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Can Paternity Leave Reduce the Gender Earnings Gap?

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Abstract

This paper examines the impact of paternity leave on the gender gap in labor market outcomes. Utilizing administrative data from Canadian tax records, we analyze the introduction of Quebec's 2006 paternity leave policy, which offers five weeks of paid leave exclusively to fathers. 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 increased fathers' uptake of parental leave and reduced their earnings immediately after the reform. 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 the reform 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 changed social norms around care-taking and family responsibilities.

Keywords: paternity leave, gender earnings gap
JEL Codes: J13, J16

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1 Introduction

An emerging consensus attributes much of the gender gap in labor market outcomes to the "child penalty", that is the large and persistent drop in earnings that women – but not men – experience following the birth of their first child.[1] 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 a 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 by earmarking leave to the second parent (Cortes and Pan 2023).[2] 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."[3]

In 2006, the Canadian province of Québec reformed its parental leave system and likewise earmarked five weeks of paid parental leave for fathers with the same aim of narrowing gender gaps in the labor market. The policy succeeded in drastically increasing the share of fathers taking some parental leave from 20 percent to 60 percent.[4] [5]

This paper shows that this reform failed to affect earnings of either mothers or fathers over the long run and thus did not reduce gender disparities in earnings. This negative empirical 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. See Kleven et al. (2019) for an overview of child penalties across countries and Connolly, Fontaine, and Haeck (2023) for the child penalty in Canada, specifically.

2. Examples include the introduction of such paternity leave in Norway in 1993, Sweden in 1995, and in Spain in 2007. Denmark extended paternity leave from two to four weeks in Denmark in 1998. In 2022, Denmark in 2022 earmarked a further 7 weeks for fathers.

3. See **p. 4**; Ekberg, Eriksson, and Friebel 2013.

4. 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.

5. In 2019, the Canadian government introduced a similar policy covering the rest of the country.

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 parental 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. However, we find no evidence that the reform impacted long-term earnings trajectories of either mothers or fathers. We obtain precise null effects that rule out that mothers' earnings increased by more than 2.1 percent three to ten years after childbirth. For fathers, we can reject earnings decreases over the same horizon of more than 1.7 percent. Combining, we can rule out that the reform reduced the gender gap by more than 4.7 percent. Together, our results indicate that the 2006 introduction of paternity leave failed to significantly attenuate gender earnings inequalities in Québec.

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 then explore potential mechanisms that have been proposed by examining how the reform impacted various indicators of social norms around care-giving. We find no significant effects on any of these indicators, suggesting that the reform failed to shift traditional care-giving norms. We believe this is the reason why we fail to find any effects: the policy reform does not suffice to affect care-giving arrangements which may reflect deeply held preferences and social norms. In light of this, ear-marking a few weeks of leave to fathers does not make much of an impact.

Our findings differ from two previous papers on the effects of the 2006 Québec reform on fathers' and mothers' labor market outcomes (Patnaik 2019; Dunatchik and Özcan 2021). In contrast to our findings, both papers report some positive effects of the reform on mothers' labor market outcomes. However, these studies rely on a small number of observations – fewer than 10,000 individuals – resulting in noisier estimates compared to ours, which are based on administrative data covering the entire population of tax filers and their non-filing spouses. In addition, the two studies use a difference-in-difference design comparing the outcomes of parents in Québec with those of parents in other Canadian provinces. This design assumes that, in the absence of the reform, outcomes of parents in Québec would have followed the same trend as in other Canadian provinces.[6] For our main analysis we do not to rely on other Canadian provinces as a control group but rather compare the outcomes of parents in Québec 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. However, in the appendix we report results using the Rest of Canada as a control group and, contrary to Patnaik 2019 and Dunatchik and Özcan 2021 we again find precise zeros. Our paper is thus important in that it overturns results from prominent papers that have received substantial attention in the literature and policy space.

Our paper contributes to the nascent literature on the effects of paternity leave use on parents' labor market outcomes following childbirth. Credible causal evidence on the topic is however limited. 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 Québec 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

6. Patnaik (2019), which only uses one year before the reform and one year after, cannot use other time-periods to test how plausible this assumption is. Dunatchik and Özcan (2021) uses more years and provides evidence of a similar trend in outcomes prior to the reform between Québec and Ontario, which they use as the control group. However, their estimated effects on mothers' outcomes appear only in 2008–2009, two to three years following the reform and coinciding with the Great Recession, which has affected Ontario more strongly than Québec (Gilmore and LaRochelle-Côté 2009).

care-giving.

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. Although paternity leave policies were hoped to directly addressing traditional social norms around care-giving, our findings suggest that their ability to significantly reduce the child penalty may, too, be limited.

2 Parental Leave Policies in Québec

Before 2006 and across all Canadian provinces, parents who at 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.[7] These weeks were compensated at 55 percent of earnings, up to a maximum of CA$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 percent of fathers took any parental leave weeks, compared to 74% percent of mothers.

On January 1, 2006, Québec launched its own parental leave program, the Québec 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. The change on which we base our study is that the 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 and did indeed succeed in this. As we show in the following sections, following the reform the

7. 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.

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.

3 Data

3.1 Data Sources

The main data source for our analysis 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 percent 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 this data to estimate the intent to treat 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. To measure take-up we turn to the Longitudinal Administrative Databank (LAD) (Statistics Canada 2023) which is derived from the same administrative datasets as the CEEDD but includes additional variables on employment insurance benefits. These variables on benefits and others contained in the LAD also allows us to derive indicators of gender norms around care-giving. We thus turn to the LAD to estimate the effect on take-up and gender norms. Unfortunately however, the LAD only covers a 20 percent 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

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

to ten years after birth. We also study the effects of the reform on two additional labor market outcome measures: whether employed and industry of employment.

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 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 percent 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 a first child in Québec around the time of the reform, i.e., between July 2004 and June 2006.[10] [11] We exclude 6 percent of the births for which the exact day of birth is not declared.

We also restrict our sample to parents who filed taxes every year from the year prior to the birth to ten years after, in order to be able to identify the province of residence around childbirth and to ensure the reliability of the labor market outcomes we use. We show in Appendix D.3 that our results are substantially the same if we relax this restriction.

9. The information from the Record of Employment file is not completely reliable to identify parental leave take-up (Hou, Magolis, and Haan 2017). Because we can not determine the precise dates when individuals took leave, we can not 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 are defined annually and reflect the dates of birth of the current dependents of the individual. We detail how we identify the first child ever born to each individual in Appendix B.3.

11. We restrict our sample to parents residing in Québec on December 31st of both 2005 and 2006 for our main group, and on December 31st of both 2004 and 2005 for our control group.

3.4 Descriptive Statistics

Table 1 presents descriptive statistics for our sample, disaggregated by gender, and date of birth of the first child. Additionals summary statistics are reported in the appendix C (table C.1).

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

	July-Dec/2004 (1)	Jan-June/2005 (2)	July-Dec/2005 (3)	Jan-June/2006 (4)
Men				
Average annual earnings (in 1,000s of 2019 CA$)	55.7 (39.0)	55.7 (38.0)	56.3 (39.4)	56.5 (38.8)
Fraction of years with positive earnings	0.9	0.91	0.9	0.91
Fraction of years in a high-paying industry	0.26	0.26	0.27	0.26
Fraction with a second child (%)	74.37	74.95	75.21	74.97
Women				
Average annual earnings (in 1,000s of 2019 CA$)	31.2 (26.7)	31.3 (26.3)	32.1 (27.0)	32.2 (26.6)
Fraction of years with positive earnings	0.8	0.8	0.8	0.8
Fraction of years in a high-paying industry	0.15	0.15	0.16	0.15
Fraction with a second child (%)	72.98	73.29	73.97	74.57

Notes: This table provides labor market outcomes for individuals who had their first child between July 2004 and June 2006 for men and women. The average annual earnings represent the employment income from two to nine years after first child birth.

In Table 1, we report the main labor market outcomes for men and women who had their first child around the reform (18 months before and 6 months after).

4 Empirical Strategy

We use a double differences methodology around the birth of the first child to identify the causal effects of the reform. Specifically, we compare 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). To ensure the difference we observe between the two groups is not driven by seasonal birth patterns, we subtract the difference in outcomes observed across parents whose child was born in the first six months of 2005 versus the last six months of 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:

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

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.[12] All standard errors are heteroskedasticity-robust.

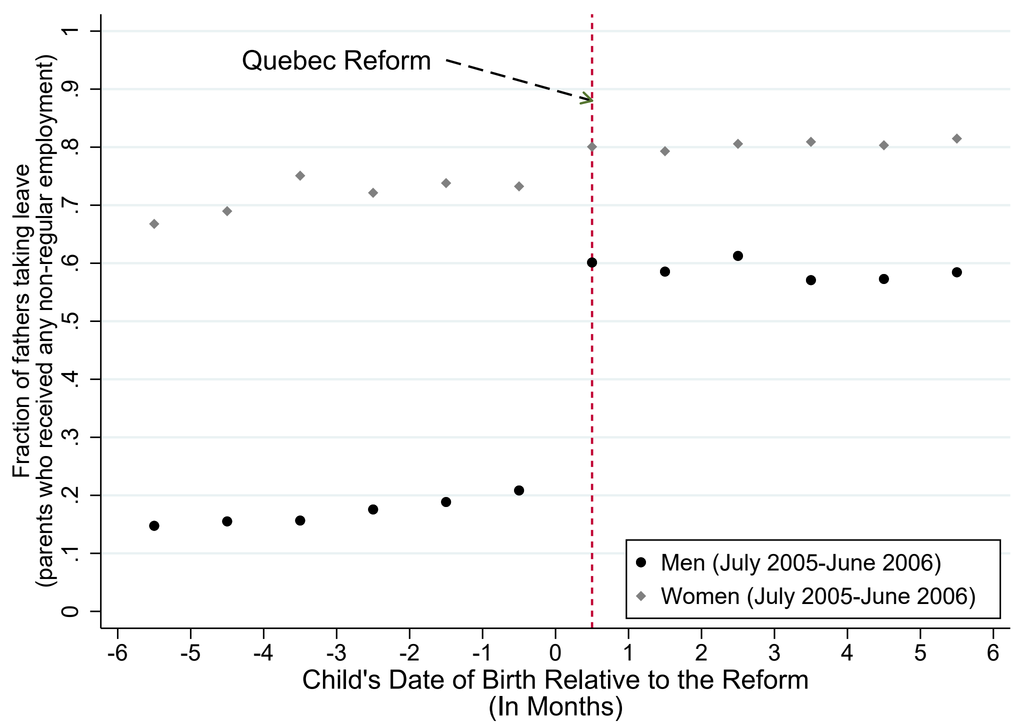
Our coefficients of interest are β_1 and β_2. While β_1 captures the average effect of the reform on men, β_2 captures the difference in the average effect of the reform between women and men, which can also be interpreted as the average effect of the reform on the gender gap in outcome y. Throughout the tables, we report these two

12. 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.

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

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

Figure 1: Leave Taking among New Parents (First Child)



Note: The figure shows how the fraction of new fathers taking any parental leave. 'Parents taking leave' refers to those who received non-regular employment benefits. benefits

5 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 as well as from July 2004 to June 2005.

Following the reform, fathers were substantially more likely to take parental leave. Take-up among fathers increased by 40 percentage points from about 20 percent to about 60 percent following the reform. (see Appendix Table C.2.) 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.[13]

6 Main Results: Effects on Earnings

We next explore the effects of the reform on parents’ employment earnings, our main outcome of interest. We report in Figure 2 the treatment effects up to ten years following the birth and in Table 2 the effect on average earnings between three to ten years after birth. All estimates are based on model 1.

As shown in Figure 2, 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 of about CA$2,100, which is equivalent to 2.4 weeks of pay for the average full-time worker. This loss is partially compensated by a CA$1,800 increase in the non-regular employment benefits they receive from the government, as shown in Appendix Table C.2. Figure 2 also documents a significant decrease of about CA$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 we document earlier, as well as an increase in leave duration due to the increase in generosity.

Beyond the first year after birth, we find precise null effects of the reform on the earnings of either fathers and mothers (Figure 2 and Table 2): the 95% CI for the three-to-ten year effect of the reform ranges from -1.3 to +2.3 percent of the pre-reform mean for fathers and between -2.2 percent and 1.7 percent for mothers.

13. 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).

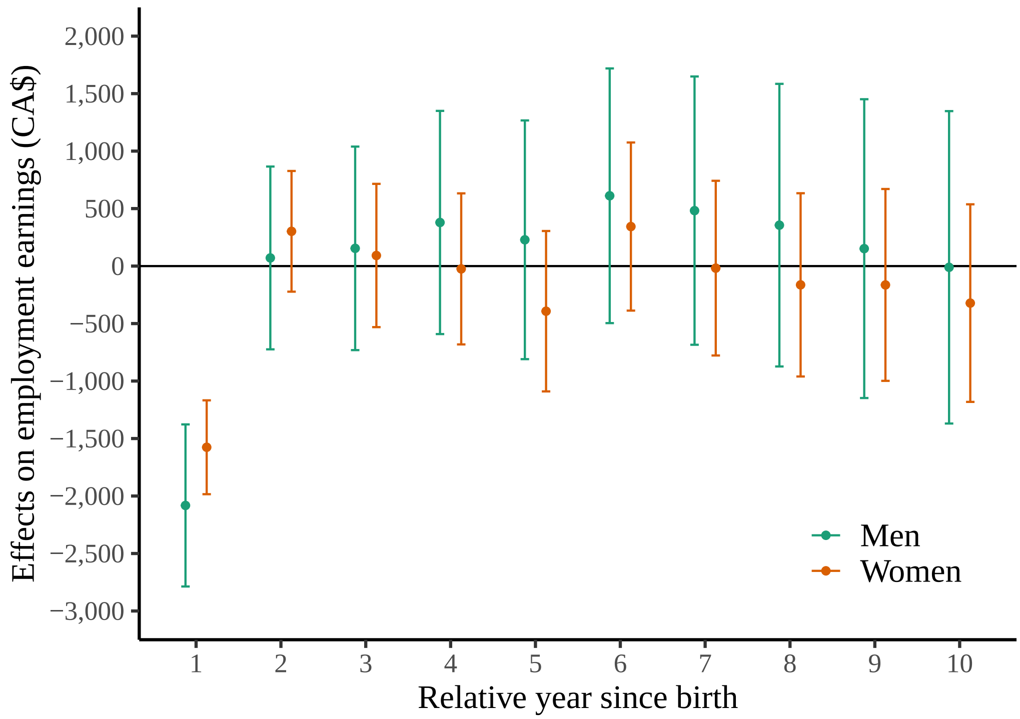


Figure 2: Effects on Annual Employment Earnings Over Time

Notes: The figure plots the effects of the 2006 reform on annual earnings following the birth of their first child identified using outcomes of parents whose first child was born in the first six months following the reform in 2006 to the outcomes of parents whose first child was born in the last six months before the reform relative to the analogous difference in 2005. 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 percent confidence intervals obtained using Huber-white robust standard errors.

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 CA$800. Given that the gender earnings gap over the same period was approximately CA$24,200, this means that we can exclude a reduction in the gap greater than 3.3 percent.

Finally, Panels B and C of Table 2 report the effects of the reform on two additional labor market outcomes: the likelihood of individuals working and the likelihood of them working in high-paying industries. Consistent with our findings on earnings, we observe precise null effects on these two outcomes for both fathers and mothers.

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 D, 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 and consistently find no effect.

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)	(599)
Pre-reform mean	56,300	32,100	-24,200
Panel B: Fraction of years with positive employment earnings			
Treatment effect	-0.0026	-0.0018	0.0008
	(0.0037)	(0.0046)	(0.0059)
Pre-reform mean	0.90	0.80	-0.10
Panel C: Fraction of years working in a high-paying industry			
Treatment effect	0.0026	-0.0032	-0.0057
	(0.0064)	(0.0049)	(0.0081)
Pre-reform mean	0.27	0.16	-0.11
Obs.	113,300	113,300	113,300

Notes: The table reports effects of the 2006 reform on labor market outcomes three to ten years following the birth of the first child from (1) using the CEEDD. Panel A reports the effect on average earnings, Panel B the effect on the fraction of years with positive earnings, and Panel C on the fraction of years worked in high-paying industries (industries that fall within the top 25 percent of industries in 2006). Huber-white robust standard errors are reported in parentheses. Pre-reform 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.

7 Mechanisms

Paternity leave policies hold promise to reduce the child penalty by shifting traditional social norms around care-giving. One reason the 2006 Québec 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. 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. We propose that as a shift in gender norms would increase the share in 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 Québec 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 6.

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)	(0.0352)	
Pre-reform 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)	(430)	
Pre-reform 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)	(0.0230)	
Pre-reform mean	0.2935	0.3714	0.0780	
Panel D: Had a second child				
Treatment effect	0.0008	-0.0095	0.0013	25,300
	(0.0159)	(0.0150)	(0.0217)	
Pre-reform 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 using (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 the reform. 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. Pre-reform 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.

8 Conclusion

In conclusion, this study shows that the Québec 2006 introduction of paternity leave reform had no significant effect on either mothers' or father's medium or long-run labor market outcomes, contrary to previous findings reported in previous studies (Patnaik 2019, Dunatchik and Özcan 2021. This aligns with recent research from Scandinavia, such as Andresen and Nix 2024, indicating that small policy nudges, such as providing a few extra weeks of paternity leave, are insufficient to change deep-rooted gender norms around childcare and labor market participation.

However, 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 such policies are structured to potentially promote broader well-being within families, even if they do not directly address the labor market disparities between genders.

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Supplementary Material

A Details regarding the 2006 Québec Reform

On January 1, 2006, the Québec government launched its own paid parental leave program, the Québec 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 percent of earnings for the first 30 weeks, with a maximum amount of $CA767. The remaining 15 weeks are compensated at 55 percent earnings with a maximum amount of $CA602. In comparison, under the federal program, individuals received 55 percent of earnings, up to a maximum of CA$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 percent of earnings, with a weekly maximum amount of $CA822.

Table A.1: Federal Employment Insurance (EI) program and Québec 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 Québec in 2005 and under the Québec Parental Insurance Plan (QPIP) introduced in 2006 in Québec.

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 in the medium-run: Average of the "Annual employment earnings" variable from three to ten years after birth.

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

- Fraction of years with positive employment earnings in the medium-run: Average of the indicator "Positive employment earnings" from three to ten years after birth.

- Works in a high-paying industry: Binary indicator equal to one if the industry of the individual's primary employment falls within the top 25 percent 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 working in a high-paying industry in the medium run: Average of the indicator "Works 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 D.3 that our results are similar if we restrict our sample to individuals who declare the first child within two years after birth.

C Additional Tables and Figures

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

	July-Dec/2004	Jan-June/2005	July-Dec/2005	Jan-June/2006
Ever received some child benefits conditional on separation (%)	**Men**			
	24.74	22.4	22.93	24.53
	Women			
	86.9	87.17	86.59	85.69
Amount of non-reg. employment benefits received for the 2nd child	**Men**			
	2600	2700	2800	2900
	(3983)	(4040)	(4237)	(4011)
	Women			
	11500	12000	11500	12600
	(10425)	(10462)	(10520)	(10892)
Age at first child Birth	**Men**			
	30.56	30.48	30.7	30.78
	(5.2)	(5.12)	(5.11)	(5.04)
	Women			
	28.12	28.17	28.38	28.56
	(4.8)	(4.68)	(4.66)	(4.48)
Fraction of immigrants (%)	**Men**			
	14.8	14.1	16.0	14.7
	Women			
	14.7	14.0	15.9	14.9

Notes: This table reports the summary statistics of individuals who had their first child between July 2004 and June 2006 for men and women. Standard deviations are reported in parentheses for continuous variables.

Table C.2: 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)
Pre-reform mean	0.171	0.716	0.545
Panel B: Amount of non-regular employment benefits received (CA$)			
Treatment effect	1,835***	5,142***	3,307***
	(154)	(227)	(275)
Pre-reform mean	800	9,100	8,300
	(2,592)	(7,515)	
Obs.	11,800	13,500	

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 obtained using (1) on 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.

D Validity Checks

D.1 Similarity in Earnings Patterns

In Figure D.1, we plot average average annual employment earnings 3-10 years following the reform for parents whose first child born between July 2005 and June 2006 and July 2004 to June 2005, respectively. The figure supports that there are no difference in trends across groups.

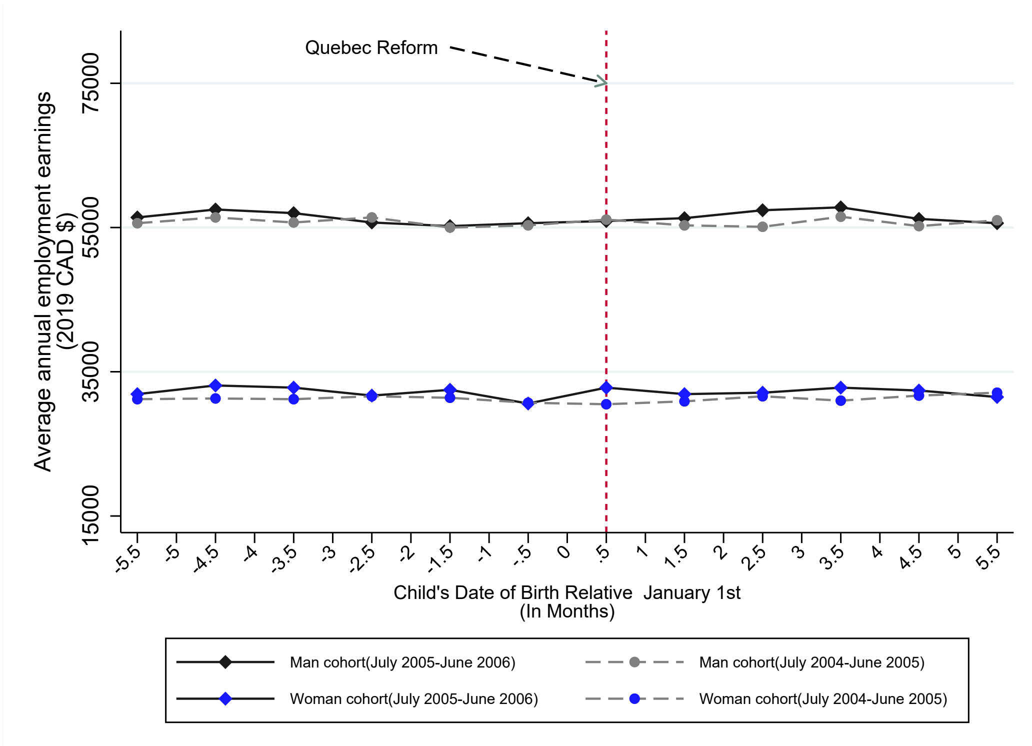

Figure D.1: Medium-run Effects Across Males and Females

Notes: Data: CEEDD.

D.2 Heterogeneity

The effects reported in Section 6 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 D.2, 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.

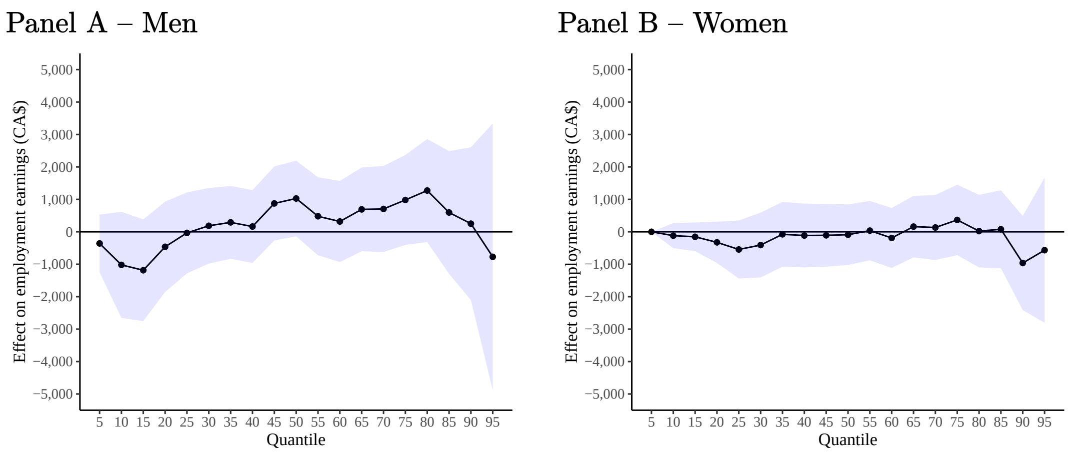

Figure D.2: Medium-run Effects Across the Earnings Distribution

Notes: The figure reports the medium-run effects of the 2006 reform on the distribution of employment earnings of fathers (Panel A) and mothers (Panel B). The treatment effects are identified from 19 quantile regressions using our main 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. 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 percent confidence intervals around the point estimates, calculated using Huber-white robust standard errors. The data source is the CEEDD.

In addition, in Figure D.3, 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 6 are not only true on average but also at the individual level.

D.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 show the estimates using tighter time windows around January 1st, ranging from one to five months. For reference, our main specification uses a six-month bandwidth.

Second, 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.

Third, we present the results using as the control group parents of children born from July 2003 to June 2004 instead of parents born from July 2004 to June 2005 that we use in our main model.

Fourth, 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:

$$\begin{aligned} y_i = \gamma_0 + \gamma_1 Post_i + \gamma_2 Post_i \times Woman_i + \gamma_3 RelativeMonth_i \\ + \gamma_4 RelativeMonth_i \times Post_i + \gamma_5 RelativeMonth_i \times Woman_i \\ + \gamma_6 RelativeMonth_i \times Post_i \times Woman_i + \gamma_7 Woman_i + \epsilon_i, \end{aligned} \tag{2}$$

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 Québec, where no change in the parental leave scheme happened at that time, using parents whose first child was born outside Québec.

Figure D.4 presents these results. Across all alternative specifications, we consistently find that the reform had no effect.

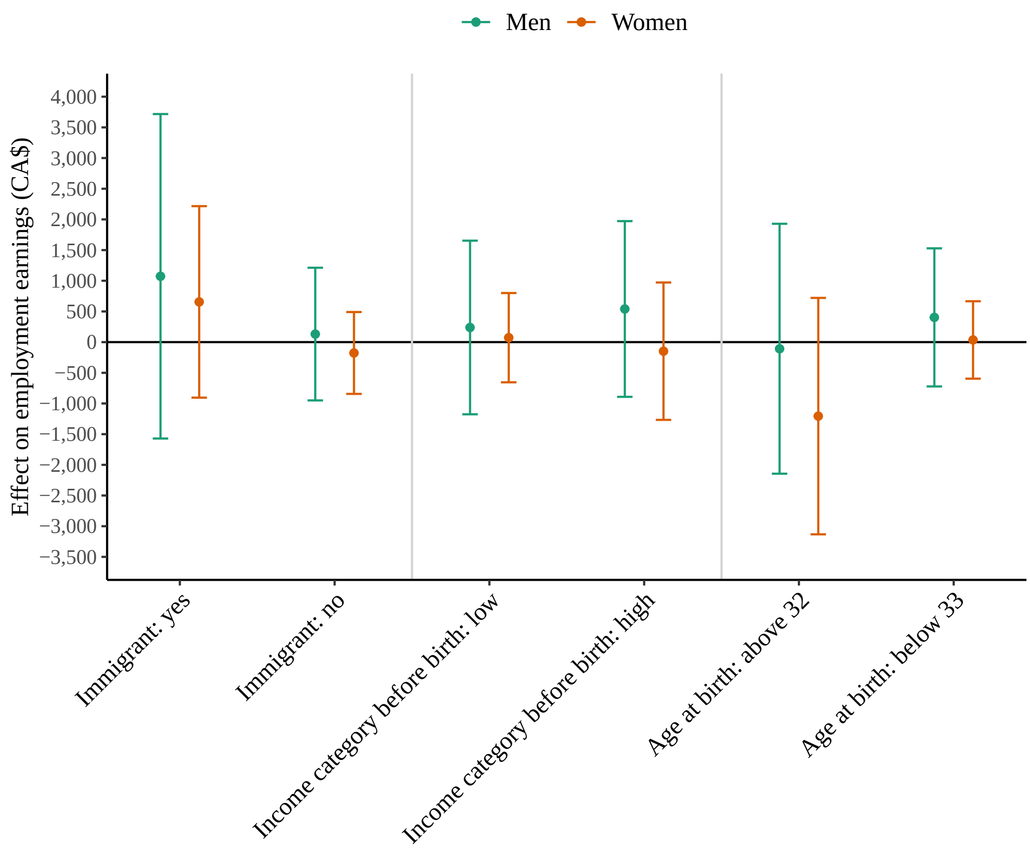

Figure D.3: Heterogeneity of the Medium-run Effects Across Groups

Notes: The figure presents the medium-run effects of the 2006 reform on the employment earnings 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, 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 (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 percent confidence intervals around each point estimate, calculated using Huber-white robust standard errors. The data source is the CEEDD.

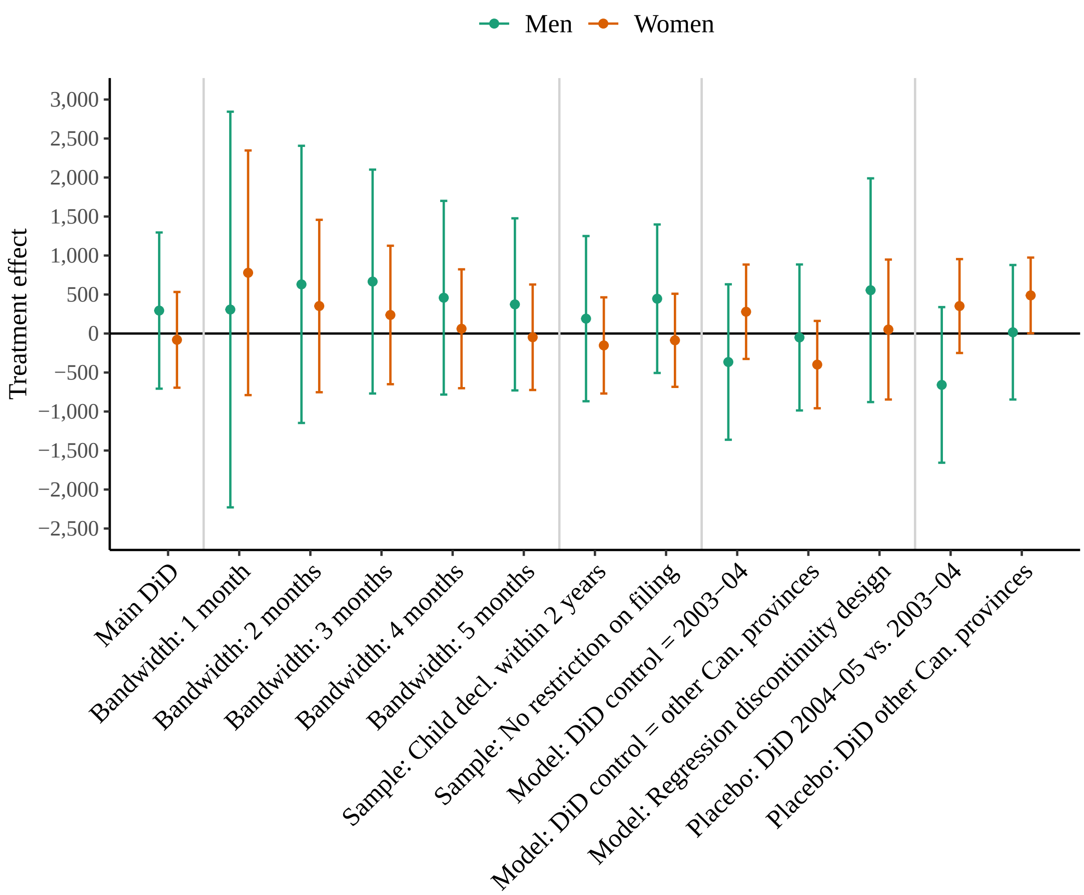

Figure D.4: Medium-run Effects on Earnings Across Alternative Specifications

Notes: The figure presents the medium-run effects of the 2006 reform on employment earnings across various specifications. First, we use tighter time windows around January 1st, ranging from one to five months. Second, we test the robustness of our results with alternative sample definitions: restricting the sample to parents who declare their first child within two years of birth and relaxing the requirement for filing tax returns from birth to ten years after. Third, we present the results using as the control group parents of children born from July 2003 to June 2004. Fourth, we employ a regression discontinuity design using the month of birth of the first child relative to the reform as the running variable. Finally, we conduct two placebo tests by applying the difference-in-differences model as if the reform took place on January 1st, 2005, and as if the reform took place in provinces other than Québec. The error bars represent the 95 percent 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 D.3.