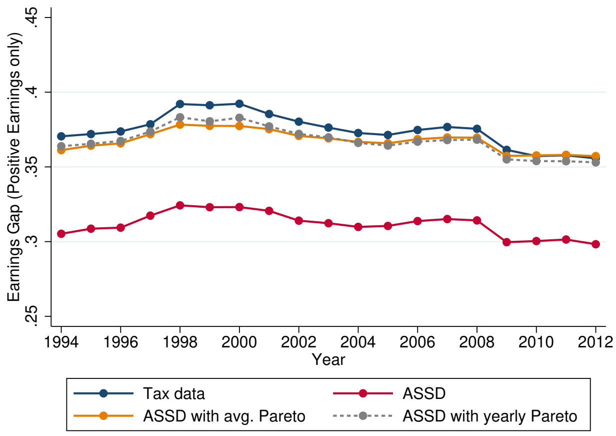
(a) Pareto Coefficients - Men



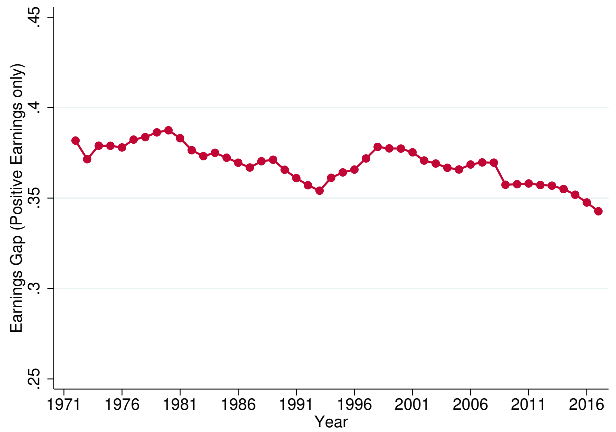
(b) Pareto Coefficients - Women



(c) Gender Gap in Tax vs ASSD Data



(d) ASSD With Correction for Top Coding



Notes: Panels (a) and (b) plot our estimated Pareto coefficients for men and women by age. This is based on our sample of income tax records (1994-2012) which are not topcoded. We fit a pareto distribution by gender and age groups for gross labour income above the social security limit. The limit is adjusted each year and our procedure only fits the Pareto distribution for income above the relevant cut off in any given year. Panel (c) plots gender earnings gaps calculated from the income tax data, ASSD, ASSD adjusting topcoded earnings based on the average Pareto parameter and ASSD adjusting topcoded earnings based on the relevant Pareto parameter by year. Panel (d) plots the ASSD gender gap in annual earnings (excluding zeroes) based on the ASSD earnings adjusted for topcoding using the 1994-2012 average Pareto parameter.

Our sample includes any individual who has at least one record in the ASSD at any point in time. One issue is that we may miss some individuals who never enter the labor force. In practice, we can compare the fraction of individuals with zero earnings in our sample and in the census data for years when the census is available. This comparison shows that our sample slighlty underepresents women with zero earnings compared to the census. We therefore added women with zero earnings to our sample to match the share of non-employed in the census.

D.1.2 Measuring Gender Gaps Before 1972

Before 1972, ASSD data exist but has not been digitized. However earnings statistics (*Lohnstufenstatistik*), including detailed tabulations by gender, drawn from the original data, were regularly published, enabling us to consistently compute the evolution of gender inequality on the population covered by the ASSD since 1953.[46] We used tabulations of the earnings distributions for women and men in 1953, 1957, 1964, 1967, 1970, 1973, 1976, and 1979 that report average monthly earnings for women and men (in July), deciles of the earnings distribution, and the share of women among all ASSD workers. Importantly, to calculate the mean and deciles of all distributions, the IHS imputed topcoded earnings based on the assumption of a lognormal distribution. To account for individuals not in the labor force, we also added men and women with zero earnings so that the employment rates of men and women before 1972 matches the census.

D.2 Decomposition of Gender Gap Between Child Related and Residual Inequality

D.2.1 Methodology

Our methodology for decomposing the gender gap follows Kleven, Landais and Søgaard (2019). It relies on estimating the effect of children (child penalties) on women and men separetely. In the spirit of a Oaxaca-Blinder decomposition, we then ask how much of the average gender gap can differences in these estimates explain. We refer to Kleven, Landais and Søgaard (2019) for further details.

We define the average gender gap in year s as $\Delta_s \equiv 1 - E[Y^w_{ist}|s] / \mathsf{E}\left[Y^m_{ist}|s\right]$. Based on our empirical evidence, and to simplify notation and exposition, we assume in what follows that there are no effect of children on the earnings dynamics of men. We are interested in the average counterfactual gender gap in year s absent child penalties for women, that is: $\tilde{\Delta}_s \equiv 1 - E[\tilde{Y}^w_{ist}|s] / \mathsf{E}\left[Y^m_{ist}|s\right]$. This gap captures the level of gender inequality that would happen absent child penalties on women's careers. In other words, $\tilde{\Delta}_s$ represents the residual inequality across men and women in the labor market that is not related to

[46]IHS Report ("*Geschlechtsspezifische Einkommensunterschiede: Österreich 1953-1979*") by Christl and Wagner (IHS Vienna 1981)

the arrival of children. The child related gender gap is therefore captured by $\Delta_s - \tilde{\Delta}_s \equiv E\left[\tilde{Y}^w_{ist} - Y^w_{ist}|s,t\right] / \mathsf{E}\left[Y^m_{ist}|s\right]$

In practice, we reconstruct the counterfactual earnings gap in year s $\tilde{\Delta}_s$ based on four inputs:

1. the observed average level of earnings of women in year s, $E[Y^w_{ist}|s]$
2. the fraction ψ_{st} of women who are in event time t with respect to the birth of their first child birth,
3. the child penalty for each event time P_{st},
4. and the average level of counterfactual earnings at each event time $E[\tilde{Y}^w_{ist}|s,t]$.

To understand the relationship between the counterfactual earnings gap and these four inputs, note that the computation of $\tilde{\Delta}_s$ solely relies on having an estimate of $E[\tilde{Y}^w_{ist}|s]$. We now show that we can estimate $E[\tilde{Y}^w_{ist}|s]$ based on the four inputs mentioned above. For this, we start by noting that the average level of earnings of women in year s $E[Y^w_{ist}|s]$ is the weighted average of the earnings of women who will never have children (childless women) and of the earnings of women who have, or will have, children and are, in year s, at event time t with respect to the birth of their first child.

$$E[Y^w_{ist}|s] = \psi^{Childless}_s \cdot \mathsf{E}\left[Y^w_{ist}|s, childless\right] + \sum_t \psi_{st} \cdot \mathsf{E}\left[Y^w_{ist}|s,t\right]$$

where we have that $\psi^{Childless}_s + \sum_t \psi_{st} = 1$. Similarly, we can write the average counterfactual level of earnings of women in year s $E[\tilde{Y}^w_{ist}|s]$ as the weighted average of the earnings of women who will never have children (childless women) and of the counterfactual earnings $\tilde{Y}^w_{ist}$ of women who have, or will have, children and are, in year s, at event time t with respect to the birth of their first child.

$$E[\tilde{Y}^w_{ist}|s] = \psi^{Childless}_s \cdot \mathsf{E}\left[Y^w_{ist}|s, childless\right] + \sum_t \psi_{st} \cdot \mathsf{E}\left[\tilde{Y}^w_{ist}|s,t\right]$$

From this, and the definition of child penalties in equation (2), it follows that:

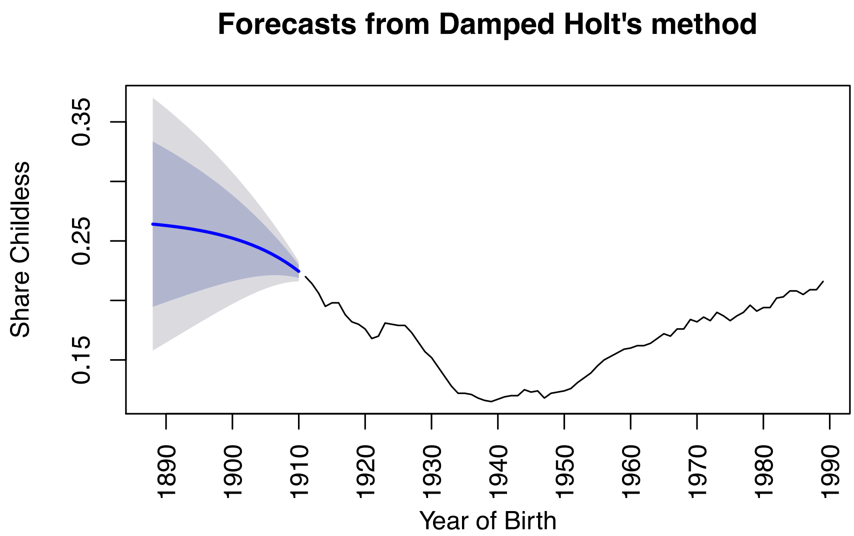
$$\begin{aligned} E[\tilde{Y}^w_{ist}|s] &= E[Y^w_{ist}|s] + \sum_t \psi_{st} \cdot \mathsf{E}\left[\tilde{Y}^w_{ist} - Y^w_{ist}|s,t\right] \\ &= E[Y^w_{ist}|s] - \sum_t \psi_{st} \cdot \mathsf{E}\left[P_{st} \cdot \tilde{Y}^w_{ist}|s,t\right] \end{aligned}$$

We show in the following sections how we computed or estimated each of these four inputs.

D.2.2 Estimating Fraction ψ_{st} of Women at Each Event Time

Fraction of Childless Women We estimated the cohort-specific share of childless women from the Human Fertility Database and the Austrian Birth Barometer. Both data sets contain data and estimates by the Vienna Institute of Demography. For older cohorts, we used a simple linear exponential smoothing predictor (Holt's method) to predict childlessness back to cohorts born in 1887 (age 64 in 1951). Figure D.II below shows the evolution of the fraction of childless women, i.e. women whose completed fertility is zero, by birth cohort.

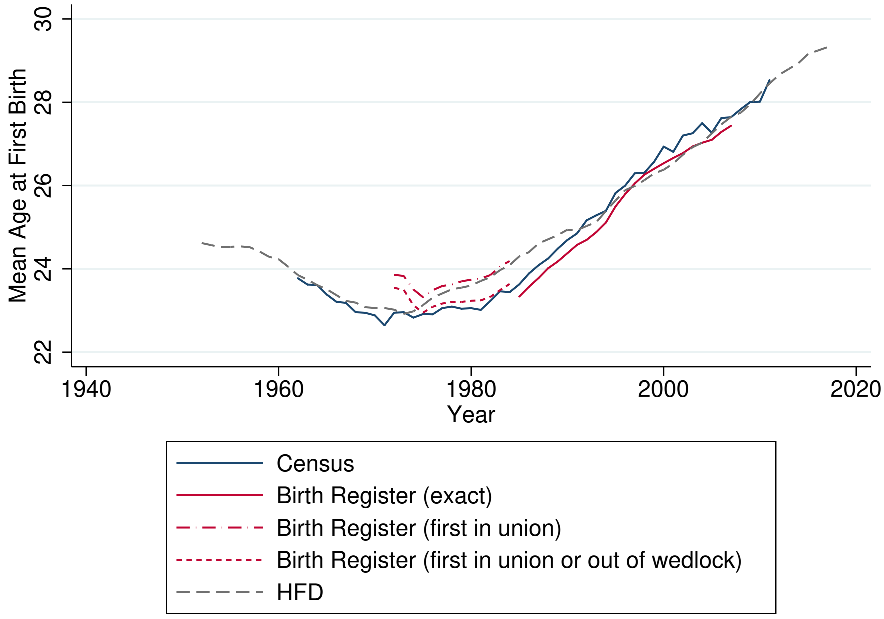
Figure D.II: Evolution of the Fraction of Childless Women by Birth Cohort



Notes: The figure plots the share of childless women (defined as women who will never have children over their lifetime) by birth cohort from the Austrian Birth Barometer (Vienna Institute of Demography) and a backward prediction based on the damped Holt's method ('holt' in R with damped=TRUE and bandwidth 14).

Fraction of Women at Each Event Time To estimate how many mothers are at event time t in a given year s, we need to know the distribution of age at first birth by cohort. Mean age at first birth is available yearly from 1952 to 2017 from the Austrian Birth Barometer (HFD). Figure D.III compares mean age at first birth from the Austrian Birth Barometer and from the Birth Register. We also show age at first birth estimates from IPUMS census microdata (1971-2011).

Figure D.III: Mean Age at First Birth



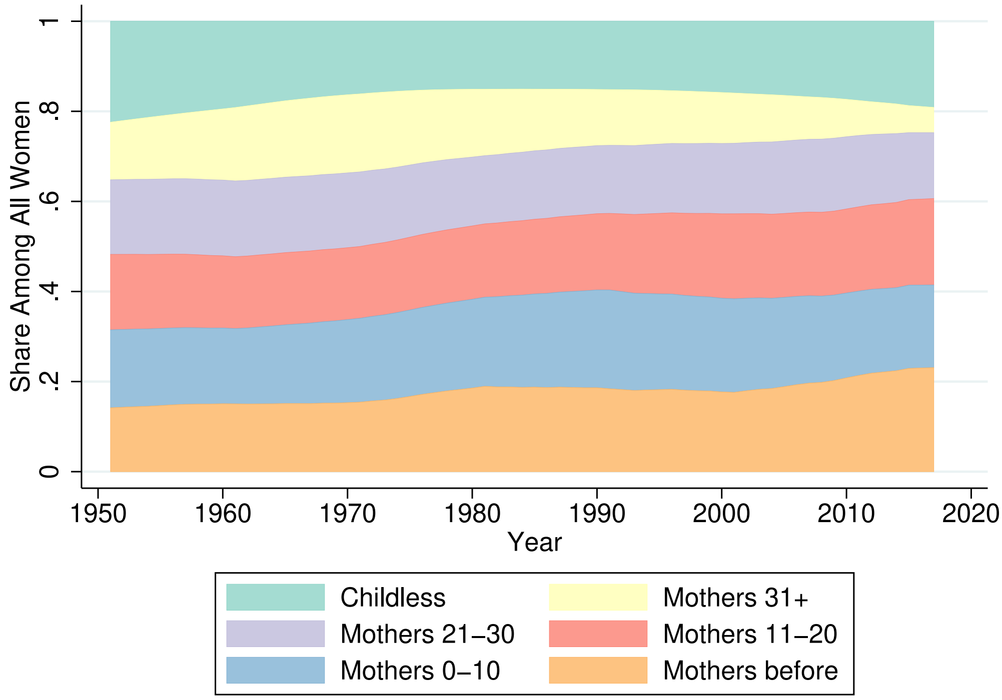
Notes: The figure plots mean age at first birth from various data sources. Census: From the 1971-2011 decennial census (IPUMS and Statistics Austria). We keep all women whose oldest child is between 0-10 at the census date and calculate the average age of the mother in the year the oldest child was born. Birth register (exact): from 1984 the birth register contains data on parity. We keep first births in each year and calculate average age of the mother. Birth register (first in union): before 1984 parity information was only available by mother-father pairs. We keep first births by union in each year and calculate average age of the mother. Birth register (first in union or out of wedlock): We keep first birth by union or births out of wedlock (no father) each year and calculate average age of the mother. HFD: Estimates from the Human Fertility Database.

To get the fraction of women at each event time, we need the distribution of age at first birth, and not just the mean. To this purpose, we fit a Beta distribution to the Birth Register data (1984-2007) for each year. Then we separately regress the two coefficients α_s and β_s of the distribution on mean age at first birth $\bar{A}_s$. This gives us: $\hat{\alpha_s} = 6.404 - 0.122 \cdot \bar{A}_s$ and $\hat{\beta_s} = 30.346 - 0.966 \cdot \bar{A}_s$.

To predict out of sample we simply plug in mean age at first birth from the HFD and predict α_s and β_s for all years in HFD (1952-2017). We then use the implied Beta distribution in each year to assign mothers to the different event times.

The distribution of age at first birth by year and the childlessness estimates finally enable us to estimate, for each year s the composition of women ($\psi_s^{Childless}$ and $\psi_{st}, \forall t$). The following figure shows the evolution of the composition of women, splitting event times into 5 bins : $t < 0, t = 0 - 10, t = 11 - 20, t = 21 - 30$, and $t > 30$.

Figure D.IV: Composition of Women



Notes: The figure plots our constructed shares of childless women and mothers by event time relative to first birth in each calendar year. See text for details on construction of these shares.

D.2.3 Estimating Child Penalties P_{st} & Counterfactual Earnings $\tilde{Y}_{ist}^{w}$ By Birth Cohort

We finally need to estimate child penalties and counterfactual earnings by birth cohort over the long run.

To do this, we extend the baseline specification (1) to allow for year-specific coefficients on event time. Specifically we consider the following specification:

$$Y_{ist}^{g} = \sum_{y} \sum_{j \neq -1} \alpha_{yj}^{g} \cdot \mathbf{I}\left[j = t\right] \cdot \mathbf{I}\left[y = s\right] + \sum_{k} \beta_{k}^{g} X_{kis}^{g} + \nu_{ist}^{g}, \tag{10}$$

where we interact the event time dummies with year dummies in order to estimate year-specific event coefficients α_{yj}^{g}. Note that estimating event coefficients by calendar year s and event year t amounts to estimating event coefficients by birth cohort $c = s - t$. As in our baseline specification (1), we include a full set of age dummies and year dummies in the set of covariates.

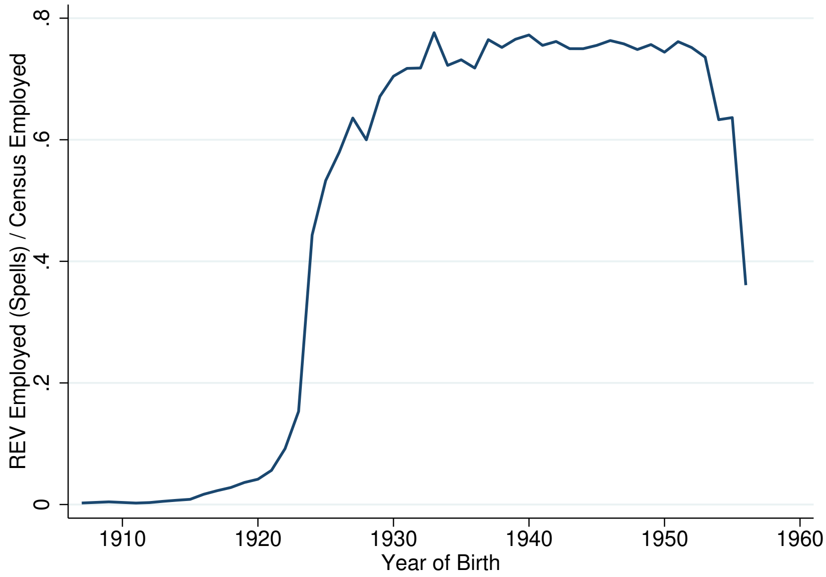
Child Penalties: ASSD Data (1972-2017) and REV data (1960-1972) To compute child penalties by birth cohort since 1960, according to specification (10) we use data from the

ASSD and from the REV before 1972. As explained in section 2, the REV data has a very high coverage rate for women born between the mid-1920s and the 1950s. Panel (a) of Figure D.V below shows that for these births cohorts, the fraction of women with an employment record represents close to 80% of women with employment records in the census. This number is very similar to the coverage rate of ASSD. But due to the way the REV was created, not all women had their earnings recorded even if the employment and maternity insurance spells are recorded in the REV data. The selection of spells that are recorded with earnings is not random: it is related to the fact that the REV data was set up for pension calculation purposes. When the earnings attached to an employment spell were not relevant for pension computation, i.e. spells with low or no earnings levels (not relevant for top 18 years of earnings variable) the corresponding earnings were less likely to be recorded. This means that spells at younger age with lower earnings level are less likely to have recorded level of earnings attached. As a consequence, the number of first births identified with earnings data in the REV data is lower than the true number of first births, as shown in panel (b) of Figure D.V.

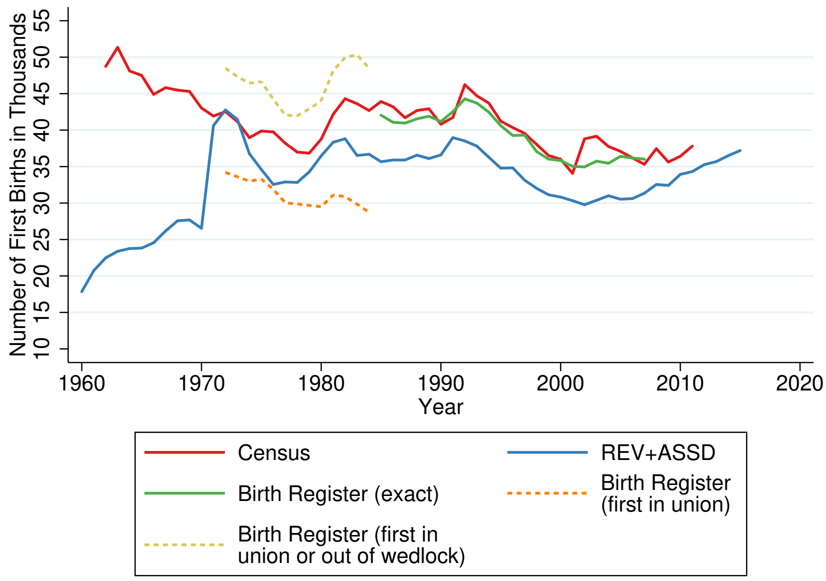
To account for this differential selection of observable first births in the REV data, we use an Inverse Probability Weighting correction. We know that first births are more likely to be recorded with earnings if earnings are relatively high. We therefore reweight the earnings of women who have a first birth in the REV data so that the average earnings of women giving birth relative to average female earnings in Austria in that year matches that observed in the ASSD data. Figure D.VI below shows the evolution of earnings for women observed prior to having children in the REV data up to 1977, and in the ASSD data afterwards. Panel (a) shows the raw data: we see a clear downbreak in the series in 1977, showing that the women who have a first birth spell recorded with earnings information are on average positively selected in the REV data. Panel (b) shows the results when applying our IPW correction.

Figure D.V: Coverage of REV Data

(a) Fraction of Women With Employment Records in REV



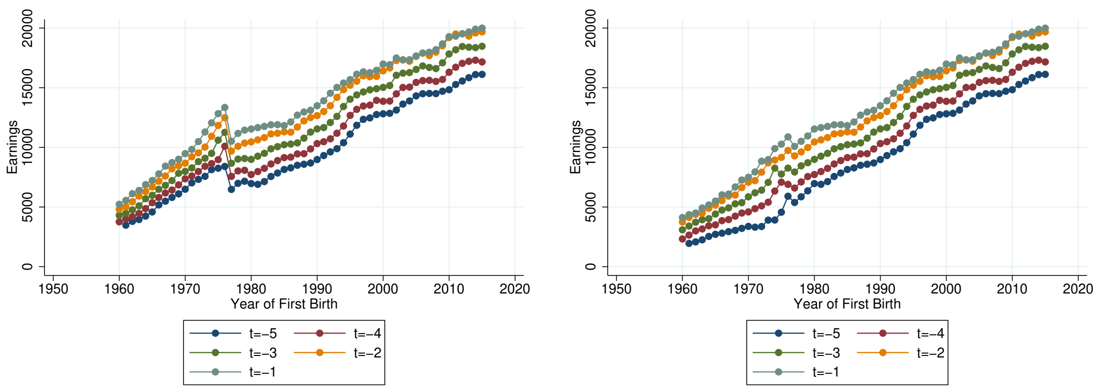
(b) Number of First Births by Year: Comparison Across Data Sets



Notes: Panel (a) plots the share of women with employment records in the REV by year of birth. We select all employment spells from the REV around the 1971 census date (December 5th 1971) and calculate the number of REV employed women by year of birth. We then divide this number by the number of women who are employed according to the 1971 census (again by year of birth). Panel (b) plots the raw number of first birth from various data sources. Census: We keep women with oldest child age 0-10 from each census 1971-2011 and calculate the total number of first births by year. REV+ASSD: We merge the REV and ASSD data and keep the first birth ever observed for each woman. In the REV data this is based on observing a maternity insurance spell, in the ASSD data a live birth is indicated separately. Birth register (various): See notes to Figure D.III for definition of exact and first in union.

Figure D.VI: Earnings by Year of Birth and Event Time in REV and ASSD Data, With and Without IPW Correction

(a) Raw REV + ASSD Data (b) With IPW Correction for REV Data



Notes: Panel (a) plots average annual earnings from the REV and ASSD by year of first birth at various event times. We select all first births over the REV+ASSD period. We exclude mothers who never have positive earnings in the REV if their birth was before 1977 to ensure we have 5 years of pre-birth data for everyone in the sample. Panel (b) plots average earnings weighted by IPW. To construct the weights we run yearly probit regressions of 'after' (first birth in 1980 vs e.g. 1970) on the logs of earnings divided by average earnings in the same year from our decomposition procedure at event times -4 to -1 (we include zeroes by adding 1 to earnings) and whether earnings are zero at any of these event times. Based on the predicted probabilities we then construct inverse probability weights.

Estimated Child Penalties By Birth Cohorts Figure D.VII shows the estimated child penalties by birth cohort, following specification 10, on the ASSD sample and the REV sample with IPW correction. We also plot the estimated counterfactual earnings $\mathsf{E}\left[\tilde{Y}^{w}_{ist}|s,t\right]$ corresponding to these specifications. We see that child penalties in the first ten years after birth have remained remarkably stable over the last 50 years. But child penalties at longer horizons (11 to 20 years after birth, and 21 to 30 years after birth), after being quite stable in the 1960s and early 1970s, have started to decrease significantly.

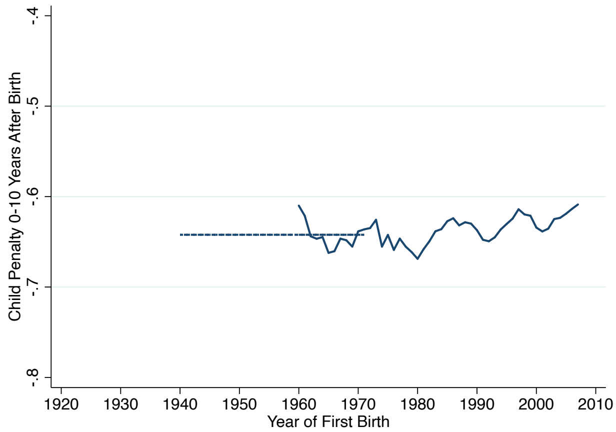
For first births before 1960, we need to extrapolate child penalties: we assume that child penalties for first births before 1960 were equal to the average penalty observed for first births between 1960 and 1972. For counterfactual earnings corresponding to births before 1960, we extrapolate them linearly based on the observed trend in counterfactual earnings for first births between 1960 and 1972.

We estimate specification 10 using event time dummies up to $t = 30$. As a result, we also need to extrapolate penalties after event time 30: given the extreme stability of penalties after event time 10, we fix them at the estimated child penalty level in event time 30: P_{s30}.

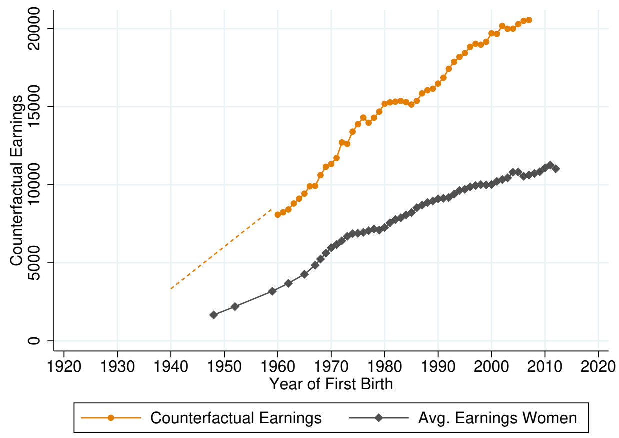
Figure D.VII: Reforms: Dynamic RD Estimates & Effects on Child Penalties

0 to 10 years After Birth

(a) Child Penalty

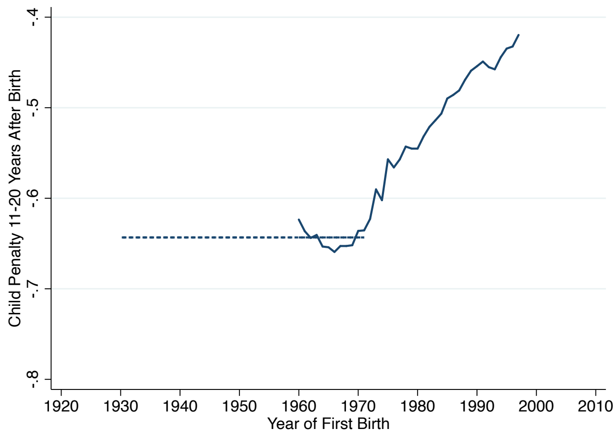


(b) Counterfactual Earnings $\tilde{Y}_{it}$

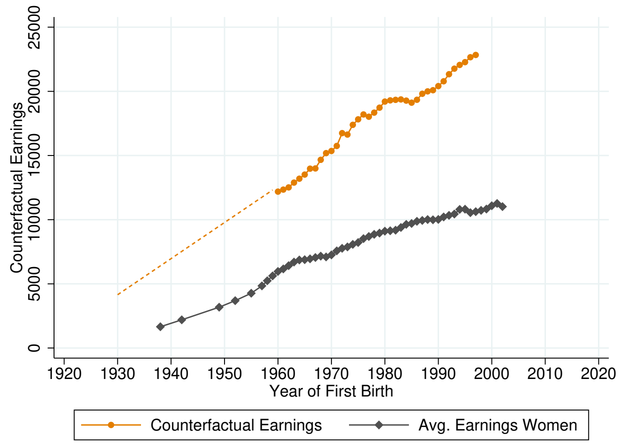


11 to 20 years After Birth

(c) Child Penalty

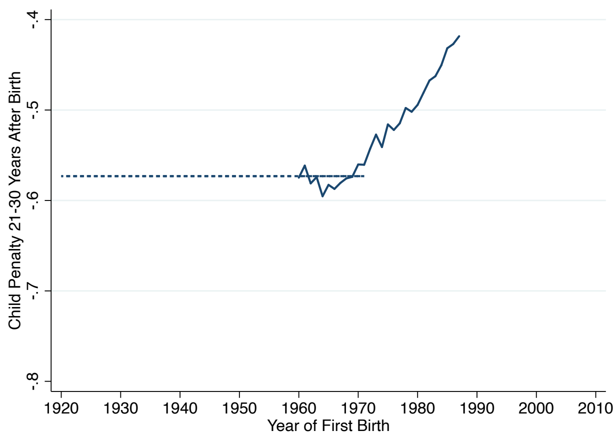


(d) Counterfactual Earnings $\tilde{Y}_{it}$

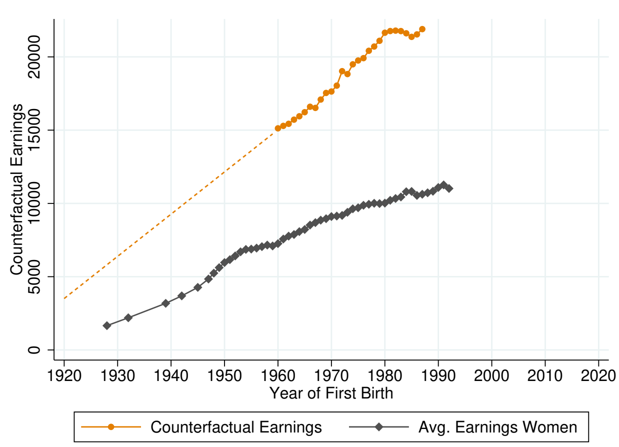


21 to 30 years After Birth

(e) Child Penalty



(f) Counterfactual Earnings $\tilde{Y}_{it}$



Notes: The figure plots average child penalties and predicted counterfactual earnings over event times 0-10, 11-20 and 21-30 by year of first birth (see text for details). Dashed lines indicate the extrapolations we use based on simple averages for child penalties (1960-72 average) and linear backward prediction for counterfactual earnings. We also add our constructed average earnings of all women (including zeroes) in the figures on the right.

D.3 Historical Gender Gaps With Counterfactual Family Policies

We now turn to explaining how we can use the previous decomposition, and our estimates of the effects of policies on child penalties to measure gender gaps under counterfactual policy scenarii.

Our focus is on the three following family policies: parental leave duration (τ_{PL}), the coverage of free nursery care (τ_N), and the coverage of free pre-school care (τ_{PS}). What we are trying to identify, for family policy $\tau_k \in \{\tau_{PL}, \tau_N, \tau_{PS}\}$, is the causal effect of any policy change $d\tau_k$ on the level of the gender gap: $\frac{d\Delta_s}{d\tau_k}$. Based on our previous decomposition exercize, we can express this effect as a simple function of the causal effects of the policy change $d\tau_k$ on the full dynamics of the child penalty ($dP_t/d\tau_k$,$\forall t$). Indeed, we have that:

$$
\begin{aligned}
\frac{d\Delta_s}{d\tau_k} &= -\frac{dE[Y^w_{ist}|s]/d\tau_k}{\mathsf{E}\left[Y^m_{ist}|s\right]} \\
&= -\frac{1}{\mathsf{E}\left[Y^m_{ist}|s\right]} \cdot \left\{ \sum_t \psi_{st} \cdot \frac{dP_t}{d\tau_k}(\tau_{PL}, \tau_N, \tau_{PS}) \cdot \mathsf{E}\left[\tilde{Y}^w_{ist}|s,t\right] \right\}
\end{aligned}
$$

To get the above expression, we used the empirical fact that earnings of men are affected neither by the arrival of kids, nor by family policies. The above expression also assumes that family policies have no effect on fertility, such that $d\psi_{st}/d\tau_k = 0$. But we explore below scenarii where we relax this assumption and allow family policies to affect fertility as well.

Note that the effect of any policy change on the child penalty is theoretically a function of *all* family policies. But in practice, our empirical evidence found no interaction effects between parental leave and child care provision. This means that we can consider the impact of a policy change on the child penalty to be solely a function of that particular policy: $\frac{dP_t}{d\tau_k}(\tau_k)$.

As a consequence, for all family policy reforms since 1961, we can compute the causal effect of that reform on the gender gap in each year s by simply combining:

1. our estimates of ψ_{st}, the fraction of women at each event time t in year s
2. our estimates of average counterfactual earnings $\mathsf{E}\left[\tilde{Y}^w_{ist}|s,t\right]$ for women absent child penalties, at each event time t in year s
3. and our estimates of $\frac{dP_t}{d\tau_k}(\tau_k)$, the effect of each the reform on child penalty at each event time t

Baseline Results: Undoing All Family Policies Our baseline counterfactual policy scenario consists in getting rid of all family policies. In other words, we simulate the evolution of the gender gap since 1961, when eliminating the effects of all parental leave reforms, and setting the level of nursery care and of pre-school care provision to zero. The table below summarizes our estimates of the effect of each policy on child penalties that we use to compute the counterfactual gender gaps. Note that for parental leave reforms, we account for the steady state effect of the reform on child penalties, that is we account for the effect on child penalties of getting access to a new regime PL regime for all children, and not just the first one. This issue is discussed at length in section 3.3 and results of these steady state effects on child penalties are presented in Figure B.XII.

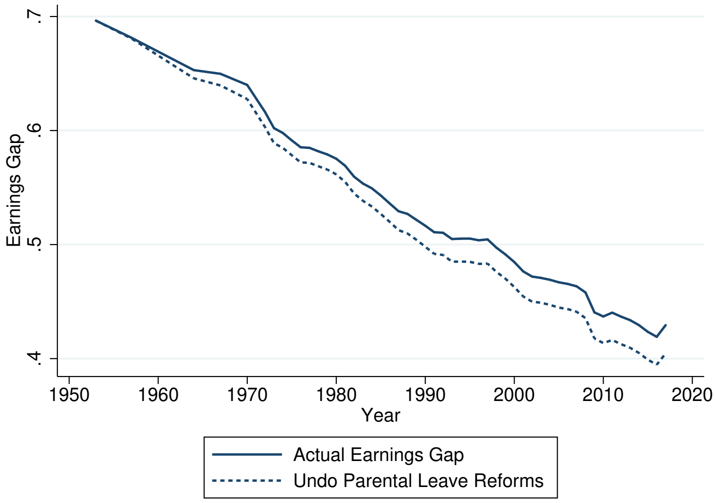
	Avg. effect on 0-10 Penalty (first birth)	Avg. effect on 0-10 Penalty (higher order births)
1961 Reform	-0.0281	-0.0459
1990 Reform	-0.0227	-0.0234
1996 Reform	0.0109	0.0251
2000 Reform	-0.0149	-0.0145
Index 1-2 Expansion (for Index from 0 to 100)	0.022	
Index 3-5 Expansion (for Index from 0 to 100)	0.027	

Note also that given we systematically found no long run effects of any of these policies on child penalties, we systematically set the effect of each policy reform on penalties to be zero for event time superior to 10: $\frac{dP_t}{d\tau_k} = 0, \forall t > 10$.

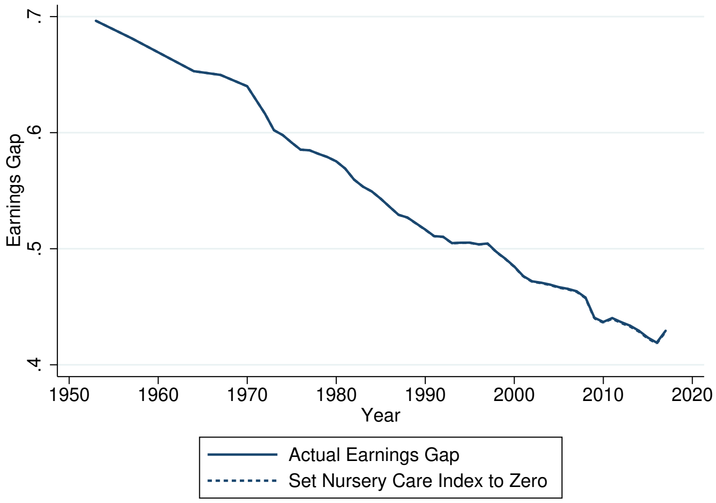
Figure D.VIII below shows the effect on the gender gap of removing each policy separately. Panel (a) shows what the gender gap would have been in the absence of any parental leave reform. In this scenario, we only undo parental leave expansion, and assume that child care provision would have followed the same path as actually observed. The graph highlights that parental leave expansions have had a negative, albeit small, impact on gender inequality. This result stems from the significant negative impact of parental leave policies on labor supply in the short run. But the absence of long run effects of PL on the careers of women means that the overall negative impact of parental leave policies on gender inequality remains somewhat modest. While parental leave policies do have a small effect on the evolution of the gender gap, we find in panel (b) and (c), to the contrary, that child care policies have none. When undoing these policies, we find that the gender gap would have been absolutely unaffected by the absence of nursery care or of pre-school care.

Figure D.VIII: Gender Gap in Earnings and Counterfactual Gender Gaps Under Different Policy Scenarii

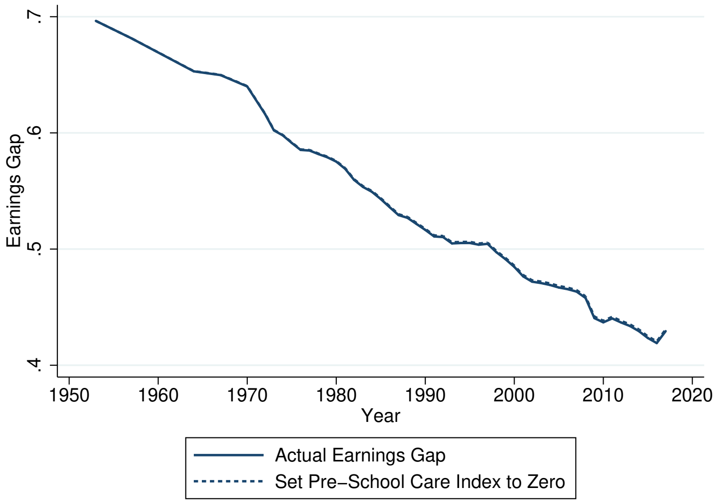
(a) Undoing Parental Leave Reforms Only



(b) Undoing Nursery Care Expansion Only



(c) Undoing Pre-School Care Expansion Only



Notes: The figures plot actual earnings gaps and counterfactual earnings gaps based on setting various policy parameters to zero in isolation. See text and notes to Table 1 for details.

Sensitivy Analysis These results reflect the small point estimates of the impact of family policies on child penalties. But how sensitive are our counterfactual gender gaps to the precision with which these null effects are estimated? Can we reject that family policies have had large effects on gender inequality? To provide some guidance for inference, we compute upper and lower bounds on counterfactual gender gaps. The first "optimistic" scenario uses, instead of our point estimates, the upper bound of the 95% confidence interval on $\frac{dP_t}{d\tau_k}(\tau_k)$ for each policy change. The second "pessimistic" scenario uses the lower bound of the 95% confidence interval. The results of these two bounding scenarii are presented in Table 1. The bottom line is that, even in the optimistic scenario, family policies do no cause any substantial reduction in the gender gap. And the difference between the optimistic and pessimistic scenarii is minor (.428 vs .400). Our null effects are therefore precise: we can rule out large positive effects of family policies on gender inequality.

Robustness to Equilibrium Effects Our mapping between family policies and aggregate gender inequality rests on our quasi-experimental estimates of the effects of these policies on child penalties. It is therefore legitimate to ask if these estimates fully account for the mechanisms through which these policies could affect gender gaps.

One potential concern relates to equilibrium effects in the labor market: do these estimates properly account for all equilibrium effects that may arise from large family policy changes? For child care provision, our quasi-experimental strategy was aimed precisely at capturing equilibrium effects by focusing on differential treatment *across* local labor markets. The fact that we found zero impacts on labor supply and earnings implies no equilibrium impact at the level of local labor markets. For parental leave policy, our quasi-experimental strategy compared treated and untreated mothers within the same labor market, and therefore did not address potential general equilibrium effects. Still, the fact that we found very limited partial-equilibrium effects of these policies on child penalties greatly limits the scope for any significant general-equilibrium effects.

Another concern may be the presence of long-run effects operating through slow-moving changes in preferences or norms. Can our estimates capture them adequately? We note that our difference-in-differences evidence reveals no effects for up to ten years after policy implementation. Moreover, our investigation in Figure 11 suggests that preferences for maternal care remain extremely strong and do not react much to the level of child care provision. It is therefore unlikely that our counterfactual exercise misses significant effects of family policies mediated by changes in preferences.

Anticipatory Effects We have shown that family policies have been largely irrelevant for the evolution of gender inequality over the last sixty years, because their impact on child penalties are extremely muted. As a consequence, the historical decline in gender inequality must have been driven by factors unrelated to children, or to behavioral changes made before the arrival of kids, such as educational investments and discrimination. This, in turn, opens two final questions.

First, could family policies have affected the choices made by women before the arrival of children, thus contributing to the reduction in gender inequality? For example, when child care became more widely available, did women respond by investing significantly more in their career prior to having children? Could our decomposition therefore miss a key mechanism through which family policies reduce the gender gap? The likely answer is no. Our analysis of local child care expansion in Figures 9 and 10 shows that large increases in child care provision have no anticipatory effect on the labor market outcomes of women prior to having children. Even ten years after local child care expansions, we found no significant differences in employment or earnings for women who were about to have kids in treated and control municipalities.

It is critical to assess how sensitive our counterfactual gender gaps estimates are to the precision with which we estimate the effects of policies on child penalties. To this effect, and to provide some guidance for inference, we computed two bounding counterfactual gender gaps. The first, "optimistic" one, uses, instead of our point estimates, the upper bound of the 95% confidence interval on $\frac{dP_t}{d\tau_k}(\tau_k)$ for each policy change. The second, or "pessimistic" scenario, uses the lower bound of the 95% confidence interval. The results of these two bounding scenarii are presented in Table 1. The bottom-line is that our null effects are relatively precise: we can clearly rule out large positive effects of family policies on gender inequality.

Accounting for Fertility Effects Even if family policies do not generate any anticipatory effects prior to motherhood, they could still affect the probability of motherhood as analyzed in sections 3.3 and 4.4. This raises our second question: how robust are our counterfactual estimates to accounting for fertility responses? What has been the causal effect of the expansion of family policies on fertility? And what were the consequences for gender inequality?

Using the estimates from Olivetti and Petrongolo (2017) for parental leave combined with our quasi-experimental estimates for child care provision, we can simulate counterfactual gender gaps when allowing for fertility responses.[47] The results are reported in Appendix Figure D.X.

[47] We take estimates from Table 3, column (8) of Olivetti and Petrongolo (2017). Appendix D.3 provides all details on our methodology.

We assumed, for our baseline counterfactual policy scenario that family policies had no effect on fertility. We now explore scenarii allowing for potential effects on fertility. In practice, this means accounting for potential effects of each policy change on the fraction of women observed at each event time t: $\frac{d\psi_{st}}{d\tau_k}$. The causal effect of the policy change on the gender gap can now be written as:

$$\begin{aligned}\frac{d\Delta_s}{d\tau_k} &= -\frac{dE[Y^w_{ist}|s]/d\tau_k}{\mathsf{E}\left[Y^m_{ist}|s\right]} \\ &= -\frac{1}{\mathsf{E}\left[Y^m_{ist}|s\right]}\cdot\left\{\sum_t \psi_{st}\cdot\frac{dP_t}{d\tau_k}\cdot\mathsf{E}\left[\tilde{Y}^w_{ist}|s,t\right]+\sum_t\frac{d\psi_{st}}{d\tau_k}\cdot P_{st}\cdot\mathsf{E}\left[\tilde{Y}^w_{ist}|s,t\right]\right\}\end{aligned}$$

where the second part in the bracketed term accounts for fertility responses. Intuitively, if a policy reform induces more couples to have children, it will increase the fraction of women observed at each event time t after birth ($d\psi_{st}/d\tau_k > 0$. As these women are facing penalty P_{st}, this will increase the gender gap.

As discussed above, we have very limited evidence on the impact of family policies on fertility and thus no consensus.

We use different approaches to account for fertility effects of parental leave and of child care expansions. First, **for parental leave**, existing empirical evidence is scant. Most studies in the literature have compared, like we did in Appendix Figure B.XI, the completed fertility rates of women who are exposed, for their first child, to short vs long parental leave durations. But this does not capture the relevant effect on total fertility, because in such a quasi-experimental setup, women face the same parental leave regime for any future children. Furthermore, one cannot identify from such variation the effect of parental leave on the probability of having a first child. To get at the effect of interest, we would need to compare the total fertility of women who are experiencing two different parental regimes *permanently*. This is challenging because parental leave policies usually apply to all women in a given country. This has forced researchers to rely on cross-country variation as in Olivetti and Petrongolo (2017). To account for fertility effects of parental leave we use the estimates from Olivetti and Petrongolo (2017) Table 3 column (8). Their formula for total fertility rate is (from the replication data set)

$$TFR = \ldots - 0.0007448 \times \text{leave\_weeks} + 0.0013043 \times \frac{\text{leave\_weeks}^2}{100} + 0.0019767 \times \%\text{weeks\_paid} + 0.0003374 \times \%\text{payrate}$$

The following table shows what these estimates imply when we undo parental leave. Note that in their data set leave_weeks is equal to weeks of maternity insurance plus weeks of parental leave. So undoing parental leave but keeping maternity insurance means setting leave duration to 8 weeks.

Table D.I: Summary of Fertility Impact Computed Based on Estimates from Olivetti and Petrongolo (2017)

Regime	leave (wks)	%wks paid	%payrate	Counterfactual leave (wks)	Counterfactual %wks paid	Counterfactual %payrate	TFR Effect of PL
1961-1990	60	100	52	8	100	100	-0.0088
1990-1996	112	100	46	8	100	100	0.0671
1996-2000	112	75	37	8	100	100	0.0146
2000-2008	112	123	30	8	100	100	0.1072

Notes: The table shows the policy parameters we use for the different periods to calculate the total fertility effect of parental leave policies. We use the formulas in the online dofiles provided by Olivetti and Petrongolo (2017).

To account for fertility effects of **child care expansions** we use our own estimates of the effect of these roll-outs on total fertility rate. We constructed total fertility rate at the municipality level as follows. First, we get the number of births at the municipality level from the birth register (1988-2007) and Statistik Austria Register Data (2002-2018).[48] Then we get the number of women age 15-49 from the census (1991, 2001, 2011) and the Population Register (2011-2017). We interpolate linearly to get yearly data. Dividing the number of births by the number of women is the crude fertility rate. We then use the Austrian aggregate yearly relationship between crude fertility rate and total fertility rate to convert crude to total fertility rate.

We use the effects 10 years after expansion as an estimate for the TFR impact of child care expansion, scaled by the difference in the index during event times 0-5.

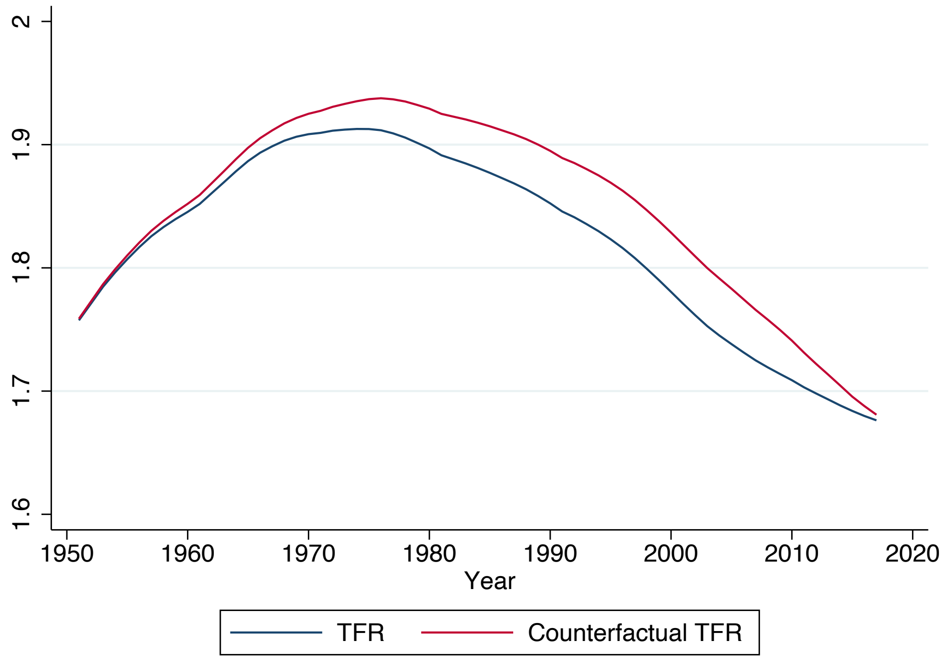
	Effect at $t = 10$	Avg. change in index	Scaled Effect
Nursery Care Expansion	-0.049645	0.363941	-0.1364
Pre-School Care Expansion	-0.046957	0.369499	-0.1271

Figure D.IX shows actual and counterfactual total fertility rates. Counterfactual means fertility when there is no parental leave and no child care.

[48] And we take the average of those two numbers for years 2002-2007 where the data overlaps, to account for a few small discrepancies in the data.

Figure D.IX: Actual and Counterfactual Total Fertility Rates

(a) By Year



Notes: The figure plots the total fertility rate (TFR) calculated by multiplying the number of children by the shares of women who have 0-4 children as constructed for the main decomposition exercise. The counterfactual TFR is based on setting all family policy parameteres to zero taking into account our estimated child care expansion fertility effects and the parental leave expansion effects based on Olivetti and Petrongolo (2017).

To convert our estimated fertility effects into estimates for $\frac{d\psi_{st}}{d\tau_k}$, we proceed as follows. Our fertility estimates are, theoretically, measuring the effect of policy reforms on total fertility $dN/d\tau_k$, where N is the average number of children by women. Note that $N = (1 - \psi^{childless}) \cdot \bar{n}$, where $\bar{n}$ is the average number of children among women with children and $\psi^{childless}$ is the fraction of childless women.

What we do is to assume that policy reforms affect the total fertility by changing the fraction of women with kids rather than the number of kids per mother. In other words, we assume that $dN/d\tau_k = d(1 - \psi^{childless})/d\tau_k \cdot \bar{n}$

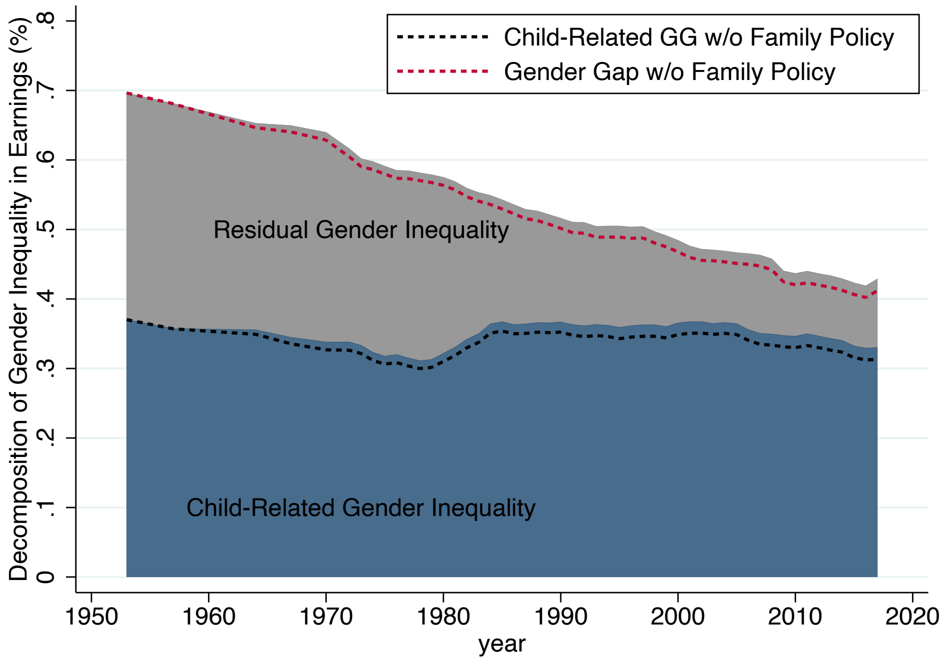
As a consequence, we easily obtain an estimate of the effect of the reform on the fraction of women at event time t in year s:

$$\begin{aligned} \frac{d\psi_{st}}{d\tau_k} &= -\frac{d(\psi^{childless})}{d\tau_k} \cdot \psi_{st} \\ &= \frac{dN}{d\tau_k} \cdot \frac{\psi_{st}}{\bar{n}} \end{aligned}$$

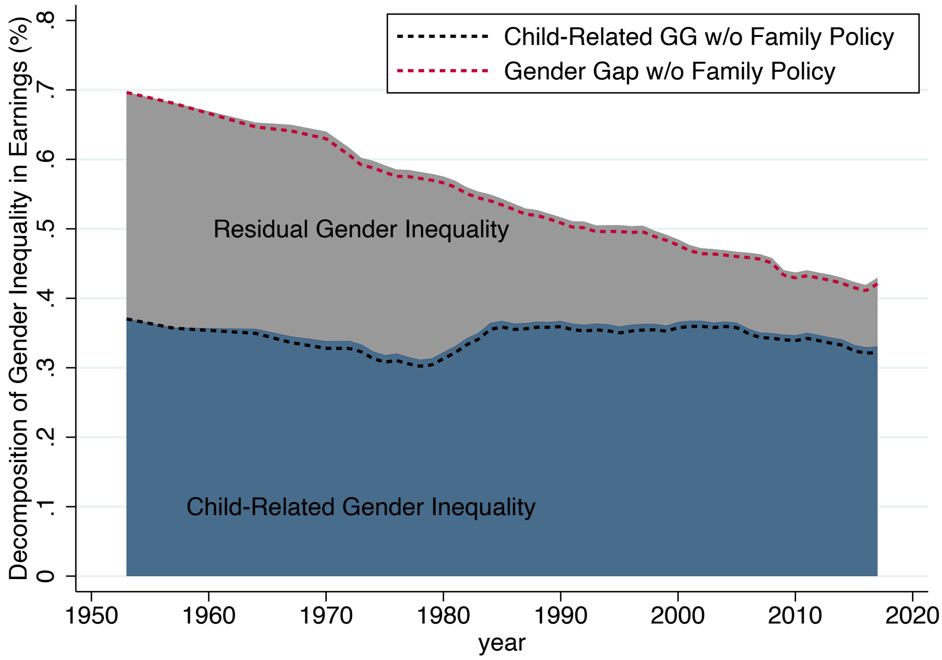
Figure D.X shows the results of our counterfactual policy scenario of removing all family policies, when accounting for fertility responses. The figure shows that fertility responses to family policies are too small to make any substantial difference to gender inequality. We estimate that, when accounting for fertility responses, the gender gap would be 0.423 today in the absence of any family policy. This is almost undistinguishable from our baseline estimate of 0.414 without fertility responses. In Table D.III we also explore the sensitivity of our results to the precision with which the fertility effects are estimated. We computed two bounding counterfactual gender gaps. The first, "optimistic" one, uses, instead of our point estimates, the upper bound of the 95% confidence interval on $\frac{dN}{d\tau_k}$ and $\frac{dP_t}{d\tau_k}(\tau_k)$ for each policy change. The second, or "pessimistic" scenario, uses the lower bound of the 95% confidence interval. We find that the range of possible gender gap estimates between these two bounds is very narrow, going from 0.437 to 0.408. We can therefore credibly rule out the presence of significant effects of family policies on gender inequality.

Figure D.X: Counterfactual Gender Gaps Under No Family Policy Accounting for Fertility Responses

(a) Without Fertility Responses



(b) With Fertility Responses



Notes: The figures plot the results of our decomposition exercise removing all family policies when we do not take fertility effects into account (panel (a)) and when we incorporate fertility effects (panel (b)). See text for details.

Table D.III: Sensitivity Of Counterfactual Gender Gap Estimates

Year	1953	1964	1970	1980	1990	2000	2010
Panel A. Decomposition							
Actual Earnings Gap	0.696	0.653	0.640	0.575	0.517	0.485	0.437
Counterfactual Earnings Gap	0.326	0.297	0.302	0.253	0.149	0.119	0.090
Gap Due to Kids	0.370	0.356	0.338	0.322	0.367	0.366	0.347
Panel B. Undoing the Reforms Without Fertility Response							
No Parental Leave and No Child Care (Baseline)							
Earnings Gap	0.696	0.646	0.628	0.562	0.499	0.463	0.414
Child-Related Gender Gap	0.370	0.349	0.326	0.309	0.349	0.344	0.324
No Parental Leave and No Child Care (Optimistic)							
Actual Earnings Gap	0.696	0.648	0.632	0.565	0.503	0.474	0.428
Child-Related Gender Gaps	0.370	0.351	0.330	0.312	0.353	0.355	0.338
No Parental Leave and No Child Care (Pessimistic)							
Earnings Gap	0.696	0.644	0.624	0.559	0.495	0.452	0.400
Child-Related Gender Gap	0.370	0.346	0.322	0.306	0.345	0.333	0.309
Panel C. Undoing the Reforms With Fertility Responses							
No Parental Leave and No Child Care (Baseline)							
Earnings Gap	0.696	0.646	0.629	0.565	0.505	0.472	0.423
Child-Related Gender Gap	0.370	0.349	0.327	0.312	0.356	0.353	0.332
No Parental Leave and No Child Care (Optimistic)							
Earnings Gap	0.696	0.649	0.633	0.568	0.510	0.483	0.437
Child-Related Gender Gap	0.370	0.352	0.331	0.315	0.360	0.364	0.347
No Parental Leave and No Child Care (Pessimistic)							
Earnings Gap	0.696	0.644	0.625	0.561	0.501	0.460	0.408
Child-Related Gender Gap	0.370	0.347	0.323	0.308	0.352	0.341	0.318

Notes: The table reports inputs and results of our decomposition exercise of gender gaps for selected years, when accounting for fertility responses to family policies. See notes to Table 1 for additional details. The "optimistic" scenario is based on the upper bound of the 95% confidence interval for all policy treatment effect estimates and the "pessimistic" scenario is based on the lower bound of the 95% confidence interval.