Can Paternity Leave Reduce the Gender Earnings Gap?*

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Abstract

This paper examines the impact of paternity leave uptake on the gender gap in labor market outcomes. Utilizing administrative data from Canadian tax records, we analyze Quebec's 2006 parental leave reform, which introduced five weeks of paid leave exclusively to fathers and significantly increased fathers' uptake of parental leave. Using mothers and fathers of children born around the reform, we estimate how the policy impacted labor market outcomes up to 10 years following birth. The reform significantly reduced fathers' earnings immediately after birth. However, in the medium to long-run, we find that the reform did not impact earnings, employment, or the probability of being employed in a high-wage industry for either parent. We for instance find a 95%-CI for the effect on average female earnings 3-10 years following birth ranging from -2.2 to +1.7%. Estimates of effects on other outcomes and for males are similarly precise zeros. There is likewise no evidence that the reform differentially affected care-taking and family responsibilities in the treated population.

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1 Introduction

A growing body of evidence attributes much of the gender gap in labor market outcomes to the "motherhood penalty", that is the large and persistent drop in earnings that women – but not men – experience following the birth of their first child (see Kleven et al. (2019) for an overview of motherhood penalties across countries and Connolly, Fontaine, and Haeck (2023) for the motherhood penalty in Canada, specifically). Mothers often shoulder the lion-share of childcare responsibilities, leading them to work fewer hours or seek flexible work arrangements even if this requires accepting lower wages. Closing gender gaps thus requires fathers to take on more of the childcare responsibilities. To induce this shift in behavior, several European and Scandinavian countries have implemented reforms aimed at increasing parental leave take-up by fathers (Cortes and Pan (2023)).[1] This motive behind these policies is attested to by the Swedish government introducing the paternity leave reform of 1995: "A shared responsibility for the practical care of children would mean a more even distribution of interruptions in work between women and men, and women would thereby gain better opportunities of development and making a career in their profession." (quoted in Ekberg, Eriksson, and Friebel (2013), p. 4).

The goal of enabling women to reconcile the demands of the labor market with those of childcare also underpin the 2006 reform by the Canadian province of Quebec of its parental leave system. The reform involved several changes to the existing federal system, including increasing the generosity by raising the replacement rate during the first 30 weeks of leave from 55% to 70% and the maximum weekly benefit from $412 to $767 (in 2006 Canadian dollars). Importantly, the reform also increased parental leave by 5 weeks but earmarked these additional weeks for fathers. The policy succeeded in drastically increasing the share of fathers taking some parental leave from 20% to 60%.[2]

This paper shows that this reform did not affect labor market outcomes such as employment and earnings of either mothers or fathers over the long run and thus did not reduce gender disparities in earnings. This null finding rests on data from tax returns that allow us to estimate mothers' and fathers' earnings trajectories from the birth of the first child up to ten years after. We estimate the causal effects of the reform using the fact that only

1. Such policies primarily reserve leave weeks for fathers ("daddy quotas"). Examples include Norway (since 1993), Sweden (since 1995), France (since 2002), and Spain and Germany (since 2007). In 2019, the Canadian federal government expanded the father-reserved weeks policy from Quebec to the rest of the Canada. Other designs aimed at increasing fathers' leave uptake include bonus weeks tied to shared leave (Quebec since 2020) and a shift from optional paid paternity leave to mandatory leave (France since 2021).

2. Estimates based on data on non-regular employment benefits from the Longitudinal Administrative Databank. Patnaik (2019) reports a larger effect on take-up of 53 percentage points using the Employment Insurance Coverage Survey.

parents of children born on or after January 1st, 2006, were eligible for the new parental leave scheme. Our identification strategy relies on a difference-in-differences design that compares the outcomes of parents whose first child was born at the beginning of 2006 to the outcomes of parents whose first child was born at the end of 2005, controlling for the corresponding difference in outcomes using pre-reform years.

In line with the increase in paternity leave uptake, we find that the reform led to a decrease in fathers' earnings during the first year following childbirth. A similar decline in earnings is found for new mothers because the increased generosity also induced them to take more leave. However, we find no evidence that the reform impacted long-term earnings trajectories of either mothers or fathers. Our estimates are precise and rule out increases in mothers' earnings 3–10 years after childbirth greater than \$530 or decreases greater than \$690.[3] Relative to average earnings in the comparison group, this corresponds to ruling out increases greater than 1.7% or decreases greater than 2.2%. For fathers, we similarly rule out changes in earnings 3–10 years after childbirth larger than a decrease of \$710 or an increase of \$1,290, corresponding to rejecting earnings declines greater than 1.3% or increases greater than 2.3%. Combining, we can rule out that the reform reduced the gender earnings gap by more than \$800, which correspond to a 3.3% reduction from a baseline earnings gap of \$24,200. Together, our results indicate that the 2006 reform did not significantly attenuate gender earnings inequalities in Quebec. In particular, the dramatic increase in paternity leave taking by fathers did not seem induce any long run changes in outcomes for either gender.

As far as we can tell, these null effects do not arise because we average large positive and negative effects in the population. We find no effect of the reform on the earnings distribution of fathers and mothers and no heterogeneity by age at first birth, immigration status, or income category prior to birth. We also perform a battery of robustness checks and consistently find no effect of the reform across alternative specifications.

We explore potential mechanisms that have been proposed by examining how the reform impacted various indicators of social norms around care-giving that we derive from the administrative data. These include the propensity of fathers to share in the custody of their children subsequent to a separation or the propensity of fathers to take leave following the birth of subsequent children. We also investigate whether the reform affected the propensity of separations or subsequent fertility. We find no significant effects on any of these indicators, suggesting that the reform failed to shift traditional norms surrounding childcare and work-sharing in the family. We believe this is the reason why we fail to find any effects: the

3. All earnings values are expressed in Canadian dollars and adjusted for inflation to 2019.

policy reform does not suffice to affect care-giving arrangements which may reflect deeply held preferences and social norms. In light of this, nudging fathers to take a few weeks of leave does not make much of an impact.

We interpret our results primarily as speaking to the absence of an effect of increased leave uptake by fathers on subsequent outcomes. This interpretation is complicated by the fact that the reform was composite and also led to an increase in leave-taking by mothers. However, the change in mothers' leave-taking is small relative to the change observed for fathers. First, at the extensive margin, mothers' leave take-up rate increased from 72% to 77%, corresponding to a 7% increase, whereas fathers' take-up rate rose from 17% to 57%, an increase of approximately 230%. Second, while we do not observe leave duration directly, evidence from Patnaik (2019) suggests that changes in total leave duration are also much larger for fathers than for mothers: she finds that the reform increased total leave duration by approximately 160% for fathers (from 2 to 5.2 weeks), while changes for mothers were around 4% (from 42.5 weeks to 44.5). These patterns suggest that the reform primarily affected fathers' leave uptake, while changes in mothers' leave-taking were modest. Thus, we argue that our inability to find long run effects on labor market outcomes suggests that the substantial increase in paternity leave had little effect on the longer run division of labor within the household and the gender earnings gap.

Our paper can't identify which specific components of the Quebec reform drove the increase in paternity leave uptake. The reform combined several changes, including the introduction of father-reserved weeks, increased benefit generosity, and expanded eligibility, each of which may have contributed to higher leave-taking by fathers. Prior evidence nevertheless points to an important role for father-reserved weeks (or "daddy quota"). For instance, Andresen and Nix (2024) shows, using several Swedish reforms, that paternity leave uptake is primarily driven by daddy quotas rather than by other policy features. Moreover, Patnaik (2019), who studies the same reform as ours and examines how the effects on leave duration differed across groups, provides several pieces of evidence that father-reserved weeks, rather than benefit generosity alone, played a key role. In particular, she shows that fathers' leave durations clustered tightly around the number of weeks reserved for them, even when additional shared leave remained unused.

Our findings add to a small set of papers that have previously examined the effects of the Quebec parental leave reform on labor market outcomes, including a well-known study published in this journal (Patnaik (2019)) as well as Dunatchik and Özcan (2021) and Choi, Margolis, and Holm (2025).[4] In the Online Appendix, we summarize key features of these

4. In a related contribution, Haeck et al. (2019) examine a wide range of outcomes, from labor market

studies (Tables C.1 and C.2) and present the results of replication exercises in which we implement each paper's empirical design using our data (Tables C.3-C.6). With respect to short-run labor market outcomes, our findings are fully consistent with the existing literature. In particular, the reform increased leave-taking and led to reductions in earnings for fathers, and to a lesser extent mothers, in the year following childbirth.

As far as medium-run outcomes are concerned, our results differ from those of Patnaik (2019) and Dunatchik and Özcan (2021), who examine labor market outcomes for parents of children aged 1–3 and 0–5, respectively. Both papers report positive effects of the reform on mothers' labor market outcomes at these ages. One potential source of this discrepancy is a difference in the outcomes considered. Our analysis focuses on employment and annual earnings, whereas these studies additionally examine intensive-margin outcomes such as full-time employment, hourly wages, and time spent in paid work, which we do not observe. Nevertheless, persistent changes along these intensive margins should, in principle, translate into changes in annual earnings, which we measure comprehensively. From this perspective, our finding of no effect on parents' annual earnings contrasts with the intensive-margin effects reported in these studies. We argue that the divergence in findings primarily reflects differences in empirical design and data limitations rather than differences in outcomes considered. Both Dunatchik and Özcan (2021) and Patnaik (2019) compare parents of young children in Quebec to parents in the rest of Canada across distant calendar years. To the extent that labor market conditions evolved differently across provinces over time, such comparisons may conflate the effects of the reform with differential underlying trends affecting control and treated groups. Moreover, because both studies rely on survey data, estimates are based on relatively small treatment samples on the order of 100 to 500 mothers or fathers which may further amplify sensitivity to sampling variation and model specification. In addition, in Dunatchik and Özcan (2021), the construction of treatment and control groups leads to substantially different child-age distributions across groups, and the analysis does not control for child age. As a result, estimated differences may partly reflect differences in child age rather than the causal effect of the reform. By contrast, our administrative data allow us to construct treatment and control groups numbering in the tens of thousands, defined around narrow birthdate windows, and to measure outcomes for treated and control parents within the same calendar years. This framework substantially limits the scope for bias from differential time trends and yields much more precise estimates.[5]

effects to child development. Their labor market analysis focuses on very short-run outcomes and therefore does not directly address the research question studied here.

5. When we replicate Dunatchik and Özcan (2021) and Patnaik (2019) using our data, we find employment effects for mothers and fathers that are either zero or substantially smaller than those reported in the original studies. We also find negative earnings effects for both mothers and fathers. See Appendix Tables C.4 and

Choi, Margolis, and Holm (2025) is the only existing study that examines long-run labor market outcomes. It is also the study most closely related to ours, as it relies on the same large administrative tax data. Their estimates are broadly consistent with ours for employment outcomes and for fathers. However, in contrast to our findings, they report positive earnings effects for mothers in the 2–10 years following birth. To better understand the source of this discrepancy, we attempt to reproduce their results by replicating their empirical approach as closely as possible given the information available in the paper (see Tables C.5 and C.6 in the Appendix). For reasons that we can not explain, we are unable to recover the positive earnings effects for mothers.

Our paper contributes to the literature on the effects of paternity leave uptake on parents' labor market outcomes following childbirth. Credible causal evidence on the topic from other countries is relatively scarce. Farré and González (2019) find that in Spain the introduction of two weeks of paternity leave quota increased mothers' labor force participation and incomes up to two years after childbirth without impacting fathers' labor market outcomes. They provide no evidence on long-run effects. Evidence on longer-run effects of paternity leave use is available for Scandinavia (Ekberg, Eriksson, and Friebel (2013); Rege and Solli (2013); Cools, Fiva, and Kirkebøen (2015); Druedahl, Ejrnæs, and Jørgensen (2019); Andresen and Nix (2024)). These studies show null or limited effects on parents' labor market outcomes and gender earnings inequalities beyond the first two years after childbirth. Our results for Quebec are consistent with these findings suggesting that the absence of such effects may not be unique to Scandinavia. We also contribute to this literature by showing that the lack of effect may stem from paternity leave's failure to alter traditional social norms around care-giving in the treated group.

For context, Quebec is a society with strong norms supporting gender equality, both compared to Canada as well as compared to many other countries in the world. For example, data from the world value survey shows that in Quebec only 16.4% of responds agree with the statement that "When a mother works for pay, the children suffer." In the rest of Canada, 22.4% agree, whereas in Germany, Spain, or Sweden close to one-third of responds agree with this statement. An interesting contrast to other countries emerges in that respondents in Quebec also are unusually willing to agree with the statement that "Being a housewife is just as fulfilling as working for pay." Two-thirds of respondents in Quebec agree with this statement, compared to just 42% in Sweden, 49.5% in Spain, and 57% in Norway. Quebec thus seems to be a society accepting of diverse life choices related to childcare. See Appendix F for more evidence on gender norms in Quebec from the World Value Surveys.

C.3 for detailed replication results.

More generally, our study speaks to the literature on the link between public policies and the child penalty (see the reviews by Kleven et al. (2019) and Cortes and Pan (2023)). Recent research indicates that policies designed to facilitate the work-life balance of mothers, such as affordable childcare and maternity leave, have only a small impacts on the child penalty. Paternity leave policies are hoped to reduce labor market disparities by changing traditional social norms around care-giving. It is of course possible that social norms at large might slowly change as paternity leave becomes more common, but our findings suggest that there is no direct effect of expanded paternity leave on the division of labor among families treated by the Quebec reform of 2006.

2 Parental Leave Policies in Quebec

Before 2006 and across all Canadian provinces, parents who had worked at least 600 hours in the preceding year were entitled to a combined total of 50 weeks of compensated parental leave surrounding the birth or adoption of a child through the federal Employment Insurance program.[6] These weeks were compensated at 55% of earnings, up to a maximum of $412 a week.

Of these 50 weeks, 15 were specifically reserved for mothers, while the remaining 35 could be shared between both parents. In practice, few fathers shared in the leave. Our estimates based on employment insurance benefits indicate that before the 2006 reform, only 20% of fathers took any parental leave weeks, compared to 74% of mothers.

On January 1, 2006, Quebec launched its own parental leave program, the Quebec Parental Insurance Plan (QPIP). QPIP introduced several significant changes detailed in Appendix A: it lowered eligibility criteria, introduced flexibility in the choice of leave duration, and increased the replacement rate. Moreover, QPIP reserves five weeks of compensated leave specifically for fathers. These earmarked weeks, often referred to as "daddy quotas" or paternity leave weeks, were specifically intended to increase leave-taking by fathers. As we show in the following sections, following the reform the share of father taking at least one week of parental leave increased by 40 percentage points, thus reducing the gender gap in this measure by two-thirds.

6. Paid parental leave was first introduced by the federal government in 1971, allowing mothers to take up to 15 weeks of paid leave. In 1990, the federal government introduced 10 additional weeks of paid leave that could be shared between both parents. This provision was extended to 35 weeks in 2000.

3 Data

3.1 Data Sources

Our main data source is the Canadian Employer-Employee Dynamics Database (CEEDD), maintained by Statistics Canada (Statistics Canada (2019a)). The CEEDD contains individual tax returns and child benefits records but also data derived from employers tax filings which we do not use. It is based on the universe of tax filers and non-filing family members (spouses and children), covering 95% of the Canadian population (Statistics Canada (2019b)). The children's birth dates listed in the CEEDD allow us to identify parents affected by the reform and define our control groups. It also contains annual earnings by employer. We can thus use these data to estimate the intent-to-treat effects of the reform on earnings and other labor market outcomes.

The version of the CEEDD we use does not allow us to directly determine parental leave take-up.[7] We therefore turn to the Longitudinal Administrative Databank (LAD) (Statistics Canada (2023)) to measure take-up. The LAD is derived from the same administrative datasets as the CEEDD but includes additional variables on Employment Insurance benefits, which enable us to measure take-up. Other information available in the LAD also allows us to construct indicators of gender norms around caregiving. We therefore use the LAD to estimate the effects on take-up and gender norms. Unfortunately, however, the LAD covers only a 20% random sample of the tax-filing population, resulting in a smaller sample size for estimating these effects.

3.2 Outcomes

Our main outcome of interest is annual employment earnings (in 2019 CA$).[8] The CEEDD version we use covers the calendar years up to 2015, allowing us to estimate the effects of the reform up to ten years after childbirth. We report the effects on earnings for every year relative to childbirth, as well as on average earnings from three to ten years after birth. We also study the effects of the reform on two additional labor market outcome measures: whether employed and whether employed in a high-paying industry.

We use the information on non-regular employment benefits to identify parental leave take-up in the LAD.[9] Non-regular employment benefits include parental leave benefits, as well

7. Information from the Record of Employment file in the CEEDD is not completely reliable for identifying parental leave take-up (Hou, Magolis, and Haan (2017)). In addition, the version we use does not contain detailed Employment Insurance benefit variables.

8. 1 Canadian dollar corresponded to roughly 0.93 US dollar during 2006-2015.

9. Because benefits are observed only on an annual basis in the LAD, we cannot determine the precise

as sickness and care-giving benefits. We cannot distinguish between parental leave benefits and sickness and care-giving benefits, however, the small share of individuals receiving any such benefits outside the years around childbirth – about 3% of the population – indicates that it can be used as a good proxy for parental leave use. Specifically, our proxy for parental leave use is a binary variable indicating if the individual has received any non-regular leave benefits during the first year following childbirth.

Appendix B provides additional details on the construction of the outcomes.

3.3 Sample Selection

We restrict our sample to parents who had their first child around the time of the reform, i.e., between July 2004 and June 2006.[10] We further restrict the sample to parents residing in Quebec at the child's birth and throughout the first year of life. We retain parents who move out of Quebec after the first year since parental leave occurs primarily during the child's first year.[11] We exclude 6% of the births for which the exact day of birth can not be ascertained. We keep all parents, including single mothers and fathers since family structure at birth may itself be endogenous.[12] Finally, we restrict our sample to parents who filed taxes every year from the year prior to the birth to ten years after. The requirement that they file taxes prior to the birth allows to identify the province of residence around childbirth. We show in Appendix E.3 that our results are substantially the same if we relax this restriction.

3.4 Descriptive Statistics

Table 1 presents descriptive statistics for our sample, disaggregated by gender, and date of birth of the first child.

dates on which individuals took leave and therefore cannot replicate results from Andresen and Nix (2024) showing that fathers primarily took leave concurrent with mothers or during the traditional summer vacation months.

10. In our datasets, the dates of birth of children refer to the current dependents of individuals. We detail how we identify the first child ever born to each individual in Appendix B.3.

11. Specifically, we restrict the sample to parents residing in Quebec on December 31 of both 2005 and 2006 for the main group, and on December 31 of both 2004 and 2005 for the control group.

12. Our data allow us to identify lone parents at birth as single filers living with a child. According to this definition, approximately 10% of births are to "lone mothers" and 1% to "lone fathers." Excluding these individuals from the analysis has a negligible effect on our results.

Table 1: Descriptive Statistics by Gender and Date of Birth of the First Child

	Date of birth of first child			
	July-Dec 2004 (1)	Jan-June 2005 (2)	July-Dec 2005 (3)	Jan-June 2006 (4)
Panel A: Women				
Average annual earnings (years 3–10)	31.2	31.3	32.1	32.2
(in 1,000s of 2019 CA$)	(26.7)	(26.3)	(27.0)	(26.6)
Frac. of years with positive earnings (years 3–10)	0.80	0.80	0.80	0.80
Frac. of years in a high-paying industry (years 3–10)	0.15	0.15	0.16	0.15
% with a second child	73.0	73.3	74.0	74.6
% of immigrants	14.7	14.0	15.9	14.9
Age at first child birth	28.2	28.2	28.4	28.6
	(4.8)	(4.7)	(4.7)	(4.5)
Panel B: Men				
Average annual earnings (years 3–10)	55.7	55.7	56.3	56.5
(in 1,000s of 2019 CA$)	(39.0)	(38.0)	(39.4)	(38.8)
Frac. of years with positive earnings (years 3–10)	0.90	0.91	0.90	0.91
Frac. of years in a high-paying industry (years 3–10)	0.26	0.26	0.27	0.26
% with a second child	74.4	75.0	75.2	75.0
% of immigrants	14.8	14.1	16.0	14.7
Age at first child birth	30.6	30.5	30.7	30.8
	(5.2)	(5.1)	(5.1)	(5.0)

Notes: This table reports labor market and demographic characteristics for individuals who had their first child between July 2004 and June 2006 in Quebec. Labor market outcomes are measured three to ten years after the first birth. Standard deviations for continuous variables are reported in parentheses.

4 Empirical Strategy

We identify the causal effects of the reform by comparing the outcomes of parents whose first child was born in the first six months following the reform (i.e., from January to June 2006) to the outcomes of parents whose first child was born in the last six months before the reform (i.e., from July to December 2005). We measure the outcomes in the same calendar year for both treated and control parents, limiting the extent to which business cycle fluctuations could affect our results. Treated and control parents may nevertheless differ in characteristics such as age and time since birth. To account for these differences, we subtract the difference in outcomes observed between parents whose child was born in the first six months of 2005 and those whose child was born in the last six months of 2004. Our identification strategy therefore assumes that, in the absence of the reform, the difference in outcomes for those whose first child was born early in 2006 versus late in 2005 mirrored the difference in outcomes for those whose first child was born early in 2005 versus late in 2004.

Formally, we estimate the average causal effects of the policy on the outcomes of men and women by estimating the following regression by Ordinary Least Squares:

$$\begin{aligned} y_i = \beta_0 &+ \beta_1 JanJun_i \times Around0506_i + \beta_2 JanJun_i \times Around0506_i \times Woman_i \\ &+ \beta_3 JanJun_i + \beta_4 Around0506_i + \beta_5 Woman_i + \beta_6 JanJun_i \times Woman_i \\ &+ \beta_7 Around0506_i \times Woman_i + \epsilon_i, \end{aligned} \tag{1}$$

where y_i is the outcome of interest for individual i, $Woman_i$ is an indicator for being a woman, $JanJun_i$ is a binary indicator equal to 1 if the first child of individual i was born between January and June regardless of the year (i.e, 2005 or 2006), $Around0506_i$ is a binary indicator equal to 1 if the first child of individual i was born around January 1st, 2006 (i.e., born between July 2005 and June 2006) and 0 if the child was born around January 1st, 2005 (i.e., born between July 2004 and June 2005), and ϵ_i is the error term.

In our main specification, we also include controls for age at first birth, immigration status, and income category two years prior to birth.[13] All standard errors are heteroskedasticity-robust and clustered at the family level. Our coefficients of interest are β_1 and β_2. While β_1 captures the average effect of the reform on men, β_2 captures the difference in the average effect of the reform between women and men, which can also be interpreted as the average effect of the reform on the gender gap in outcome y. Throughout the tables,

13. Age at first birth is controlled for using a continuous variable, immigration status using a binary indicator, and income category two years prior to birth using indicators for eleven categories of income, including one for missing values.

we report these two coefficients together with the average treatment effect of the reform on women, which we obtain by adding β_1 and β_2.

In Appendix E, we provide evidence in support of our identification strategy. We also show that our results are robust to alternative sample definitions and specifications definitions, including a regression discontinuity design.

5 Results

5.1 Effect on Parental Leave Take-up

We first document the effect on parental leave take-up. Figure 1 plots the share of parents who received any non-regular employment benefits during the first year following birth – our proxy for parental leave use – for children born from July 2005 to June 2006.

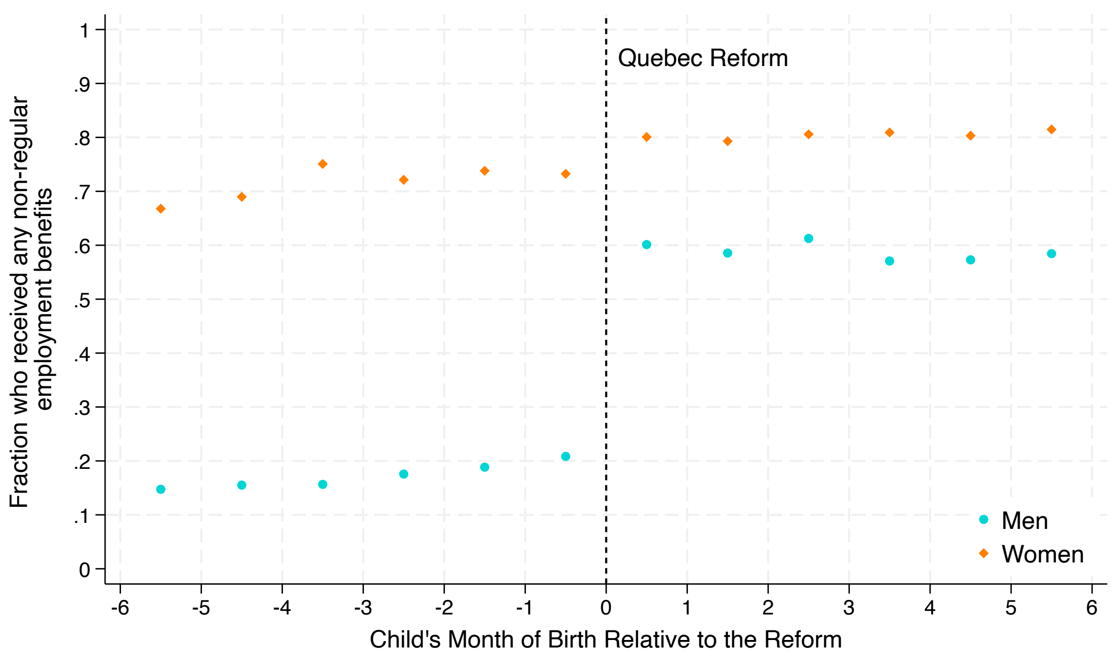

Figure 1: Parental Leave Take-Up by Child's Month of Birth Relative to the Reform

Notes: The figure reports the fraction of parents who received any non-regular employment benefits during the first year following birth – our proxy for parental leave use – grouped by the child's date of birth relative to the reform using monthly bins. The statistics are computed using data from the LAD.

Following the reform, fathers were substantially more likely to take parental leave. Take-up among fathers increased by 40 percentage points from about 20% to about 60% following the reform (see Appendix Table D.1 for the exact point estimates). We also find that the share of mothers taking leave increased but only by about 5 percentage points. This increase like results from the fact that the reform, on top of introducing a "daddy quota", also increased the coverage and generosity of paid parental leave, as we detail in Appendix A. Combined, the reform reduced the gender gap in the fraction taking any leave by two-thirds.[14]

Ideally, we would also like to estimate the effect of the reform on total leave duration, which would capture both the changes at the extensive margin reported above and potential changes at the intensive margin. However, leave duration is not directly observed in our data. Evidence from Patnaik (2019) nonetheless indicates that changes in total leave duration were also substantially larger for fathers than for mothers. Specifically, she finds that the reform increased fathers' total leave duration by approximately 160%, from 2 to 5.2 weeks, whereas mothers' leave duration increased by only about 4%, from 42.5 to 44.5 weeks.

5.2 Effects on Labor Market Outcomes

We next explore the effects of the reform on parents' labor market outcomes and in particular earnings, our main outcome of interest. We report in Figure 2 the treatment effects for each year relative to the birth of the first child, from four years prior to birth and up to ten years after. In Table 2 we also report the average treatment effects computed over years 3–10 after birth (corresponding to relative years 2–9 in the figures). All estimates are based on Model (1).

As shown in Figure 2(a), the reform significantly decreased fathers' employment earnings in the first year following birth, consistent with the large increase in parental leave use. We estimate a decrease in annual employment earnings among fathers of about $2,100, which is equivalent to 2.4 weeks of pay for the average full-time worker. Figure 2(a) also documents a significant decrease of about $1,600 in mothers' employment earnings in the first year following birth, which can be explained by the 5 percentage point increase in leave-taking among mothers we document earlier, as well as an possible increase in leave duration due to the increase in generosity. We can benchmark these treatment effects against the typical decline in earnings experienced by parents in the first year after childbirth (a.k.a. the child penalty): this comparison indicates that the reform increased the child penalty in the first year by 130% for fathers and 6% for mothers.[15]

14. Our estimates are smaller than the increase of leave-taking of 53 percentage points for fathers and 18 percentage points for mothers reported in Patnaik (2019).

15. We show in Appendix Figure D.2, the earnings trajectories around childbirth of mothers and fathers

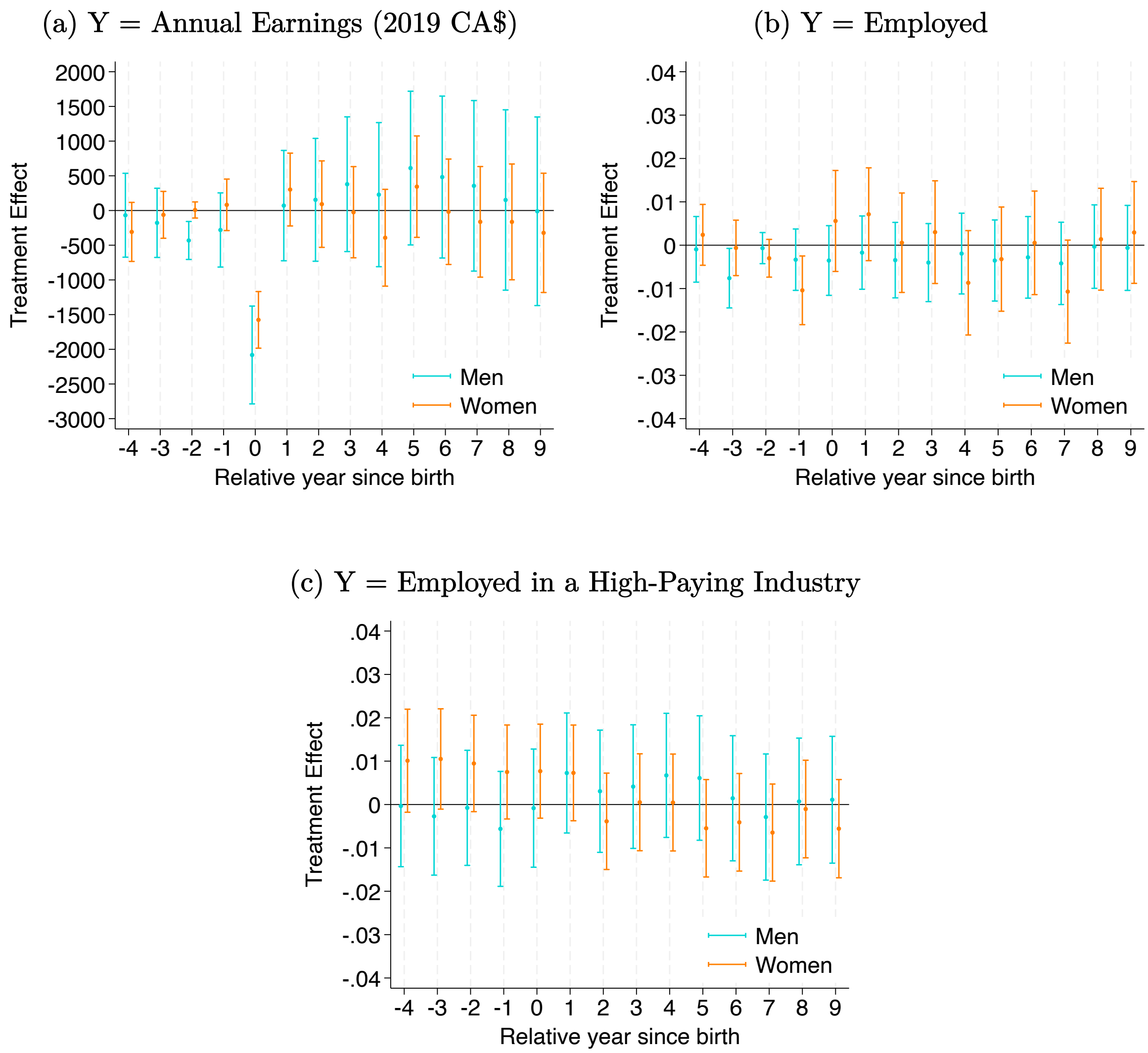

Figure 2: Dynamic Effects of the 2006 Reform on Labor Market Outcomes

Notes: The figure plots the effects of the 2006 reform on the labor market outcomes of mothers and fathers, with outcomes measured relative to the birth of their first child. Panel A reports the effect on annual employment earnings (in 2019 CA$), Panel B reports the effect on employment (defined as having positive annual employment earnings), and Panel C reports the effect on employment in a high-paying industry (defined as an industry in the top 25% of industries in 2006). Separate regressions are estimated for each relative year, with the outcome measured in that year. Coefficients for both mothers and fathers at each relative year are jointly estimated within a single estimation of Equation 1. Our sample is restricted to individuals who filed tax returns in the year before the birth and in all subsequent years. This implies that we have a balanced panel of 113,300 individuals (60,100 women and 53,200 men) for the years t-1 to t+9. We do not require individuals to file taxes in all years prior to the birth and the sample sizes for those years are therefore smaller and variable. The error bars represent the 95% confidence intervals obtained using Huber-white robust standard errors.

in our control group, which we use as our benchmark.

Table 2: Treatment Effects of the 2006 Reform on Labor Market Outcomes 3–10 Years After Birth

	Men	Women	Gap
Panel A: Average annual employment earnings (2019 CA$)			
Treatment effect	294	-81	-375
	(510)	(313)	(589)
Comparison group mean	56,300	32,100	-24,200
Panel B: Fraction of years employed			
Treatment effect	-0.0026	-0.0018	0.0008
	(0.0037)	(0.0046)	(0.0056)
Comparison mean	0.90	0.80	-0.10
Panel C: Fraction of years employed in a high-paying industry			
Treatment effect	0.0026	-0.0032	-0.0057
	(0.0064)	(0.0049)	(0.0076)
Comparison mean	0.27	0.16	-0.11
Obs.	53,200	60,100	113,300

Notes: The table reports effects of the 2006 reform on labor market outcomes from three to ten years following the birth of the first child, estimated from Equation (1) using the CEEDD. Panel A reports the effect on average earnings (in 2019 CA$), Panel B the effect on the fraction of years employed (i.e., with positive annual earnings), and Panel C on the fraction of years employed in high-paying industries (industries that fall within the top 25% of industries in 2006). Huber-white robust standard errors are reported in parentheses. Comparison means refer to averages for individuals with children born between July and Dec 2005 observed 3-10 years following the reform. The sample size (rounded to 100) refers to all individuals utilized in estimating equation 1. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Beyond the first year after birth, we find precise null effects of the reform on the earnings of either fathers and mothers (Figure 2(a) and Table 2(a)): the 95% CI for the three-to-ten year effect of the reform are \$$[-710, 1{,}290]$ for fathers and \$$[-690, 530]$ for mothers. In percent of average earnings in the comparison group, the intervals are $[-1.3, 2.3]$% and $[-2.2, 1.7]$% respectively. We also report in Table 2, the effect of the reform on the gender gap in earnings. Again, we find a precise null effect. Specifically, we can reject a decrease in the gender earnings gap greater than \$800. Given that the gender earnings gap over the same period was approximately \$24,200, this means that we can exclude a reduction in the gap greater than 3.3%.

Finally, Panels B and C of Table 2 and Figure 2 report the effects of the reform on two additional labor market outcomes: the likelihood of employment (proxied by positive annual earnings) and the likelihood of employment in a high-paying industry. Consistent with our findings on earnings, we observe precise null effects on these two outcomes for both fathers and mothers.

5.3 Robustness Checks

The estimates we report suggest no impact of the 2006 reform on fathers' and mothers' labor market outcomes beyond the first year after birth. In Appendix E, we present several validity checks to support our findings.

First, we discuss and provide evidence for the common trend assumption underlying our empirical strategy. Second, we examine the heterogeneity of the effects across the earnings distributions of fathers and mothers and across socio-economic groups. We find no effect across these dimensions, suggesting that the null average effects on earnings we report do not mask any compensatory effects. Finally, we estimate the effects of the reform using various alternative specifications (see Figures E.5, E.6, and E.7). We consistently find no effect of the reform, regardless of the bandwidth choice or whether we include a donut hole. The results are also unchanged when using alternative identification strategies, including a local difference-in-differences design with the rest of Canada as a control group and a regression discontinuity design. Our findings are further robust to the inclusion of individual fixed effects and to varying sample restrictions. Overall, the absence of a treatment effect beyond the first year following the birth of the child on either mothers' or fathers' labor market outcomes is a robust result.

Online Appendix C also reports results from replicating specifications found in the prior literature and summarized in the same appendix. These replication exercises confirm the absence of substantial effects of the reform on mothers' and fathers' employment, while results on earnings differ across specifications, as discussed in the Introduction.

5.4 Total Household Income

As a complement to our analysis of individual labor market outcomes, we also report estimates of the effects of the reform on total household income in Appendix Figure D.1. The figure presents effects on total household income as well as on its four main components, all measured at the household level: earnings; other market income (e.g., capital income and self-employment income); parental benefits (proxied by non-regular employment benefits); and other government transfers (which includes unemployment insurance, social assistance, child benefits, and other programs).

Consistent with our individual-level results, Panel (a) of the appendix figure shows a decline in household earnings in the first year following birth. Panel (c) shows an increase in parental benefits received from the government in the same period, consistent with increased leave-taking and the higher generosity of the program.[16] We find no meaningful changes in other market income (Panel (b)) or in other government transfers (Panel (d)) immediately after birth. Taken together, the increase in benefits slightly exceeds the earnings loss, implying a small increase in total household income in the first year following birth, however, this effect is not statistically significant (p-value = 0.15).

Beyond the first year after birth, we find no statistically significant effects of the reform on any component of household income or on total household income. Overall, the household income results are consistent with our individual-level findings and indicate no meaningful long-run effects of the reform on family resources.

5.5 Mechanisms

Paternity leave policies hold promise to reduce the child penalty by shifting traditional social norms around care-giving. One reason the 2006 Quebec reform might have had no effect on labor market outcomes is that the reform did not succeed in altering these norms. We explore this possibility in what follows.

Specifically, using information provided in the LAD, we study how the reform impacted several outcomes related to gender norms around care-giving. First, we examine the effects of the reform on sharing of custody in the event of separation. If the reform did shift traditional norms, we would expect to see an increase in the fraction of fathers having shared or full-time custody of children in the event of separation. To proxy for shared custody, we use whether individuals receive child benefits when separated, which indicate that they have at least shared custody of children.[17] Second, inspired by the work of Andresen and Nix (2024), we analyze the effects of the reform on how parental leave for the second child was shared.

16. Appendix D.1 documents the corresponding individual-level changes in non-regular employment benefits received.

17. When parents live together, child benefits are typically paid to the mother. After separation, benefits are paid to the parent with primary custody, and in cases of shared custody both parents receive a portion. Thus, fathers' receipt of child benefits indicates at least shared custody. Consistent with this, only about 2% of fathers receive child benefits in two-parent households, compared with approximately 21% among separated fathers not living with a partner.

We propose that a shift in gender norms would increase the share of parental leave for the second child taken by fathers. Finally, we also study the effects of the reform on family dynamics, namely separation and fertility. We do not have any directional hypothesis for these outcomes, but do suspect that equitable sharing of care-giving responsibilities might be reflected in these outcomes.

Table 3 reports the estimates for these four outcomes. None of them was affected by the reform. In particular, we don't observe that the share of fathers receiving child benefits upon separation changed significantly following the reform (Panel A) nor do we see that the treatment by the reform when the first child was born affected the amount of leave that fathers took in later periods (Panel B). The reform also seems to have had no effect on the likelihood to separate (Panel C) or to have an additional child (Panel D). All these null results together suggests that the 2006 Quebec reform failed to alter traditional social norms around care-giving and family arrangements. This lack of change in social norms may explain the absence of effects on earnings documented in Section 5.2.

Table 3: Treatment Effects of the 2006 Reform on
Social Norms and Family Outcomes

	Men	Women	Gap	Obs.
Panel A: Ever received some child benefits conditional on separation				
Treatment effect	0.0383	-0.0105	-0.0488	8,300
	(0.0292)	(0.0198)	(0.0350)	
Comparison mean	0.2293	0.8659	0.6366	
Panel B: Amount of non-reg. employment benefits received for the 2nd child				
Treatment effect	-130	324	454	18,800
	(208)	(376)	(432.1)	
Comparison mean	2,800	11,500	8,700	
Panel C: Ever been divorced or separated				
Treatment effect	0.0136	-0.0146	-0.0282	25,300
	(0.0165)	(0.0161)	(0.0215)	
Comparison mean	0.2935	0.3714	0.0780	
Panel D: Had a second child				
Treatment effect	0.0008	0.00212	0.0013	25,300
	(0.0159)	(0.0149)	(0.0203)	
Comparison mean	0.7397	0.7521	-0.0124	

Notes: The table reports the effects of the 2006 reform on several outcomes related to social norms and family dynamics, estimated from Equation (1) using the LAD. Panel A, C, and D are linear probability models using as dependent variables indicators of receiving child benefits conditional on separation (Panel A), divorce or separation (Panel C) and having a second child (Panel D) within 10 years of birth. The dependent variable in Panel B is the amount of non-regular employment benefits received for the second child, a proxy for the duration of leave taken. Huber-white robust standard errors in parentheses. Comparison means refer to averages for individuals with children born between July and Dec 2005 observed 3-10 years following the reform. The sample size (rounded to 100) refers to all individuals utilized in estimating equation 1 and varies across specifications due to the applicable population. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

6 Conclusion

In conclusion, this study shows that the Quebec 2006 parental leave reform, which drastically increased paternity leave take-up, had no significant effect on either mothers' or father's medium or long-run labor market outcomes, contrary to findings reported in previous studies

(Patnaik (2019), Dunatchik and Özcan (2021), Choi, Margolis, and Holm (2025)). This aligns with recent research from Scandinavia, such as Andresen and Nix (2024), indicating that a few extra weeks of paternity leave are insufficient to change deep-rooted gender norms around childcare and labor market participation.

It is important to note that our findings are based on behavioral changes of parents whose children were born around the time the reform was implemented. The immediate effects we estimate may not fully extrapolate to parents of children born further from the reform period – especially if the reform gradually and indirectly affected norms in the population.[18]

Moreover, paternity leave may still have value for women by promoting more equitable care-giving responsibilities in the early weeks of parenthood. Such a shift could contribute to improvements in women's physical and mental health postpartum. To maximize this benefit, future policy reforms might focus on making paternity leave more effective by ensuring that it is taken close to the time of childbirth, as suggested by Andresen and Nix (2024).

Overall, while this study raises questions about the effectiveness of paternity leave in achieving labor market equality, it underscores the importance of considering how parental leave policies are structured to potentially promote broader well-being within families, even if they do not directly address the labor market disparities between genders.

18. For evidence illustrating such mechanisms, see the work on peer effects in paternity leave taking by Dahl, Løken, and Mogstad (2014) and Diallo and Lange (2025).

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Supplementary Material

A Details regarding the 2006 Quebec Reform

On January 1, 2006, the Quebec government launched its own paid parental leave program, the Quebec Parental Insurance Plan (QPIP), diverging from the federal Employment Insurance program. This reform brought several changes which we summarize in Table A.1:

1. Eligibility Expansion: To receive benefits under the new program, individuals need to have received at least $2,000 in employment earnings during the past year instead of the 600 hours required under the federal program. The new calculation also includes self-employment earnings which were previously excluded under the federal program.

2. Increased Benefits: The QPIP offers an additional five weeks of parental leave compared to the previous federal program. The replacement rate also increased to 70% of earnings for the first 30 weeks, with a maximum amount of $767. The remaining 15 weeks are compensated at 55% earnings with a maximum amount of $602. In comparison, under the federal program, individuals received 55% of earnings, up to a maximum of $412 a week.

3. Daddy Quota: Of the 55 weeks of parental leave, 5 weeks are exclusively reserved for fathers. No earmarked weeks for fathers existed under the federal system at that time.

4. Flexibility: The QPIP allows parents to choose, in place of the "Basic Plan", a "Special Plan" which covers fewer weeks but at a higher replacement rate. Specifically, the "Special Plan" covers 43 weeks of parental leave instead of the 55 weeks under the "Basic Plan" at a replacement rate of 75% of earnings, with a weekly maximum amount of $822.

Table A.1: Federal Employment Insurance (EI) program
and Quebec Parental Insurance Plan (QPIP)

	2005 Federal EI	2006 QPIP	
		Basic Plan	Special Plan
Total number of weeks	50	55	43
Reserved for mothers	15	18	15
Reserved for fathers	0	5	3
Shareable	35	32	25
Replacement rate			
Baseline replacement rate	55%	70%–55%	75%
Maximum weekly amount	$412	$767–$602	$822
Eligibility			
Work requirement in past year	600 hrs of work	$2,000 of earnings	
Self-employed?	Excluded	Included	

Notes: The table summarizes the characteristics of the paid parental leave schemes under the federal Employment Insurance (EI) program in place in Quebec in 2005 and under the Quebec Parental Insurance Plan (QPIP) introduced in 2006 in Quebec. The reported weekly amounts refer to the 2006 program rules and are expressed in 2006 Canadian dollars.

B Data Appendix

B.1 Outcome Definitions

We construct the following outcomes from the CEEDD:

- Annual employment earnings (2019 CA$): All earnings received from formal employment during a year, excluding tips, gratuities, and self-employment income. We exclude tips, gratuities, and self-employment income since they can be misreported. The variable is expressed before deductions. We winsorize the variable at the 99th percentiles by year and gender to mitigate the effects of outliers. The variable is expressed in 2019 Canadian Dollars.

- Average annual employment earnings 3–10 years after birth: Average of the "Annual employment earnings" variable from three to ten years after birth.

- Employed: Binary indicator for whether the individual reports any positive employment income during the year.

- Fraction of years employed years 3–10 years after birth: Average of the indicator "Employed" from three to ten years after birth.

- Employed in a high-paying industry: Binary indicator equal to one if the industry of the individual's primary employment falls within the top 25% of highest-paying industries. We classify industries according to the average income earned by the employees working in each industry in 2006. Industries are defined using 3-digit NAICS codes. The variable takes the value of zero for individuals who do not work.

- Fraction of years employed in a high-paying industry 3–10 years after birth: Average of the indicator "Employed in a high-paying industry" from three to ten years after birth.

We also construct the following outcomes of interest from the LAD:

- Amount of non-regular employment benefits received (2019 CA$): Total amount of non-regular employment benefits received during the year following the birth of the first child. Non-regular employment benefits include parental leave, sickness, and caregiving benefits. The variable is expressed in 2019 Canadian Dollars.

- Any non-regular employment benefits received during the first year following childbirth: Binary indicator equal to one if the individual has received any non-regular employment benefits during the year following childbirth.

- Ever been divorced or separated: Binary indicator equal to one if the individual is ever observed as a lone parent or an individual tax filer across the period from three to ten years after the first birth.
- Ever received some child benefits conditional on separation: Binary indicator equal to one if the individual ever received some child benefits during the years he/she is a lone parent or an individual tax filer. Child benefits are attributed to both parents in case of shared custody or to the primary caregiver in case of sole custody.
- Had a second child: Binary indicator equal to one if the individual is ever observed with a second child across the period from three to ten years after the first birth.
- Amount of non-regular employment benefits received for the second child (in 2019 CA$): Total amount of non-regular employment benefits received during the year of birth of the second child.

B.2 Definition of Relative Years

Our outcomes are measured relative to childbirth. Our main group of parents are parents whose children are born between July 2005 and June 2006. We consider the year 2006 as the first year relative to childbirth for them, 2007 as the second year, and so forth until 2015, which we consider the tenth year. Similarly, for parents in our control group, whose first child was born between July 2004 and June 2005, we assign 2005 as the first year, 2006 as the second year, and so forth until 2014, which we consider the tenth year.

B.3 Identification of Individuals' First Child

Since we are interested in the effects of the reform on parents' labor market outcomes following the birth of their first child, we must determine the first child ever born for each individual. We face two challenges in doing so. First, the birthdates of children within each tax return reflect the birthdates of the individual's current dependents, implying that the oldest child listed on a given tax return is not necessarily the individual's firstborn child. Second, parents do not always immediately declare their children upon birth.

We consider an individual's first child to be the first child declared across all tax returns, regardless of when the child is declared. To address concerns that the child we consider might not be the individual's own child, we show in Appendix E.3 that our results are similar if we restrict our sample to individuals who declare the first child within two years after birth.

C Literature on the Quebec 2006 Parental Leave Reform and Parental Labor Market Outcomes

There are 4 closely related papers to our study. These papers are summarized in Tables C.1 and C.2 below.

We also replicated these studies using the data at our disposal mimicking their empirical settings as closely as possible based on the information provided in the papers (including empirical strategy, sample selection and time horizon). The main treatment effects from our replications together with the corresponding estimates reported in the papers can be found in Tables C.3 to C.6.

Table C.1: Summary of Studies Examining the Impact of the 2006 Quebec Reform on Labor Market Outcomes – Panel A

	This paper	**Patnaik (Journal of Labor economics, 2019)**	**Dunatchik and Özcan (Journal of European Social Policy, 2021)**	**Choi et al. (Demography, 2025)**
Data	Statistics Canada T1 Family File (T1FF) data 2001–2015, obtained from the Canadian Employer–Employee Dynamics Database (CEEDD). + Longitudinal Administrative Data Bank for family outcomes.	Canada's General Social Survey (GSS) 2005 and 2010 waves. + Employment Insurance Coverage Survey 2002–2010 for leave-taking outcomes.	Survey of Labour Income Dynamics (SLID) 2003–2011.	Statistics Canada T1 Family File (T1FF) data 2003–2016. Exact extract version not specified.
Empirical strategy	Difference-in-differences: compares parents with first birth in Quebec just after versus just before the reform (Jan.–June 2006 vs. July–Dec. 2005), differenced against the same comparison in the previous year (Jan.–June 2005 vs. July–Dec. 2004). Extensive robustness analyses.	Difference-in-differences: compares parents of with youngest child is aged 1–3 in Quebec vs. the rest of Canada, before (2005) and after (2010) the reform. A triple-differences design adds parents of children aged 5–8 as an additional control group.	Difference-in-differences: compares parents in Quebec vs. the rest of Canada, before (2003–2005) and after (2007–2011) the reform. Age restriction on children depends on year of interview to ensure parents observed after (2007–2011) have a child born after the reform.	Difference-in-differences comparing parents with first births in Quebec just after versus just before the reform (2006 vs. 2005), differenced against the same comparison in the rest of Canada. Two-stage least squares (2SLS) is used to estimate the effects of fathers' leave take-up.
Treated group	Parents who had their first child between January and July 2006 in Quebec.	Parents who live in Quebec and have a child born in 2006–2009.	Parents who live in Quebec and have a child born 2006–2011.	Parents who had their first child in 2006 in Quebec.
Main sample restrictions	Limited to individuals who have continuously filed taxes from 1 year before to 10 years after the first birth.	No restrictions beyond children's age and date of birth.	Limited to married or cohabiting mothers at the time of interview.	Limited to parents aged 21–45 and married or cohabiting at the time of birth, both with positive earnings one year prior to birth, who have continuously filed taxes from 1 year before to 10 years after birth.

Table C.2: Summary of Studies Examining the Impact of the 2006 Quebec Reform on Labor Market Outcomes – Panel B

	This paper	**Patnaik (Journal of Labor economics, 2019)**	**Dunatchik and Özcan (Journal of European Social Policy, 2021)**	**Choi et al. (Demography, 2025)**
Sample sizes	Total: 60,100 mothers and 53,200 fathers. Treatment group: 13,400 fathers and 15,100 mothers.	Total: 1,939 mothers and 1,596 fathers. Treatment group (based on our own calculation): $\sim$ 110 mothers and 100 fathers.	Total: 6,448 mothers, no father. Treatment group (accounting for SLID panel structure): $\sim$ 400-600 mothers.	Total: 145,900 mothers, and 136,441 fathers. Treatment group: 20,680 mothers and 19,670 fathers. Two subsamples: high-earning couples (3/4 of sample) and low-earning couples (1/4 of sample).
Outcome definitions (labor market outcomes only)	Annual earnings, employment defined as reporting positive earnings during the year, employment in a high-paying industry.	Time spent in paid work, employment, usual weekly hours, weeks worked in the previous year, full-time employment (35+ hours/week).	Labor force participation, full-time and part-time employment, unemployment; and hourly wages.	Annual earnings (expressed as log changes relative to the pre-birth year), employment defined as reporting positive earnings during the year.
When outcomes are measured	From the birth year through ten years after birth; results are reported annually.	When the child is aged 1–3 years.	When the child is aged 0–5 years.	From the birth year through ten years after the first birth, grouped as: birth year; 1 year after; 2–4 years after; 5–7 years after; and 8–10 years after birth.
Main concerns or limitations	Can't measure hours worked directly.	Double- and triple- differences based on very small sample sizes leading to very small treatment groups ($\sim$ 100 treated mothers and fathers).	1. Sample selection induces uncontrolled variation in age-of-child distribution in treatment and control groups, known to be important drivers of outcomes. 2. The authors do not report clustering to account for multiple observations on the same mothers. Standard errors are therefore very likely too small.	Outcome variables are measured multiple times but not clustered at the individual level. For outcomes 2-4, 5-7, or 8-10 years after the birth, reported standard errors are possibly up to 1.73 ($=\sqrt{3}$) times too small.

Table C.3: Replication of Patnaik (2019) Using CEEDD

	Estimates using the CEEDD		Estimates in Patnaik (2019)	
	Whether employed	Annual earnings	Whether employed	Time in paid work
Panel A: Treatment effects for mothers				
Double-differences model	0.017***	-532.9*	0.046*	60.1***
	(0.002)	(298.7)	(p=0.09)	(p=0.00)
	[1,091,100]	[1,091,100]	[1,115]	[1,115]
Triple-differences model	0.000	-1,065***	-0.003	35.4
	(0.002)	(216.5)	(p=0.92)	(p=0.23)
	[2,135,000]	[2,135,000]	[1,939]	[1,939]
Panel B: Treatment effects for fathers				
Double-differences model	-0.007***	-3,535**	0.002	-43.3
	(0.002)	(1,372)	(p=0.90)	(p=0.16)
	[1,070,600]	[1,070,600]	[988]	[988]
Triple-differences model	0.003**	-1,976***	0.017	-178.0***
	(0.001)	(142.1)	(p=0.44)	(p=0.00)
	[2,054,800]	[2,054,800]	[1,596]	[1,596]

Notes: Columns under "Estimates using the CEEDD" report replication results with standard errors in parentheses. Columns under "Estimates in Patnaik (2019)" reproduce estimates from Table 10 of Patnaik (2019), which reports p-values in parentheses instead of standard errors. Sample sizes are reported in square brackets. Earnings are expressed in 2019 Canadian dollars. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.4: Replication of Dunatchik and Özcan (2021) Using CEEDD

	Estimates using the CEEDD		Estimates in Dunatchik and Özcan (2021)		
	Whether employed	Annual earnings	Labor force participation	Hourly wage	Full-time employment
Treatment effects for mothers					
2007	0.006***	-2,173***	0.00	0.03	-0.01
	(0.0017)	(99.10)	(0.05)	(0.05)	(0.05)
2008	0.006***	-1,909***	0.08*	0.02	0.14**
	(0.0014)	(85.62)	(0.04)	(0.04)	(0.04)
2009	0.007***	-1,969***	0.07*	0.03	0.14**
	(0.0013)	(78.84)	(0.03)	(0.04)	(0.04)
2010	0.009***	-1,644***	0.06	0.06	0.02
	(0.0012)	(74.24)	(0.03)	(0.04)	(0.04)
2011	0.018***	-1,141***	0.02	0.06	-0.01
	(0.0012)	(70.83)	(0.03)	(0.03)	(0.04)
Sample size	5,183,900	5,183,900	6,448	4,663	5,306

Notes: Columns under "Estimates using the CEEDD" report replication results with standard errors in parentheses. Columns under "Estimates in Dunatchik and Özcan (2021)" reproduce estimates from Dunatchik and Özcan (2021). Labor force participation estimates correspond to Model 3 of Table 5, hourly wage estimates correspond to Model 3 of Table 9, and full-time employment estimates correspond Model 3 of Table 6. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.5: Replication of Choi et al. (2025) Using CEEDD
Sample of Higher-Earning Women

	Estimates using the CEEDD		Estimates in Choi et al. (2025)	
	Employment	Log(earnings)	Employment	Log(earnings)
Panel A: Treatment effects for mothers				
Year of first birth	0.004	−0.125***	0.004	−0.108**
	(0.003)	(0.015)	(0.003)	(0.014)
1 year after first birth	0.005*	0.0108	0.004	0.019
	(0.003)	(0.015)	(0.003)	(0.014)
2–4 years after first birth	−0.002	−0.001	0.002	0.039**
	(0.004)	(0.011)	(0.002)	(0.008)
5–7 years after first birth	-0.005	0.018	−0.001	0.037**
	(0.004)	(0.012)	(0.003)	(0.008)
8–10 years after first birth	-0.002	−0.008	−0.002	0.019**
	(0.004)	(0.013)	(0.001)	(0.007)
Panel B: Treatment effects for fathers				
Year of first birth	−0.001	−0.044***	−0.002	−0.045**
	(0.001)	(0.006)	(0.001)	(0.005)
1 year after first birth	0.0003	−0.003	0.001	−0.013*
	(0.002)	(0.007)	(0.002)	(0.006)
2–4 years after first birth	0.003	0.018**	0.002	0.016**
	(0.002)	(0.007)	(0.001)	(0.006)
5–7 years after first birth	0.002	0.011	0.002	0.006
	(0.003)	(0.009)	(0.002)	(0.005)
8–10 years after first birth	0.002	0.017*	0.002	0.019**
	(0.003)	(0.010)	(0.002)	(0.005)

Notes: Columns under "Estimates using the CEEDD" report replication results by event time relative to first birth. Columns under "Estimates in Choi et al. (2025)" reproduce estimates from Table 2 of Choi, Margolis, and Holm (2025). In both cases, the sample is limited to couples with high-earning mothers prior to birth. "Change in earnings" is defined as the log of earnings at time t minus the log of earnings in the pre-birth year. Standard errors are reported in parentheses. The baseline sample consists of 92,700 mothers and 90,100 fathers in the CEEDD estimates, and 107,590 mothers and 102,510 fathers in Choi, Margolis, and Holm (2025). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table C.6: Replication of Choi et al. (2025) Using CEEDD
Sample of Low-Earning Women

	Estimates using the CEEDD		Estimates in Choi et al. (2025)	
	Employment	Log(earnings)	Employment	Log(earnings)
Panel A: Treatment effects for mothers				
Year of first birth	0.026***	−0.293***	−0.011	−0.096*
	(0.008)	(0.049)	(0.010)	(0.040)
1 year after first birth	0.031***	0.042	0.034**	−0.011
	(0.009)	(0.052)	(0.010)	(0.041)
2–4 years after first birth	0.015**	0.040	0.016**	0.023
	(0.008)	(0.049)	(0.006)	(0.023)
5–7 years after first birth	0.008	0.063	0.014*	0.016
	(0.008)	(0.053)	(0.006)	(0.023)
8–10 years after first birth	0.0002	−0.038	−0.002	0.010
	(0.008)	(0.055)	(0.006)	(0.022)
Panel B: Treatment effects for fathers				
Year of first birth	−0.005	−0.078**	−0.001	−0.060**
	(0.005)	(0.032)	(0.004)	(0.017)
1 year after first birth	−0.001	−0.042	−0.002	−0.021
	(0.005)	(0.036)	(0.005)	(0.020)
2–4 years after first birth	0.001	0.002	−0.001	0.023
	(0.005)	(0.036)	(0.004)	(0.013)
5–7 years after first birth	0.001	0.018	−0.004	−0.014
	(0.006)	(0.039)	(0.004)	(0.013)
8–10 years after first birth	0.007	0.023	−0.003	−0.006
	(0.006)	(0.041)	(0.004)	(0.014)

Notes: Columns under "Estimates using the CEEDD" report replication results by event time relative to first birth. Columns under "Estimates in Choi et al. (2025)" reproduce estimates from Table 3 of Choi, Margolis, and Holm (2025). In both cases, the sample is limited to couples with low-earning mothers prior to birth. "Change in earnings" is defined as the log of earnings at time t minus the log of earnings in the pre-birth year. Standard errors are reported in parentheses. The baseline sample consists of 48,500 mothers and 45,800 fathers in the CEEDD estimates, and 37,760 mothers and 33,940 fathers in Choi, Margolis, and Holm (2025). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D Additional Tables and Figures

D.1 Non-regular Employment Benefits

Table D.1: Treatment Effects of the 2006 Reform on Parental Leave Take-up

	Men	Women	Gap
Panel A: Received any non-regular employment benefits			
Treatment effect	0.399***	0.052***	-0.346***
	(0.015)	(0.012)	(0.019)
Comparison mean	0.171	0.716	0.545
Panel B: Amount of non-regular employment benefits received (2019 CA$)			
Treatment effect	1,835***	5,142***	3,307***
	(154)	(227)	(279)
Comparison mean	800	9,100	8,300
Obs.	11,800	13,500	25,300

Notes: The table reports the effects of the 2006 reform on non-regular employment benefits received following the birth of the first child, a proxy for parental leave use, estimated from Equation (1) using the LAD. Panel A reports the effects on the share of individuals who received any non-regular employment benefits, and Panel B reports the effects on the amount of non-regular employment benefits received, expressed in 2019 CA$. Huber-white robust standard errors are reported in parentheses. Sample size is rounded to 100. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

D.2 Treatment Effects on Household Income

As described in Section 5.4, we also estimate the effects of the reform on total household income and its key components, measured in 2019 Canadian dollars. Figure D.1 presents the results. Panel (a) shows the treatment effects on total household employment earnings; Panel (b) displays the effects on other market income, including capital income, self-employment income, and related sources; Panel (c) presents the effects on total non-regular employment benefits, which we use as a proxy for parental benefits; and Panel (d) shows the effects on other government transfers, including unemployment insurance, social assistance, child benefits, and other programs. Together, these four components sum to the effect on total household income shown in Panel (e).

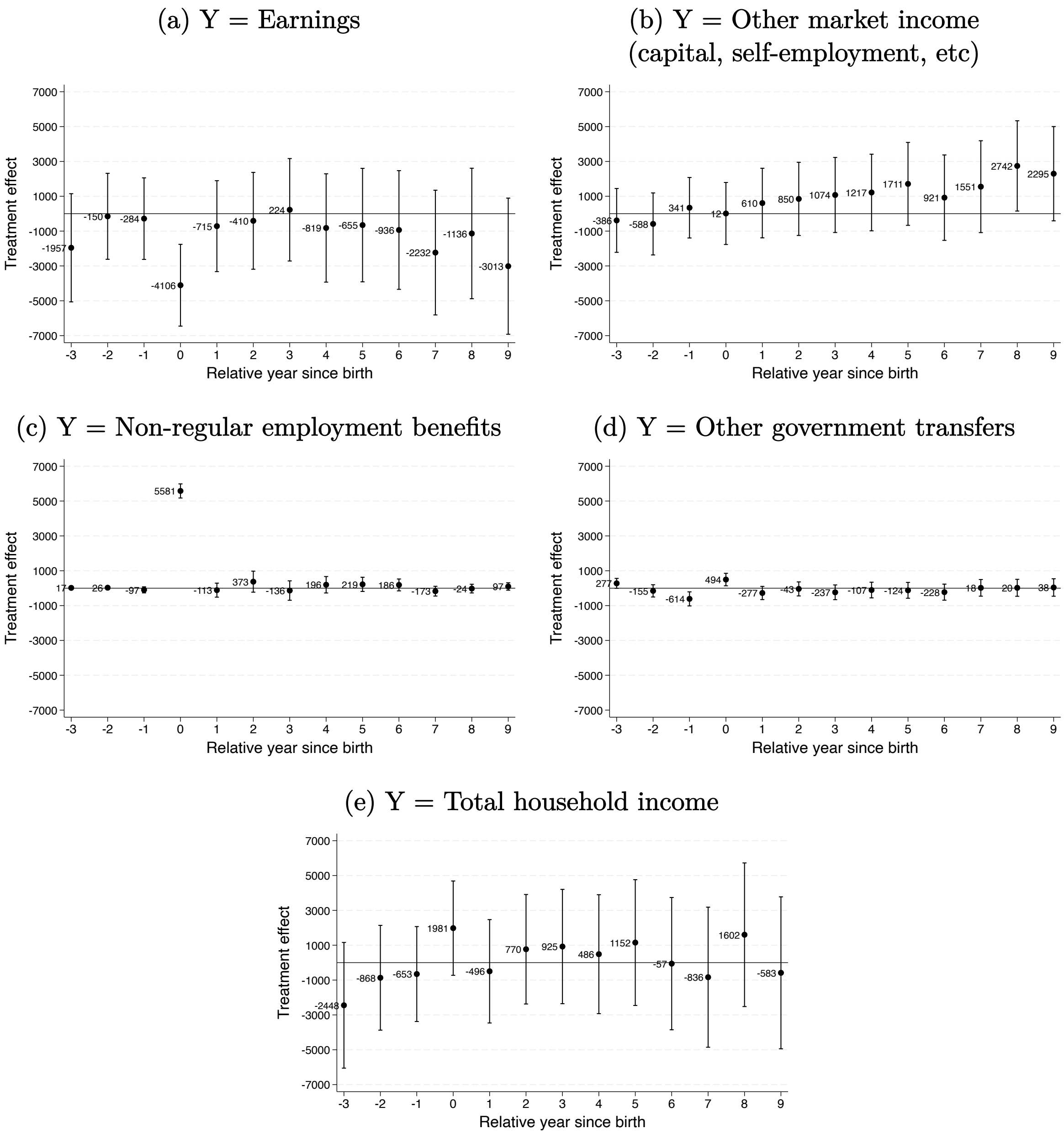

Figure D.1: Dynamic Effects of the 2006 Reform on Household Income

Notes: The figure plots the effects of the 2006 reform on total household income and its components, with outcomes measured relative to the birth of the first child. Outcomes are derived from the LAD. Separate regressions (Equation (1)) are estimated for each relative year, with the outcome measured in that year. To avoid complications arising from partnership dissolution and formation over time, and to ensure a single observation per family, we focus on mothers' families only and define family income based on the current family the mother lives in, rather than the family structure at the time of birth. Error bars represent 95% confidence intervals constructed using Huber–White robust standard errors.

D.3 Child Penalty in Earnings

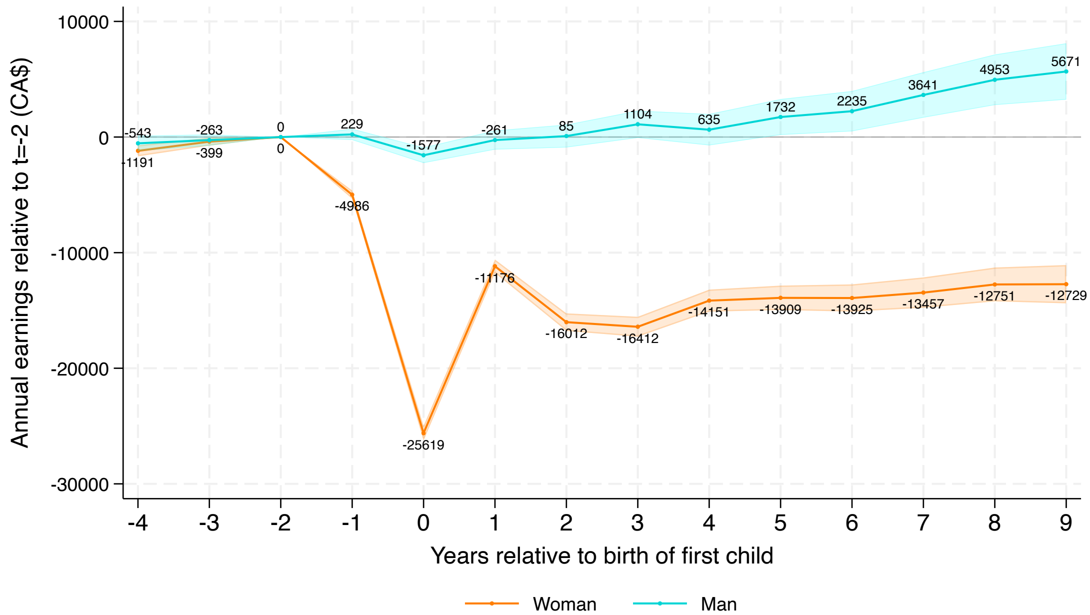

Figure D.2: Child Penalty in Earnings

Notes: The figure plots the relative-time coefficients obtained from regressions of annual earnings on relative-time indicators and age dummies, with event time -2 omitted and thus serving as the reference category. Relative time is defined as years relative to the birth of the first child. Regressions are estimated separately for men and women. Shaded areas represent 95% confidence intervals based on robust standard errors. The sample is restricted to mothers and fathers whose first child was born in Quebec in January or February 2005.

E Validity Checks

E.1 Similarity in Earnings Patterns

In Figures E.1 and E.2, we plot raw means of our main outcome of interest – average annual earnings 3–10 years after birth – for mothers and fathers, grouped by the timing of first childbirth. Figure E.1 shows mean outcomes for parents whose first child was born between July 2005 and June 2006 and for those whose first child was born between July 2004 and June 2005, corresponding to the two groups used in our difference-in-differences strategy. Figure E.2 zooms out and presents the same statistics for births between 2001 and 2006, grouped into six-month windows. In the figure, the four data points in 2005 and 2006 correspond to the ones used in our difference-in-differences analysis, while the earlier years can be used to analysis broader trends in outcomes.

Across both figures, we find reassuring evidence in support of the common trends assumption. First, as shown in Figure E.2, the difference in earnings between parents whose first child was born early in the year and those whose child was born later in the preceding year remains small and stable over time for both mothers and fathers. Second, zooming in, earnings patterns across the month of birth of the child are similar for children born in July–December 2004 and July–December 2005. Together, these findings indicate that seasonal patterns in the timing of childbirth are stable over time and that there are no differential earnings trends prior to the reform across the groups used in our difference-in-differences strategy.

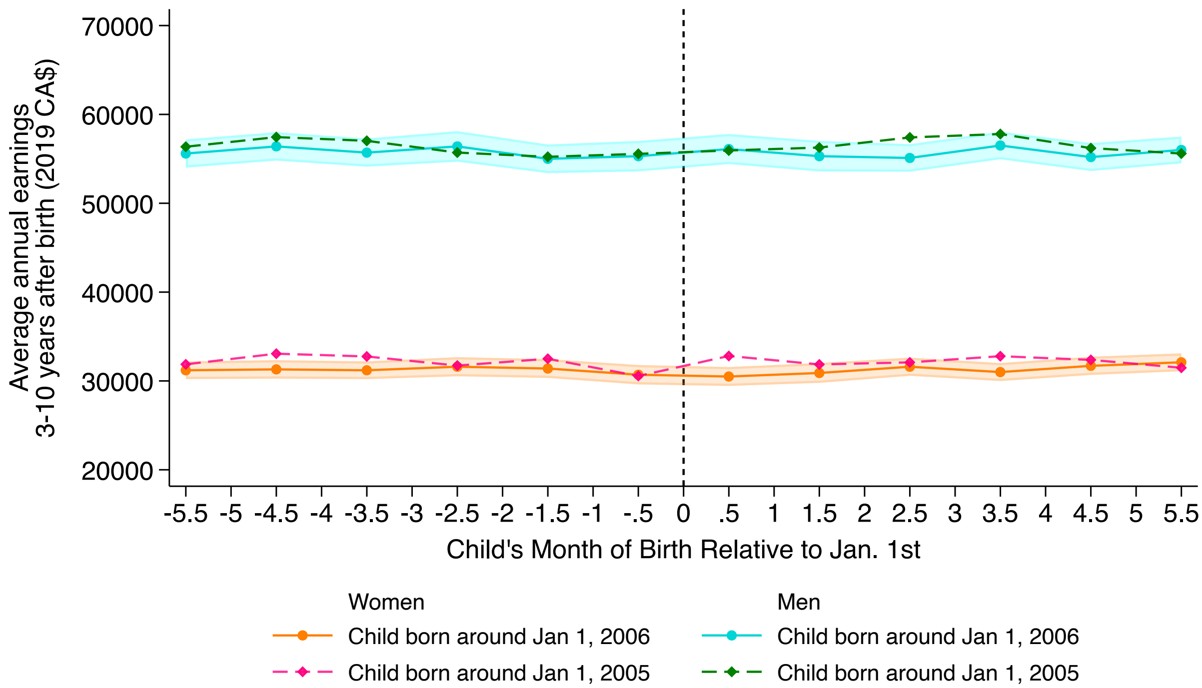

Figure E.1: Average Annual Earnings 3–10 Years After Birth, by Month of First Childbirth, in a Six-Month Window Around the Reform

Notes: The figure plots the raw means of our main outcome of interest – average annual earnings over years 3–10 after birth – for mothers and fathers grouped by their first child's date of birth using monthly bins. Dashed lines show mean outcomes for parents of children born between July 2004 and June 2005, while solid lines show mean outcomes for parents of children born between July 2005 and June 2006. Shaded areas indicate 95% confidence intervals for the solid lines, illustrating the precision of these estimates. The statistics are computed using data from the CEEDD.

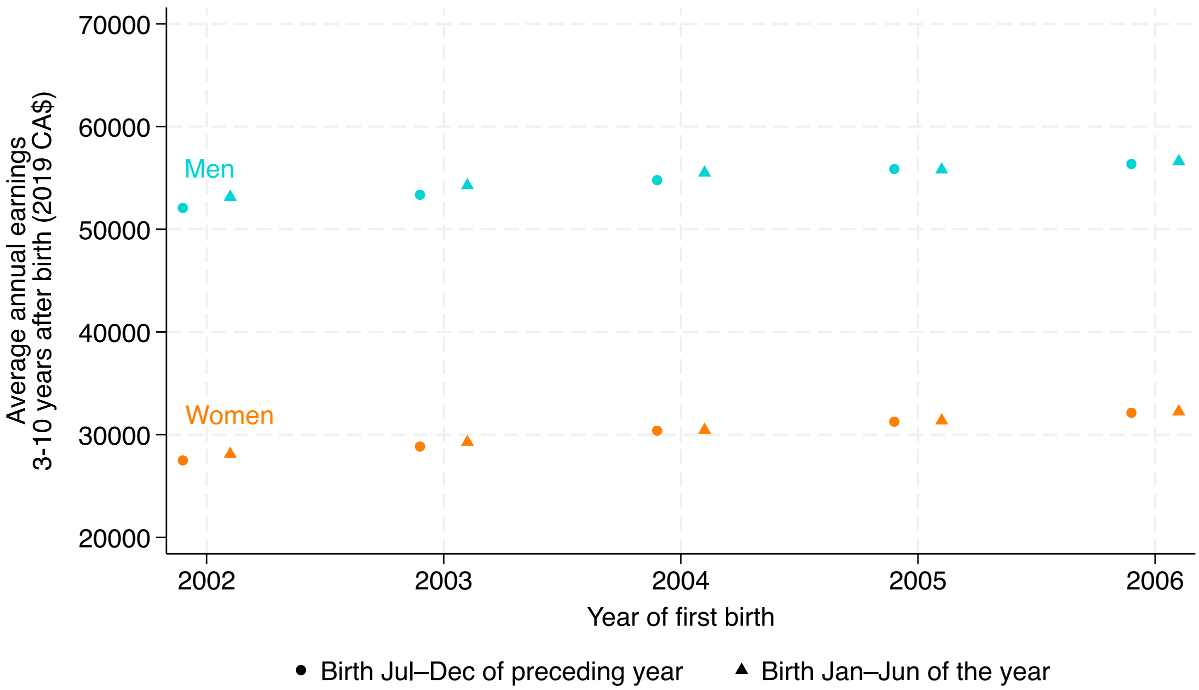

Figure E.2: Average Annual Earnings 3–10 Years After Birth by Date of First Childbirth — Births Grouped in Six-Month Windows from 2001 to 2006

Notes: The figure plots the raw means of our main outcome of interest – average annual earnings over years 3–10 after birth – for mothers and fathers grouped by their first child's date of birth using six-months windows. Triangles denote parents whose first child was born between January and June of the year shown on the x-axis, while circles denote parents whose first child was born between July and December of the preceding year. The statistics are computed using data from the CEEDD.

E.2 Heterogeneity

The effects reported in Section 5.2 suggest no impact of the 2006 reform on fathers' and mothers' labor market outcomes beyond the first year after birth, *on average*. In this section, we examine the heterogeneity of the effects to uncover any compensatory effects that might be hidden within the null *average* effects.

We first investigate, in Figure E.3, the effects of the reform on the earnings distribution of fathers and mothers. The results indicate no effect of the reform at any point in the earnings distribution for either group.

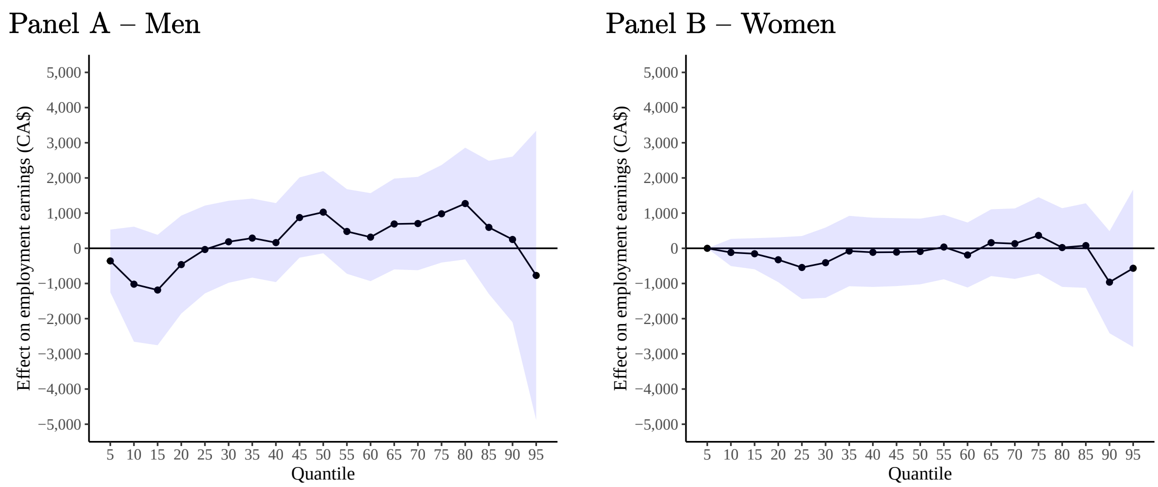

Figure E.3: Treatment Effects Across the Earnings Distribution

Notes: The figure reports the effects of the 2006 reform on the distribution of employment earnings 3–10 years after birth of fathers (Panel A) and mothers (Panel B). The treatment effects are identified from 19 quantile regressions using our main difference-in-differences model. Each point represents a separate quantile regression. The sample of men includes 53,200 individuals and the sample of women includes 60,100 individuals. The blue ribbons represent the 95% confidence intervals around the point estimates, calculated using Huber-white robust standard errors. The data source is the CEEDD.

In addition, in Figure E.4, we explore the heterogeneity of the effects by age at first birth, immigration status, or earnings category prior to birth. Again, we find no variation in the effects across these dimensions. Overall, these findings suggest that the null effects of the reform on fathers' and mothers' labor market outcomes reported in Section 5.2 are not only true on average but also at the individual level.

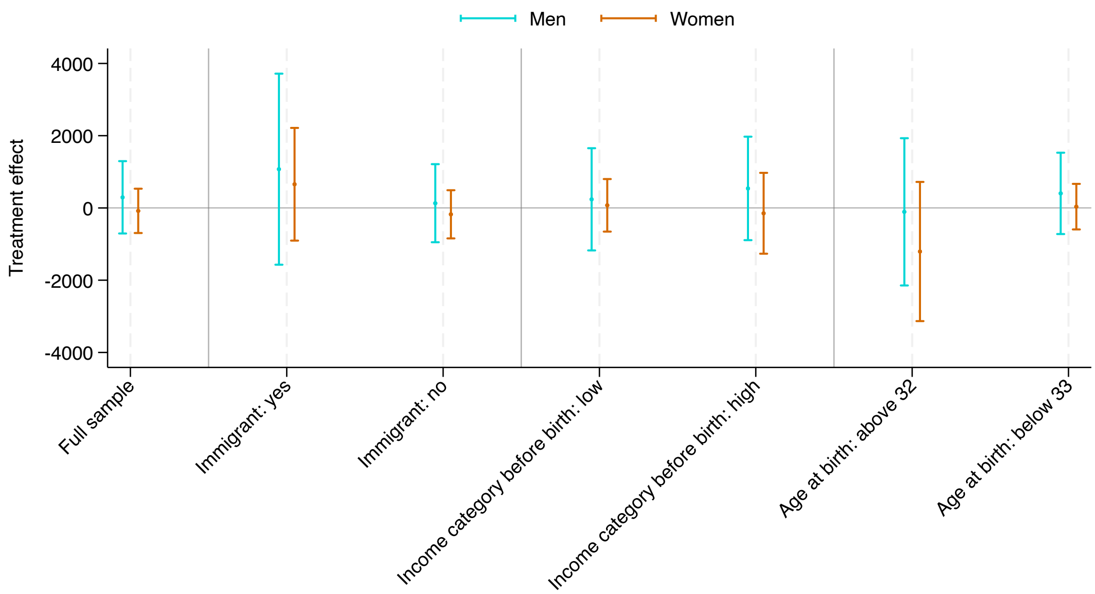

Figure E.4: Heterogeneity of the Effects on "Average Earnings 3-10 Years After Birth" Across Groups

Notes: The figure presents the effects of the 2006 reform on the employment earnings 3–10 years after birth of fathers and mothers across various subgroups. An immigrant is defined as an individual who was not a Canadian citizen by birth. "Income category before birth" is equal to "high" if the individual's employment earnings two years prior to birth were above the gender-specific median for that year, and "low" otherwise. Age at birth is recorded for the first child ever reported in the tax files. Effects are identified using our main difference-in-differences model (Equation 1). Each pair of points is obtained from a separate estimation of Equation 1, using a sample of 113,300 individuals (60,100 women and 53,200 men). The error bars represent the 95% confidence intervals around each point estimate, calculated using Huber-white robust standard errors. The data source is the CEEDD.

E.3 Alternative Specifications

Our main estimates rely on a difference-in-differences model which compares the outcomes of parents whose first child was born in the first six months following the reform to the outcomes of parents whose first child was born in the last six months before the reform, controlling for the difference in outcomes observed between parents whose child was born in the first six months of 2005 versus in the last six months of 2004. To test the robustness of our results, we also estimate the effects of the reform using various alternative specifications.

First, we estimate a model that includes individual fixed effects. To do so, we pool pre-birth years (years -4 to -2 relative to the birth of the first child) with post-birth years (years 2 to 9 relative to the birth of the first child) and estimate the following regression:

$$\begin{aligned} y_{it} = \gamma_0 &+ \gamma_1 JanJun_i \times Around0506_i \times Post_t \\ &+ \gamma_2 JanJun_i \times Around0506_i \times Woman_i \times Post_t \\ &+ \gamma_3 Post_t \\ &+ \gamma_4 JanJun_i \times Post_t \\ &+ \gamma_5 Around0506_i \times Post_t \\ &+ \gamma_6 Woman_i \times Post_t \\ &+ \gamma_7 Woman_i \times Around0506_i \times Post_t \\ &+ \gamma_8 Woman_i \times JanJun_i \times Post_t \\ &+ \alpha_i + \mu_{it}, \end{aligned} \tag{2}$$

where y_{it} is the outcome of individual i in relative year t. $Post_t$ is an indicator equal to 1 for post-birth years (relative years 2 to 9) and equal to 0 for pre-birth years (relative years -4 to -2). The term α_i denotes individual fixed effects, capturing all time-invariant characteristics of individual i. The remaining variables are defined as in Equation (1). The coefficient γ_1 captures the treatment effect for men (corresponding to β_1 in the baseline specification (1)), while γ_2 measures the differential treatment effect for women relative to men (corresponding to β_2 in the baseline specification).

Second, we show the estimates using tighter time windows around January 1st, ranging from one to five months (following our main specification in equation (1)). For reference, our main specification uses a six-month bandwidth.

Third, we implement donut exclusions by excluding parents whose first child was born within one week and within four weeks of the reform cutoff, addressing concerns about potential manipulation of the timing of births around the reform date.

Fourth, we test the robustness of the results using alternative sample definitions: (1) We restrict the sample to parents who declare the first child ever reported – which we designate as the first child ever born – within the first two years after birth, addressing concerns that this child might not always be the individual's own child; (2) We relax the sample restriction that requires individuals to have filed a tax return every year from the year before birth to ten years after.

Fifth, we re-estimate the DiD model using alternative control groups. In one specification, we use parents of children born between July 2003 and June 2004 as the control group instead of parents of children born between July 2004 and June 2005, as in the main analysis. In another specification, we use parents of children born between July 2005 and June 2006 in provinces outside Quebec, where no contemporaneous changes to parental leave policies occurred.

Sixth, we estimate the results from a regression discontinuity design, using the month of birth of the first child relative to the reform as the running variable. As our main differences-in-differences model, we estimate the regression discontinuity design model using a six-month bandwidth. And to capture the trends in the outcomes around the reform, we estimate a linear polynomial in the relative month of birth on both sides of the cutoff. Specifically, we estimate the following Ordinary Least Squares regression:

$$\begin{aligned} y_i = {} & \gamma_0 + \gamma_1 Post_i + \gamma_2 Post_i \times Woman_i + \gamma_3 RelativeMonth_i \\ & + \gamma_4 RelativeMonth_i \times Post_i + \gamma_5 RelativeMonth_i \times Woman_i \\ & + \gamma_6 RelativeMonth_i \times Post_i \times Woman_i + \gamma_7 Woman_i + \epsilon_i, \end{aligned} \tag{3}$$

where y_i is the outcome of interest for individual i, $Post_i$ is a binary indicator equal to 1 if the first child of individual i was born between January and June of 2006 (i.e., individual i is treated by the reform) and $Woman_i$ is an indicator for being a woman. $RelativeMonth_i$ is a continuous variable equal to the number of months between the date of birth of individual i's first child and January 1st, 2006 (e.g., it takes the value of -5.5 for a child born in July 2005, -0.5 for a child born in December 2005, and 5.5 for a child born in June 2006). Finally, ϵ_i is the error term. We include controls for the individual's age at first birth, immigration status, and income category two years prior to birth, and compute heteroskedasticity-robust standard errors.

Finally, we conduct two placebo tests: (1) We estimate the same difference-in-differences regression as if the reform took place on January 1st, 2005. In this scenario, we compare the outcomes of parents whose first child was born in the first six months of 2005 with the outcomes of parents whose first child was born in the last six months of 2004, controlling for the difference in outcomes observed between parents whose child was born in the first six months of 2004 versus the last six months of 2003; (2) We estimate the same difference-in-differences regression as if the reform had occurred in Canadian provinces outside Quebec, where no change in the parental leave scheme happened at that time, using parents whose first child was born outside Quebec.

Figures E.5, E.6, and E.7 presents these results. Across all alternative specifications and outcomes, we consistently find that the reform had no effect.

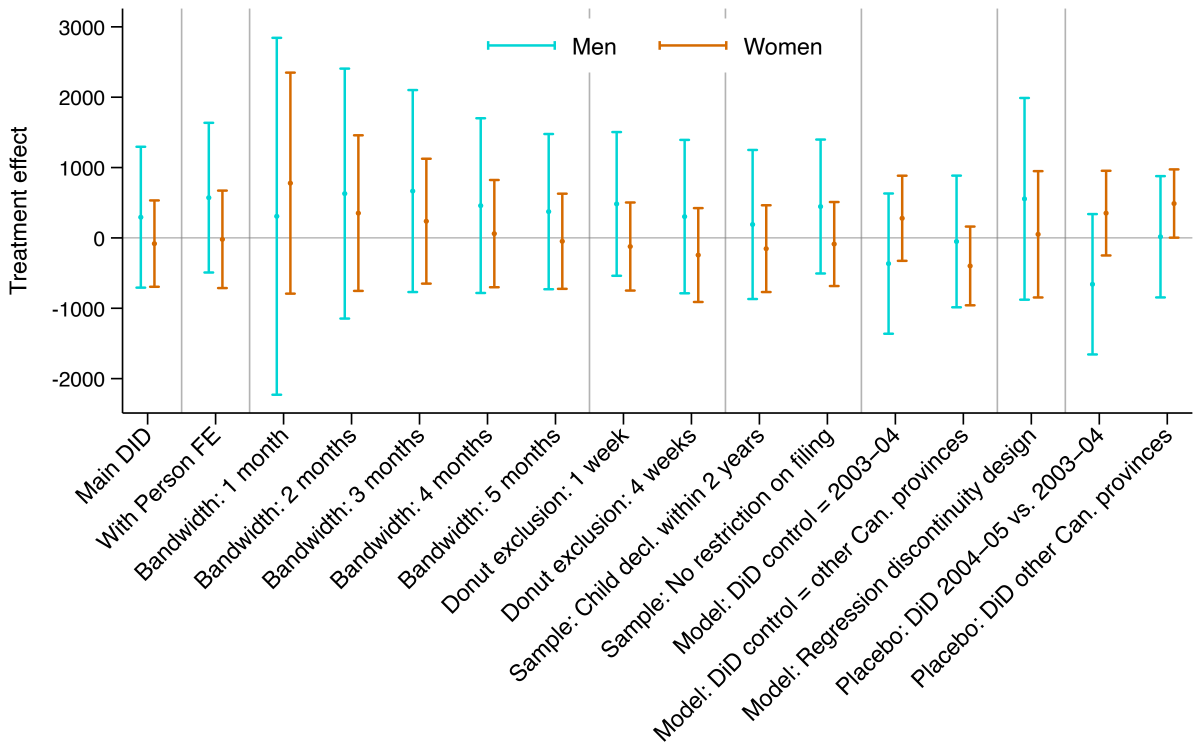

Figure E.5: Effects on "Average Earnings 3-10 Years After Birth" Across Alternative Specifications

Notes: The figure presents the effects of the 2006 reform on employment earnings 3–10 years after birth across various specifications. First, we estimate the model including individual fixed effects. Second, we use tighter time windows around January 1st, ranging from one to five months. Third, we exclude parents whose first child was born within one week and four weeks of the reform cutoff (donut exclusions). Fourth, we use alternative sample definitions by (i) restricting the sample to parents who report their first child within two years of birth and (ii) relaxing the requirement to file tax returns from birth to ten years after. Fifth, we re-estimate the DiD model using alternative control groups: (i) parents of children born between July 2003 and June 2004 and (ii) parents of children born between July 2005 and June 2006 in provinces other than Quebec. Sixth, we estimate a regression discontinuity model using the date of birth of the child as the running variable. Finally, we conduct two placebo tests by estimating the DiD model (i) as if the reform took place on January 1st, 2005, and (ii) as if the reform took place in provinces other than Quebec. The error bars represent the 95% confidence intervals around each point estimate, calculated using Huber-white robust standard errors. Sample sizes vary across specifications. The data source is the CEEDD. For more details on each specification, please refer to Appendix Section E.3.

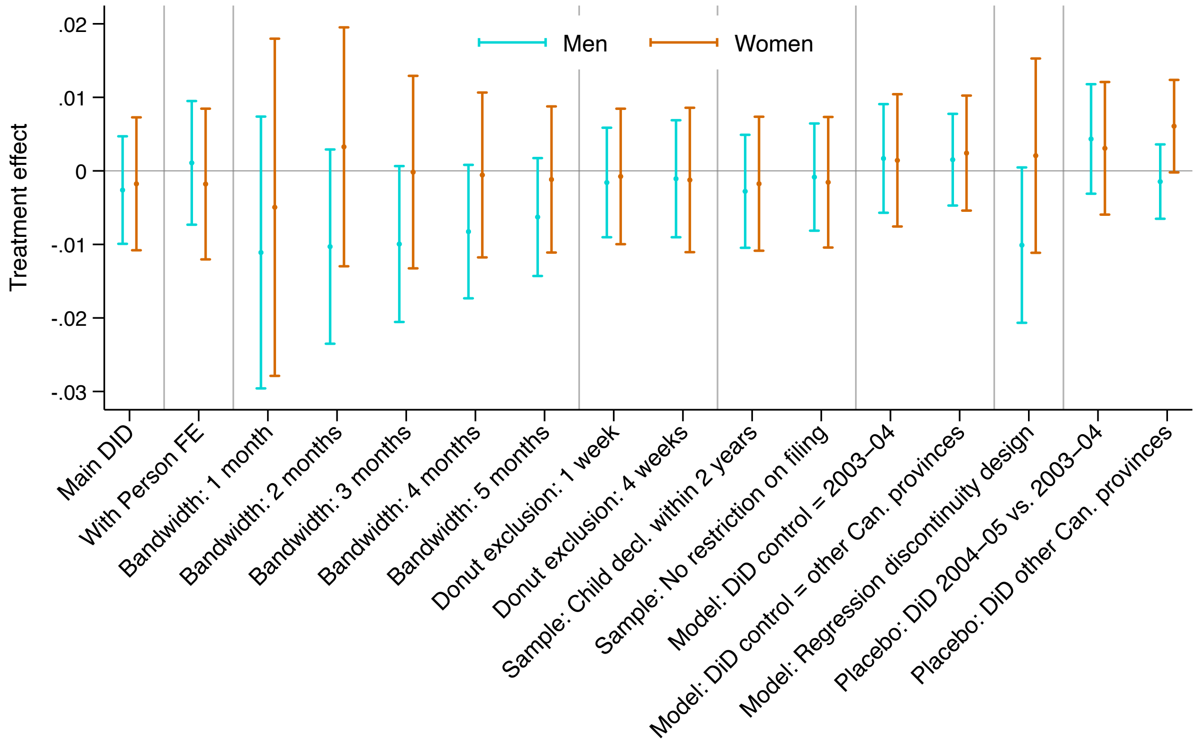

Figure E.6: Effects on "Fraction of Years Employed 3-10 Years After Birth" Across Alternative Specifications

Notes: The figure presents the effects of the 2006 reform on the fraction of years employed 3-10 years after birth across various specifications. First, we estimate the model including individual fixed effects. Second, we use tighter time windows around January 1st, ranging from one to five months. Third, we exclude parents whose first child was born within one week and four weeks of the reform cutoff (donut exclusions). Fourth, we use alternative sample definitions by (i) restricting the sample to parents who report their first child within two years of birth and (ii) relaxing the requirement to file tax returns from birth to ten years after. Fifth, we re-estimate the DiD model using alternative control groups: (i) parents of children born between July 2003 and June 2004 and (ii) parents of children born between July 2005 and June 2006 in provinces other than Quebec. Sixth, we estimate a regression discontinuity model using the date of birth of the child as the running variable. Finally, we conduct two placebo tests by estimating the DiD model (i) as if the reform took place on January 1st, 2005, and (ii) as if the reform took place in provinces other than Quebec. The error bars represent the 95% confidence intervals around each point estimate, calculated using Huber-white robust standard errors. Sample sizes vary across specifications. The data source is the CEEDD. For more details on each specification, please refer to Appendix Section E.3.

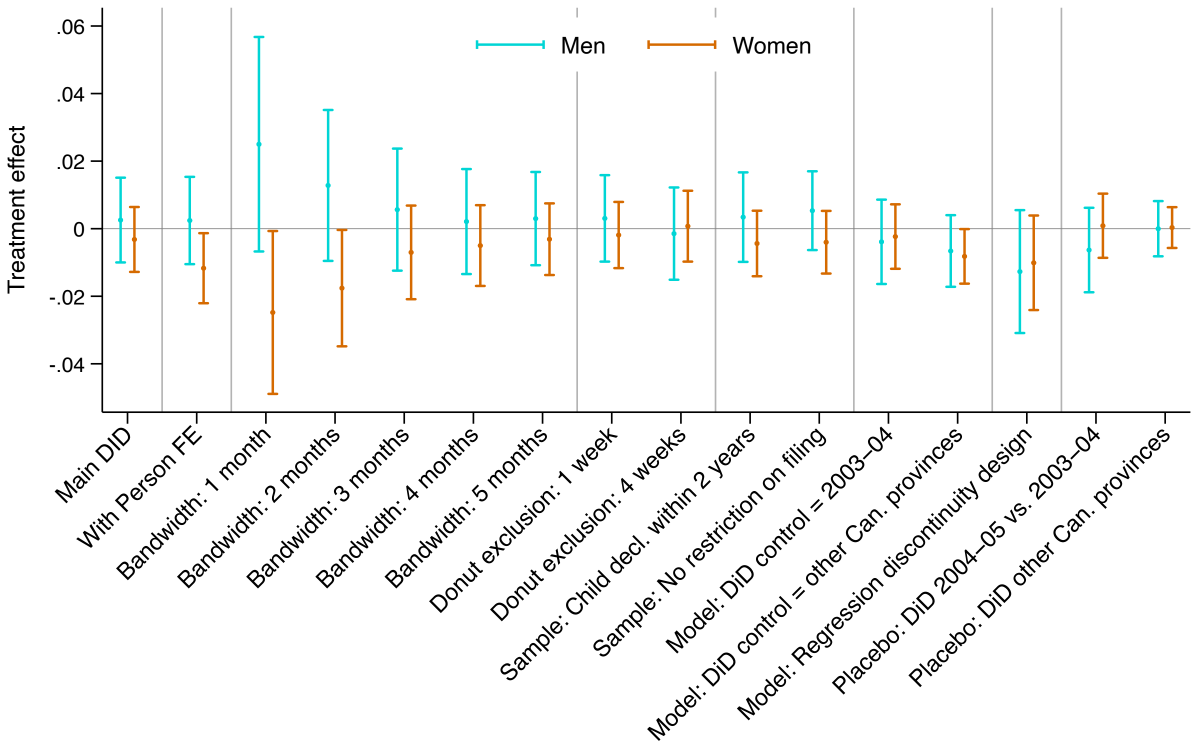

Figure E.7: Effects on "Fraction of Years Employed in a High-Paying Industry 3-10 Years After Birth" Across Alternative Specifications

Notes: The figure presents the effects of the 2006 reform on the fraction of years employed in a high-paying industry 3-10 years after birth across various specifications. First, we estimate the model including individual fixed effects. Second, we use tighter time windows around January 1st, ranging from one to five months. Third, we exclude parents whose first child was born within one week and four weeks of the reform cutoff (donut exclusions). Fourth, we use alternative sample definitions by (i) restricting the sample to parents who report their first child within two years of birth and (ii) relaxing the requirement to file tax returns from birth to ten years after. Fifth, we re-estimate the DiD model using alternative control groups: (i) parents of children born between July 2003 and June 2004 and (ii) parents of children born between July 2005 and June 2006 in provinces other than Quebec. Sixth, we estimate a regression discontinuity model using the date of birth of the child as the running variable. Finally, we conduct two placebo tests by estimating the DiD model (i) as if the reform took place on January 1st, 2005, and (ii) as if the reform took place in provinces other than Quebec. The error bars represent the 95% confidence intervals around each point estimate, calculated using Huber-white robust standard errors. Sample sizes vary across specifications. The data source is the CEEDD. For more details on each specification, please refer to Appendix Section E.3.

F Social Norms

In this section, we use data from the World Values Survey (WVS) to provide insights into gender norms by comparing Quebec with the rest of Canada (ROC) and European countries. The WVS spans seven waves: more details here. For Canada, data is available for 2000, 2006, and 2020. Our analysis focuses on the section of "*Social values, Attitutes and Stereotypes*" to examine gender-related beliefs and attitudes.

F.1 Gendered Education Priorities

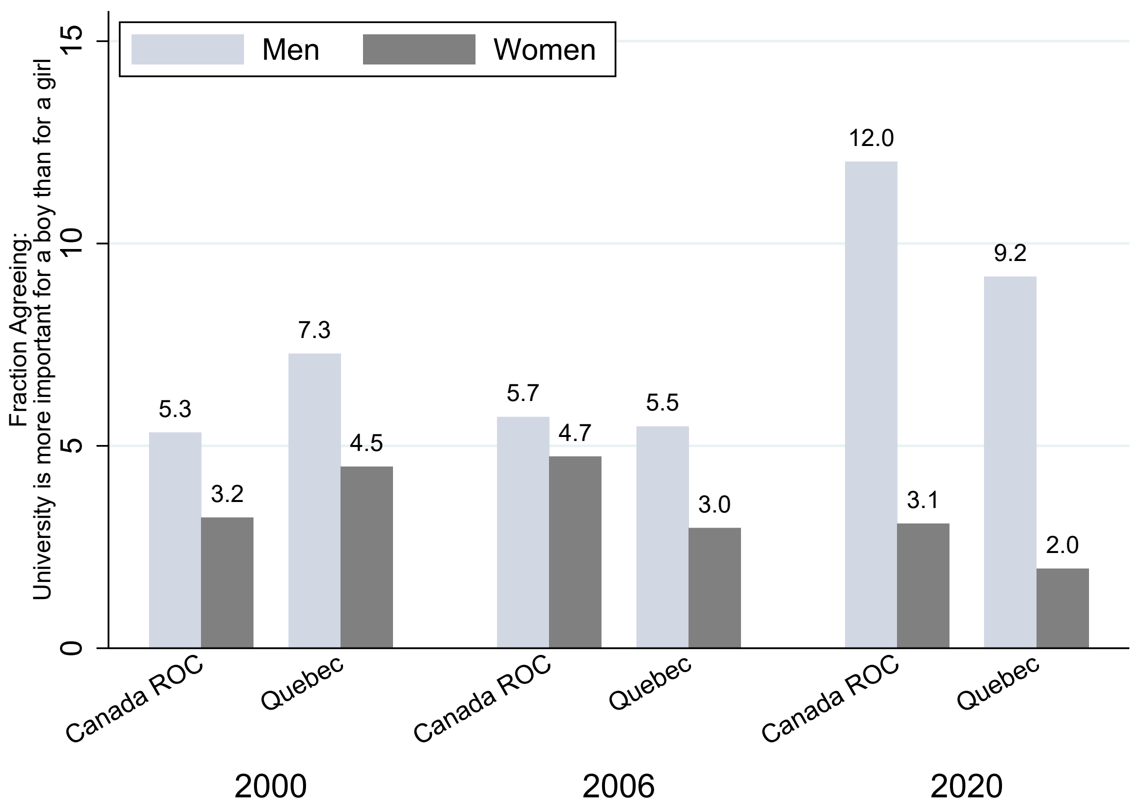

Figure F.8: Fraction Agreeing "*University is more important for a boy than for a girl*" in Quebec and Rest of Canada

Notes: The figure presents the fraction of respondents who agree with the statement "*A university education is more important for a boy than for a girl.*" The data comes from the World Value Survey (wave 4 (1999-2004), wave 5 (2005-2009), and wave 7 (2017-2022)) for Quebec and the rest of Canada (ROC). Higher values indicate stronger agreement with this traditional gender norm.

Figure F.8 highlights the shifting attitudes towards gender roles in education between

Quebec and the rest of Canada (ROC). The proportion of women agreeing with the statement "*A university education is more important for a boy than for a girl*" has decreased between 2000 and 2020 from 4.5% to 2% in Quebec. Attitudes among men on this question are relatively higher than those of women in both regions, with notable fluctuations and a significant increase in the Rest of Canada (ROC) in 2020. Figure F.11 shows Quebec's position among Western countries in terms of attitudes towards gender roles in education.

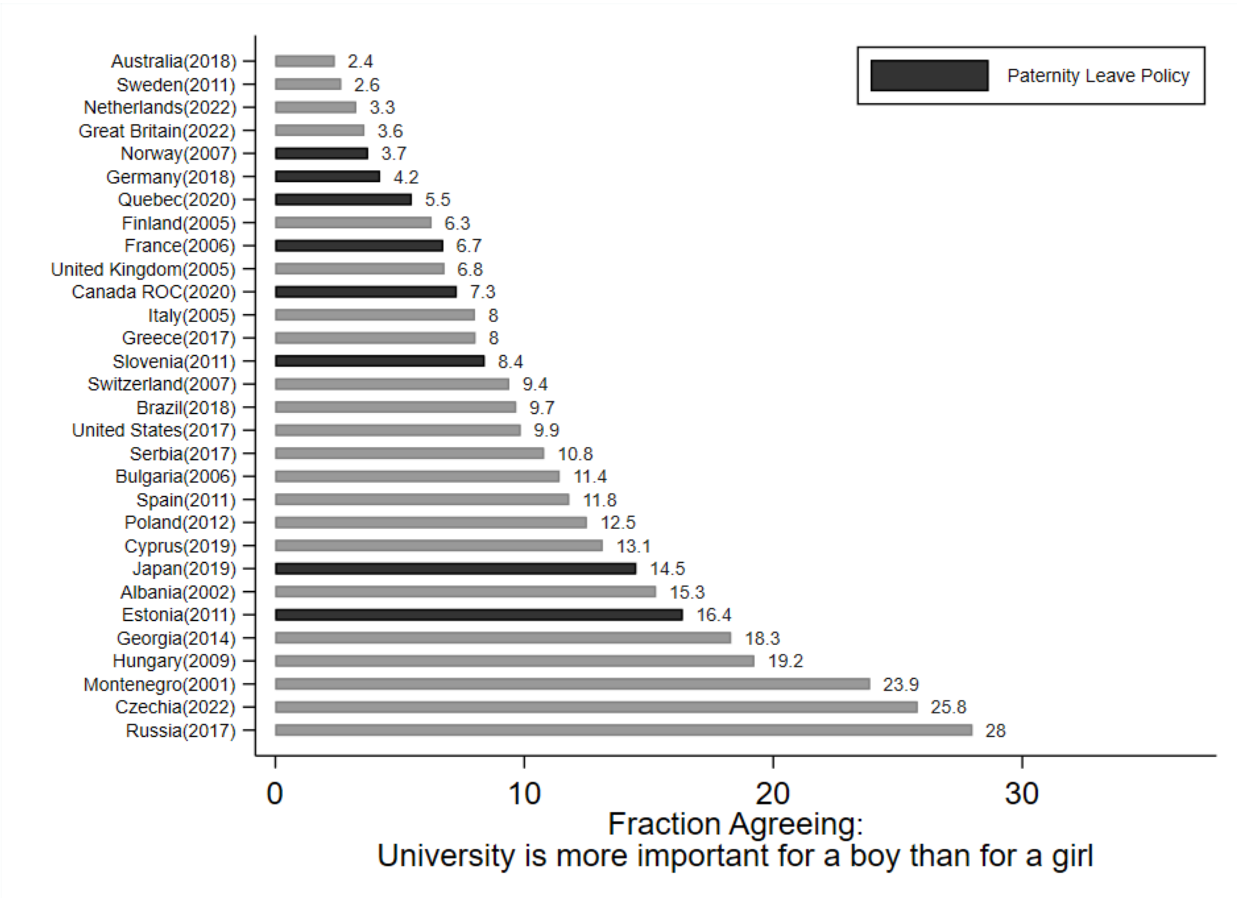

Figure F.9: Fraction Agreeing "*University is more important for a boy than for a girl*" Across High Income Countries

Notes: The data is comes from wave 7 of the World Value Survey (WVS). Countries with paternity leave policies "Daddy Quota" (indicated in dark bars) and those without such policies (in light bars). Higher values reflect stronger adherence to traditional gender norms.

F.2 Traditional Gender Norms Regarding the Role of Mothers

The question asked in Wave 7, "When a mother works for pay, the children suffer" reflects traditional gender norms and societal attitudes regarding the role of women, emphasizing their responsibility in child-rearing rather than participation in the workforce.

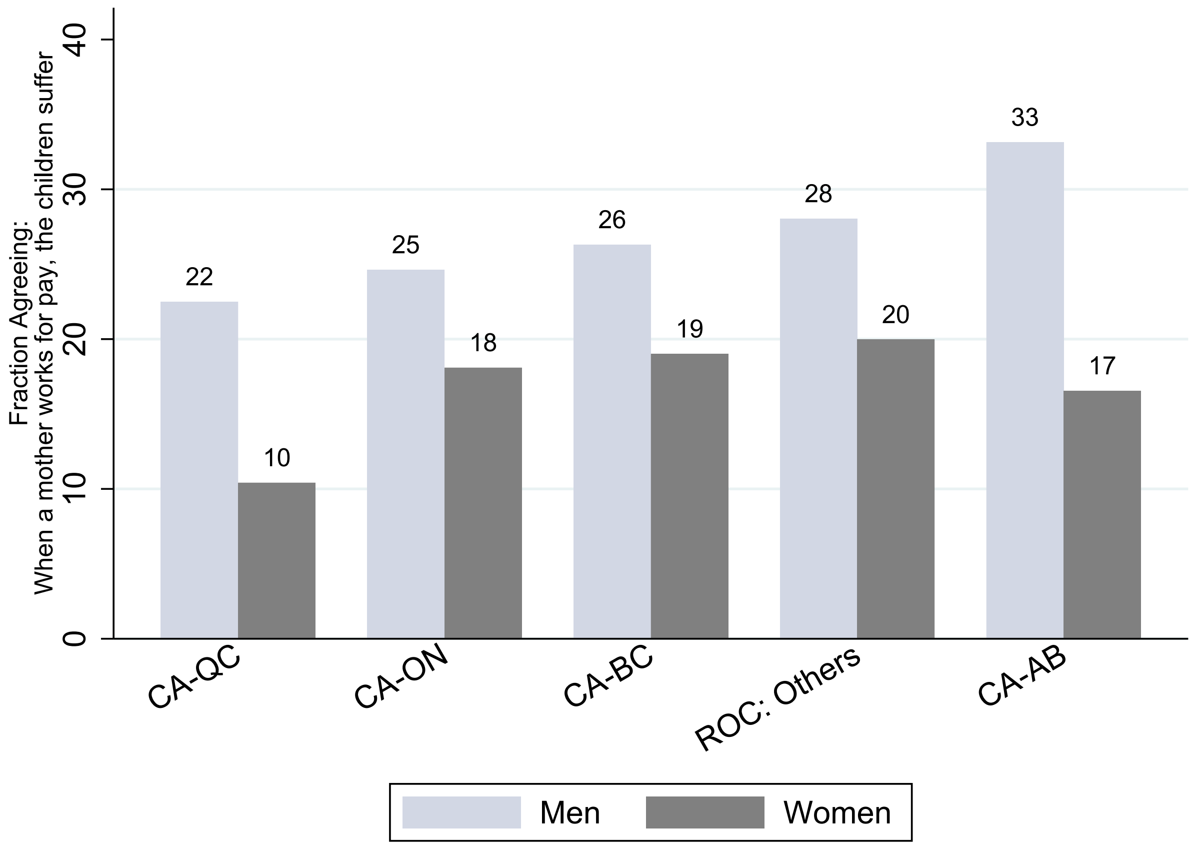

Figure F.10: Fraction Agreeing "*When a mother works for pay, the children suffer*" Across High Income Countries

Notes: the WVS wave 7 was conducted in 2020 in Canada.

In Figure F.10, more than one in five men across all regions in Canada agree with this traditional views of gender norms, reflecting persistent traditional views on the impact of working mothers on children. The agreement is particularly high in Alberta, where 33% of men believe that when a mother works for pay, the children suffer. In contrast, Quebec shows the lowest level of agreement among men (22%) and women (10%), indicating more progressive attitudes in this province. Women generally exhibit lower levels of agreement than men across all regions, further emphasizing the gender gap in perceptions of working mothers.

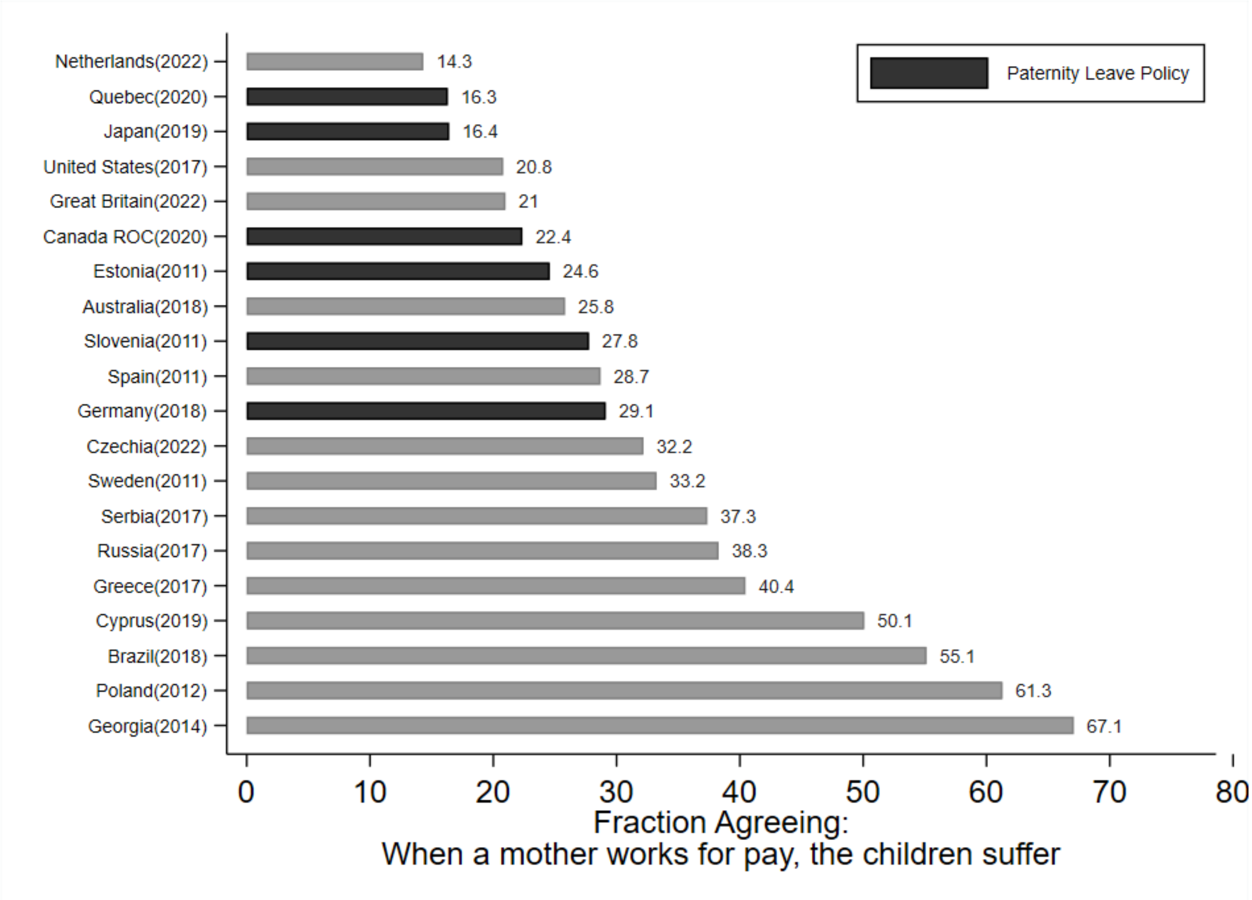

Figure F.11: Fraction Agreeing "*When a mother works for pay, the children suffer*" Across High Income Countries

Notes: the WVS wave 7 was conducted in 2020 in Canada.

Figure F.12 presents evidence however that gender norms in Quebec are not uniformly pushing toward having mothers participate in the labor market. Rather, respondents from Quebec are tolerant and accepting of diverse approaches to childcare and work. This figure shows that the Quebecers are more likely to agree that *being a housewife is just as fulfilling as working for pay* than are responds from many other high income countries.

Overall then, these responses suggest a society open to diverse approaches to arranging child care responsibilities without imposing strong norms on these approaches onto parents.

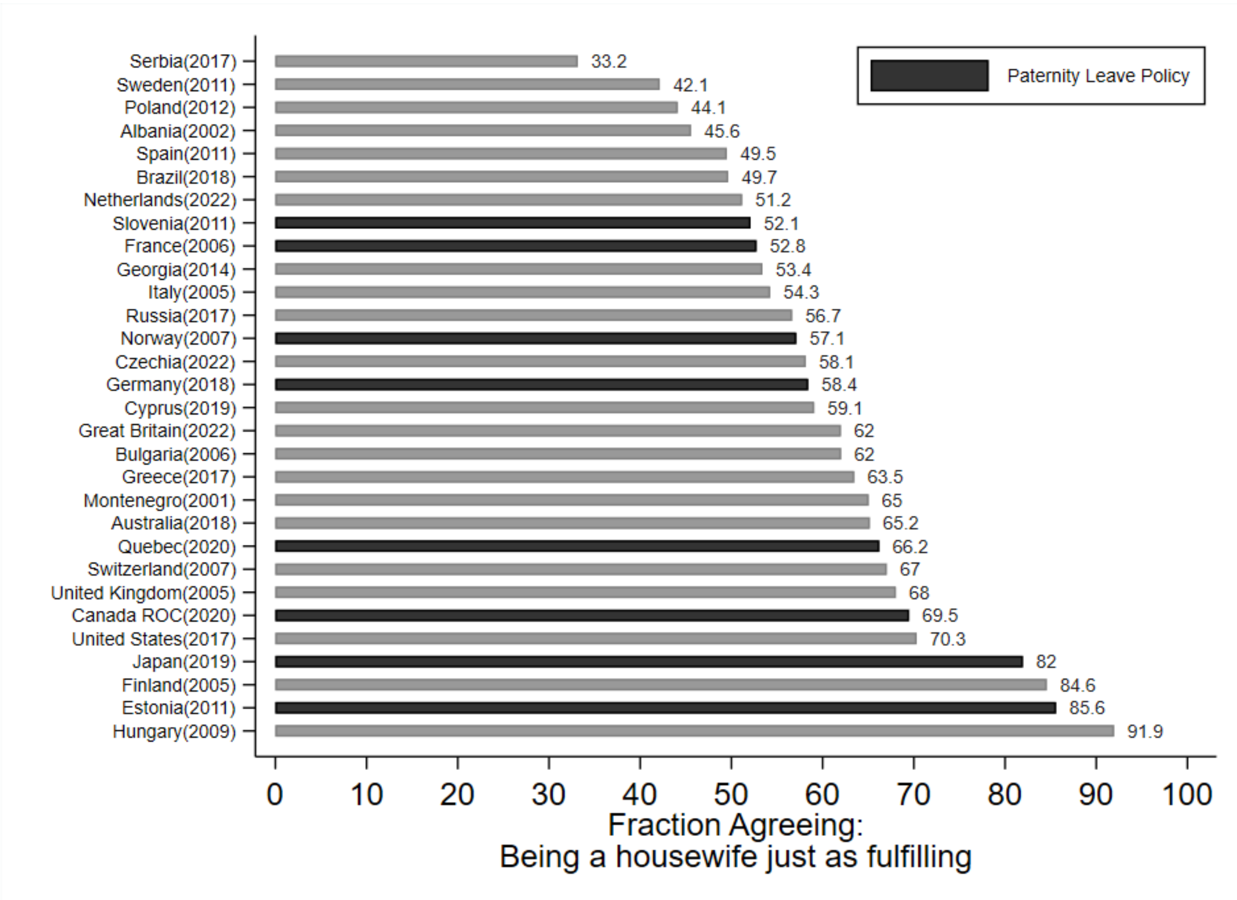

Figure F.12: Fraction Agreeing "*Being a housewife is just as fulfilling as working for pay*" Across High Income Countries

Notes: the WVS wave 7 was conducted in 2020 in Canada.