

DISCUSSION PAPER SERIES

IZA DP No. 15894

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ISSN: 2365-9793

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ABSTRACT

The Multigenerational Impact of Children and Childcare Policies*

The disproportionate impact of children on women's earnings constitutes the primary factor contributing to persistent gender inequality in many countries. This paper examines the multigenerational impact of children and whether the public provision of formal childcare lessens the earnings and employment impacts of children. Our analyses notably incorporate the role of grandparents as informal providers of childcare. We find that the arrival of a firstborn reduces the employment and earnings of mothers and grandmothers, suggesting that the life-cycle impact of children on women is larger than previously thought. Studying the implementation of a universal childcare program in the province of Quebec, we find that formal childcare increases the employment rates of mothers, as well as that of grandmothers to a lesser extent. Examining heterogeneity of the program's impact across census divisions, we find a negative correlation between the magnitude of the positive effects on mothers' employment and the pre-policy supply of informal childcare by grandmothers. Our findings together indicate that (1) analyses of social policies should consider broader family units and (2) the impact of childcare policies on mothers depends on pre-existing care arrangements, particularly the amount of care provided by grandparents.

JEL Classification: H31, H42, J08, J13, J16, J18, J22

Keywords: childcare policies, labour supply, grandparents, parents, event study, policy reform

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* We thank Janice Compton, Catherine Haeck, Olga Malkova, Doug Miller, Matt Notowidigdo, Alex Whalley, Nicolas Ziebarth, and seminar participants at various seminars and conferences for helpful comments. Financial support from the Social Sciences and Humanities Research Council of Canada is gratefully acknowledged.

1 Introduction

Despite formidable improvements in the gender gap in labour force participation over the 20th century, convergence has stagnated since the turn of the millennium (Fortin, 2019). A rapidly expanding literature has shown that the impact of children on earnings and employment falls disproportionately on mothers relative to fathers, causing large and lasting "child penalties" on women (Kleven et al., 2019a,b). These child penalties constitute the primary driver of persistent gender inequality. One possible key factor contributing to child penalties is an unequal distribution of childcare responsibilities between parents. For instance, in Canada, 11% of 24-35-year-old men provide over 30 hours of unpaid childcare per week. For women, the proportion is 38%.¹

This paper examines how the availability of alternative modes of care—notably the public provision of formal childcare—affects the impact of children on women’s employment and earnings. Our analyses incorporate one aspect of childcare that has received little attention: a significant amount of childcare is provided by grandparents. In Canada, 15% of children below age 6 are cared for by a non-parent relative (see Figure 1). In the US, grandparents are the primary caregivers for 20% of children below age 5 (Posadas and Vidal-Fernandez, 2013).

Findings from prior studies of the impact of childcare provision on mothers’ employment are mixed, with estimates varying considerably across countries.² For example, Andresen and Nix (2020) finds that expanding access to child care in Norway reduced the child penalty by 23%, while Kleven et al. (2020) finds no effect of either parental leave policies or child care policies on the child penalty in Austria. Could grandparents be the missing element to reconcile these mixed findings? For instance, Figure A3 shows that the use of informal childcare provided by grandparents is far greater in Austria than in Norway. Our study uses Canadian data to probe the role of grandparents as care providers and evaluate the impact of childcare policies on both mothers’ and grandmothers’ labour market outcomes. In particular, we examine whether the availability of care provided by grandmothers modulates the impact of childcare policies on mothers.

Our analysis proceeds in several steps. First, we combine the event-study framework developed in Kleven et al. (2019a) with detailed Canadian tax data to estimate the multi-generational impact of children on their parents and grandparents.³ We find that the child penalty on mothers in Canada is comparable to that found in Scandinavian countries and

¹See Figure A1 for details.

²See Cortes and Pan (2020) and Olivetti and Petrongolo (2017) for excellent reviews of this literature.

³Concurrent work by Gørtz et al. (2020) also estimate a grandchild penalty using Danish data and obtain qualitatively similar results.

substantially lower than in the US, the UK, and Austria (Kleven et al., 2019b). The arrival of children also coincides with a sharp reversal in the likelihood of residing in the same location as one’s parents (the grandparents), which increases dramatically following childbirth. This pattern suggests families value proximity, plausibly for reasons associated with provision of childcare by grandparents. Consistent with this idea, we document a substantial impact of grandchildren on the earnings and employment trajectories of grandmothers and grandfathers. Consequently, existing child penalty estimates that focus only on parents understate the impact of children on gender inequality as mothers incur an additional penalty when they become grandmothers.

Second, we estimate the impact of children on mothers’ and grandmothers’ earnings and employment separately for 281 Census Divisions to document patterns of substitution and complementarity across modes of care from spatial correlations.⁴ Even within Canada, there is substantial variation in the severity of the impact of parenthood on mothers’ and grandmothers’ labour supply. We find that the earnings and employment effects of children on mothers are negatively correlated with the corresponding impacts on grandmothers, suggesting some scope for substitution between parental and grandparental care. Importantly, the impact of children on mothers’ earnings and employment is considerably smaller in places with greater formal childcare use. Finally, we find that places with more formal childcare use exhibit larger earnings reductions among grandmothers, suggesting that formal and grandparental care could be complements. These cross-sectional spatial correlations could, however, reflect unobserved heterogeneity. To study the causal impact of formal childcare, we consider an exogenous shock to childcare costs next.

We exploit the introduction of a universal childcare program in the province of Quebec to estimate the impact of childcare policies on mothers’ and grandmothers’ labour market outcomes. Prior evaluations of this program have found significant concurrent increases in mothers’ labour supply (Haeck et al., 2015; Kottelenberg and Lehrer, 2013; Lefebvre and Merrigan, 2008; Baker et al., 2008). We extend these results in three important ways. First, we also consider the effects on grandmothers. Second, we estimate heterogeneous effects across 98 Census Divisions within Quebec. This analysis allows us to examine the key correlates of effect sizes, comparing across locations within the same institutional and cultural setting. We study these patterns through the lens of a local average treatment effects framework that allows for three alternative modes of care, in the spirit of Kline and Walters (2016). Finally, with longitudinal data, we can use the outcomes of yet-to-be mothers as an additional comparison group to account for differential trends in women’s labour market

⁴We will make this atlas of parenthood and grandparenthood effects publicly available for other researchers to use.

outcomes between Quebec and the rest of Canada.

Our results confirm that formal childcare programs can substitute for parent-provided care, helping attenuate the impact of children on mothers' employment and earnings. We find that the introduction of the universal childcare program led to a 4 percentage point increase in the annual employment rate of mothers. Moreover, the program reduced the long-run impact of children on earnings (10 years after the first childbirth) by roughly 8%. Childcare subsidies also affect grandmothers' labour market outcomes. The policy increased grandmothers' employment by about 2% but somewhat reduced their earnings on average. These results are consistent with formal childcare care substituting for "intensive" care provided by relatives but complementing low-intensity informal care (e.g., 5 hours or less per week).

Comparing effect sizes of childcare subsidies on mothers' outcomes across places, we find that a key correlate is the pre-policy intensity of unpaid care provided by grandmothers. Specifically, mothers' employment increase far more in locations where grandparents play a less important role. This finding suggests the potential effect of childcare subsidies on mothers' labour market outcomes depends on pre-existing care arrangements, and that international comparisons of family policies should therefore take grandparents into account.

Our paper contributes to several streams of the vast literature on gender earnings inequality. A growing body of work, using data from several countries, has shown that the impact of children on earnings is significant, persistent, and falls disproportionately on women (Andresen and Nix, 2020; Cheng, 2020; Connolly et al., 2020; Lloyd, 2020; Kleven et al., 2019b; Sieppi and Pehkonen, 2019; Kuziemko et al., 2018; Bertrand et al., 2010). This differential effect of parenthood is responsible for the majority share of the overall gender earnings gap (Cortes and Pan, 2020; Kleven et al., 2019a). We find that children affect both mothers' and grandmothers' earnings, suggesting that the total impact of children on women over the life cycle is larger than previously thought.

Our work also relates to a smaller literature on the role of grandparents as caregivers and the implications for labour market outcomes. Most existing studies examine how the availability of grandparents as childcare providers affects maternal labour supply (Kaufmann et al., 2022; Zamarro, 2020; Bratti et al., 2018; Compton and Pollak, 2014; Posadas and Vidal-Fernandez, 2013). Consistent with the idea that parent- and grandparent-provided care are substitutes, they find that greater availability of grandparents increases maternal labour supply. We similarly find that child penalties are smaller for mothers living close to their parents at the time of childbirth. We additionally show that the impacts of children on mothers' and grandmothers' earnings are negatively correlated across places. Other recent work has considered the labour supply of grandparents as the outcome of interest. Rupert and Zanella (2018) and Backhaus and Barslund (2021) use the gender of the first child as an

instrument for becoming a grandparent and find that having a grandchild reduces women’s labour supply. Frimmel et al. (2020) and Gørtz et al. (2020) exploit variation in the timing of the birth of a grandchild and come to similar conclusions. Our results are consistent with these findings and further demonstrate that grandparents’ labour supply is responsive to policy-induced changes in the cost of alternative modes of childcare.

We also contribute to the literature on the impact of family policies on mothers’ labour market outcomes. Findings are generally mixed, with estimates varying considerably across countries and policies. In most cases, maternity leave policies have only modest effects on maternal labour supply, with negative long-term impacts in some instances (Blair and Posmanick, 2023; Dahl et al., 2016; Schönberg and Ludsteck, 2014; Lalive et al., 2014). Several studies of childcare provision find positive effects on maternal labour supply, with Quebec’s case generally producing estimates among the largest in the literature (e.g., Brewer et al., 2022; Andresen and Havnes, 2019; Bauernschuster and Schlotter, 2015; Nollenberger and Rodríguez-Planas, 2015). Others, such as Kleven et al. (2020), find no effect. Here, our contribution is twofold. First, we demonstrate that grandparents can be affected by childcare policies too. This finding calls for broader assessments of the impact of family-friendly policies on female labour supply that encompasses spillovers onto grandparents. Second, we provide evidence that the availability of informal care provided by relatives is a crucial predictor of differences in the magnitude of the impact of childcare subsidies on mothers.

The remainder of the paper is organized as follows. Section 2 describes the institutional context and presents the databases used in our empirical analyses. Section 3 presents estimates of child and grandchild penalties in Canada. Section 4 introduces a conceptual framework, and Section 5 estimates the impact of Quebec’s childcare program on mothers and grandmothers. Section 6 estimates the heterogeneous effects of the Quebec program and discusses cross-country differences in childcare arrangements and norms to put our results in context. Section 7 concludes.

2 Context and Data

2.1 Institutional Setting

Background Parental Leave and Retirement Policies. The Canadian federal government implemented a maternity leave program in the 1970s, which operates through the employment insurance (EI) system. The program’s eligibility criteria, duration of paid leave, and generosity of benefits have expanded over time. Mothers were initially eligible for ma-

ternity leave benefits for 15 weeks at a replacement rate of 55%. In 1990, couples became eligible for an additional 10 weeks of parental benefits that can be split between parents. In 2001, the duration of parental benefits was extended to 35 weeks. Take-up rates among women exceed 80% and are much lower for fathers (Beaupre, 2021). As a result, women's earnings are expected to drop precipitously in the year they become mothers when they are on leave. In contrast, any impact of children on grandparents would likely materialize about a year later when mothers return to work.

In 2006, the province of Quebec established the Quebec Parental Insurance Plan (QPIP, or RQAP in French). Under this program, parents can choose from a menu of plans: 18 weeks of maternity leave at a 70% replacement rate, 32 weeks of parental leave at a replacement rate of 70% for the first 7 weeks and 55% for the remaining 25, and 5 weeks of paternal leave at a 70% replacement rate. The program also contains several other features, such as better coverage for self-employed workers. Thus, QPIP is more generous than the federal plan that prevails in other provinces, which incentivizes families to take longer leaves, likely resulting in a larger earnings drop in the year following childbirth in Quebec relative to the rest of Canada from 2006 onward.

Many men and women become grandparents close to retirement age. Quebec and the rest of Canada have separate pension plans (QPP and CPP, respectively), but they are essentially identical in eligibility rules and the types and amounts of benefits available. Individuals 65 or older are also eligible for federal Old Age Security (OAS) benefits, which depend on how long one has lived in Canada and on current annual income. While OAS has remained largely unchanged for the past four decades, CPP and QPP underwent significant reforms in the early 1980s concerning early retirement rules (Staubli and Zhao, 2021). A reform in 2011 also phased in changes in benefit generosity to incentivize later retirement, but the adjustments were virtually the same in the CPP and QPP. Eligibility to and generosity of pension benefits is unrelated to grandparenthood status. The average retirement age is relatively lower in Quebec and declined in Quebec and the rest of Canada until the late-1990s. Since the turn of the millennium, the average retirement age has been increasing, going back up to 1970s levels by 2020.⁵

Quebec's Universal Childcare Program. First introduced in September 1997, Quebec's childcare policy provided generous subsidies to eligible childcare providers. Under this program, the out-of-pocket price charged to parents for a subsidized place was 5\$ per day with the government covering the remaining costs. The program was gradually phased-in for different age groups, and the number of subsidized places has steadily increased. Still,

⁵Statistics Canada Table 14-10-0060-01.

a common perception is that the supply of subsidized places has not kept up with demand (Haeck et al., 2015). Four-year-olds were first eligible in September 1997. Eligibility was extended to three-year-olds in September 1998 and two-year-olds in September 1999. Finally, all children aged 0-5 were eligible by September 2000. Simultaneously, in 1998, the province started requiring that all elementary schools offer before- and after-school childcare. Haeck et al. (2015), Lefebvre and Merrigan (2008), and Baker et al. (2008) provide further details regarding the program’s implementation. When reporting summary estimates of the program’s impact, we only consider the years 2000-2005 as treatment years. We exclude the phase-in years (1997-1999) and years 2006 onward, which may conflate any effect of Quebec’s 2006 parental leave reform.

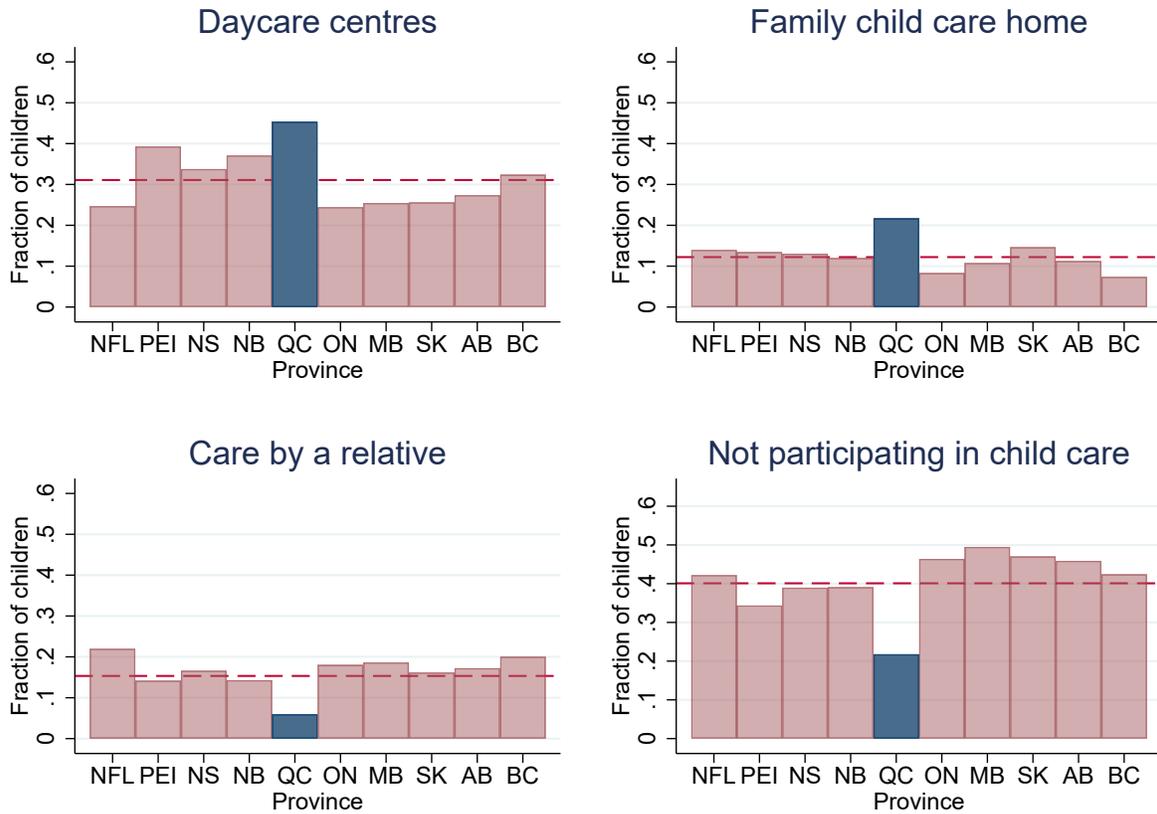
Today, families in Quebec are considerably more likely to use formal childcare services than families in the rest of Canada. Figure 1 shows the fraction of children 0-5 in different types of care by province. Whereas about 40% of Canadian children are not participating in any form of non-parental child care, the figure is only 22% in Quebec. Moreover, take-up rates of services provided by daycare centres and family child care homes (both are eligible for the subsidized rate in Quebec) are far higher in Quebec than elsewhere. Children residing in Quebec are also less likely to be cared for by a relative, suggesting that formal childcare services provide a substitute for grandparent-provided care for some families. Cultural differences between Quebec and the rest of Canada could partly explain differences in formal childcare use, but survey evidence suggests differences in costs of childcare are important. Appendix Figure A2 shows that significantly fewer families in Quebec report not using childcare because the cost is too high. This pattern suggests that subsidies directly affect families’ decisions to use childcare services.

To put these numbers in context, Figure A3 plots childcare use against relative (women vs. men) employment rates for Canadian provinces and European countries.⁶ The left panel shows that Quebec has one of the highest relative employment rates for women aged 25-34 (the typical child-bearing ages) and the greatest use of formal childcare. Across countries and provinces, there is a positive correlation between the two variables. The right panel shows that informal care use in Quebec is comparable to Scandinavian countries and substantially lower than in the UK, Austria, Italy, and Greece. The scatter plot also indicates that the relative employment rate of women aged 55-64 (e.g., potential new grandmothers) is negatively associated with informal care use.

Finally, International Social Survey Programme data indicate that people in Quebec and the Rest of Canada are equally likely to think children suffer when the mother works and

⁶Variable definitions are not perfectly consistent across European countries and Canadian provinces; hence one must interpret descriptive patterns across datasets with caution.

Figure 1: Percent of children aged 0-5 by type of care and province



Notes: All statistics are based on data for year 2019 in Statistics Canada Tables 42-10-0004-01 and 42-10-0005-01. These summary statistics are produced by Statistics Canada using the Survey on Early Learning and Child Care Arrangements. The horizontal dashed red lines indicate the corresponding fraction of children for all of Canada. Survey respondents who declared participating in child care then indicated which types of child care arrangements they are in. Parents could select multiple options. Both daycare centres and family child care homes are eligible for Quebec’s childcare subsidy program. Other care options included in the survey but omitted from this figure are before and after school programs, care by a non-relative in the child’s home, and other childcare arrangements. These other options are used for 6%, 3% and 2% of children aged 0-5 in Canada, respectively.

whether it is possible for a working mother to have a warm relationship with their child (Appendix Figure A4). In contrast, fewer people in Quebec agree that having children restricts employment and career chances.

2.2 Data

Sample Selection. Our main analyses rely on administrative tax files from Statistics Canada’s Intergenerational Income Database (IID). This database was designed for and is typically used to study intergenerational income mobility (Corak and Heisz, 1999; Connolly et al., 2019). It contains detailed tax data for fiscal years 1978 to 2016 for all Canadians born between 1963 and 1985 (the child generation) and their parents (the parent generation).⁷ Family linkages between parents and children are identified based on Statistics Canada’s T1 Family File (T1FF) for years during which the child is between 16 to 19 years old. Hence, individuals born in Canada are over-represented, as any child who immigrated to Canada after age 19 is necessarily excluded.

From this database, we construct two main analytical samples. To estimate the impact of children on parents, we consider a sample of new parents, including all individuals from the child generation of the IID who became parents before age 40 between 1981 and 2016. We infer the timing of parenthood using the date of birth of dependents, including one’s spouse’s dependents.⁸

To estimate the impact of grandchildren on grandparents, we consider a sample of potential grandparents, including all individuals from the parent generation of the IID aged 40-85 between 1981 and 2016. For each individual, we identify grandparenthood status using each of their children’s previously defined parenthood status. Note that grandparenthood status and its timing are likely measured with some error since some grandparents may have other (unobserved) children from non-IID birth cohorts who are themselves parents. That is, all individuals in this sample are parents, but we may not observe all of their children and therefore not all of their grandchildren. To avoid introducing sample selection based on such non-random measurement error, we include all individuals from the parent generation. 72% of individuals in our sample of potential grandparents are observed with at least one grandchild in the IID.

When examining the impact of Quebec’s childcare reform on mothers, we turn to the Longitudinal Administrative Databank (LAD), a 20% representative sample of tax filers in

⁷Birth cohorts 1971, 1976, and 1981 are not included in the IID.

⁸Dependents are claimed in tax files by only one parent, and we use spousal linkages to identify the parenthood status of non-claiming parents. In the vast majority of cases, a woman claims the dependent. The Data Appendix provides details on the use of spousal links.

Canada, covering all birth cohorts as well as immigrants. It covers fiscal years 1982 to 2018. While the LAD does not permit intergenerational linkages and suffers from smaller sample sizes than the IID, it includes women born before 1962 who became mothers in the 1980s and early 1990s. This feature is essential for examining pre-policy trends in outcomes measured several years after childbirth.⁹ We use the IID to estimate the reform effects on outcomes that necessitate intergenerational linkages (the impacts on grandparents), with the caveat that coverage is imperfect in early pre-policy years.

Variables Definitions. The tax files include each dependent’s exact date of birth, allowing us to infer the year of the first childbirth for each parent. We then examine changes in earnings and employment around the time of childbirth. Earnings include all paid-employment income (wage, salaries, and commissions).¹⁰ To ensure that outliers do not drive results, we top-code annual earnings, assigning the value of the 99th percentile to individuals who earn more than that, separately by calendar year and gender. We also construct an indicator for employment status, which takes a value of one for any positive annual paid-employment income.

In some analyses, we additionally use the place of residence from yearly tax files. For instance, we use the province of residence to estimate the impact of Quebec’s childcare program by comparing Quebec residents with individuals in the rest of the country. We also use longitudinal geographic information to examine residential mobility patterns around the time of childbirth. In particular, for new parents, we create indicator variables for residing in the same census division (which roughly corresponds to a county in the US) as one’s parents—the grandparents—in a given year.¹¹

Tax files also contain information on childcare expenses that have been allowed as a federal tax deduction since 1972. Expenses related to formal childcare services are eligible for these deductions, including payments made to daycare centres, educational institutions for childcare service fees, and day camps. We create an indicator variable for any positive childcare expenses and use it as a proxy for formal childcare use.¹²

⁹The IID covers most births from the mid-1990s onward but has limited coverage for earlier births. For instance, among all births in 1990, the IID only includes those from women who became mothers at the age of 27 or younger (i.e., were born in 1963 or later). Similarly, among children born in 2000, the IID includes women who became mothers at age 37 or younger.

¹⁰Our main measure of earnings (T4 earnings) does not include self-employment earnings, but our results are robust when including them.

¹¹The geographic information in each year is based on the latest available census for each given year. To avoid measurement error from changes in census divisions, we construct time-consistent definitions of census divisions that line up with the 2016 census definitions. The Data Appendix provides details on how we define geographic units.

¹²Generally, childcare expense deductions of a couple are claimed by only one household member. Therefore, our main variable takes a value of 1 whenever individuals or their spouses report any childcare expenses.

Note that the administrative datasets we use only include individuals filing a tax return. Individuals may not be legally required to file taxes if, for example, they have no taxable income. Yet, filing a tax return is necessary to receive social assistance benefits, including child benefits. As a result, tax filing rates are very high, particularly for parents. Individuals who *never* file taxes are necessarily excluded from the datasets. For everyone else, we impute an income of zero in years individuals do not file taxes. Within that sample, we estimate that the average tax filing rate for mothers is around 92% over the 1982-2018 sample period (including pre-motherhood years). In most of our analyses, we make additional sample restrictions based on tax filing behavior. We discuss these restrictions and the associated econometric specifications in the corresponding sections, and examine possible biases due to endogenous tax filing in Appendix section A.3.

3 Child and Grandchild Penalties

As a first step, we estimate child and grandchild penalties in Canada, pooling all provinces and years together. Our empirical approach largely follows the methodology developed in Kleven et al. (2019a).

3.1 Econometric framework

The conventional event-study estimating equation takes the following form:

$$Y_{ist}^g = \alpha^g \mathbf{I}_t^{Event} + \beta^g \mathbf{I}_{is}^{Age} + \gamma^g \mathbf{I}_s^{Year} + v_{ist}^g \quad (1)$$

where Y_{ist}^g is an outcome for individual i of gender $g \in \{m, w\}$, in year s , at event time t . \mathbf{I}_t^{Event} is a vector of event-time dummies, from which the $t = -1$ dummy is omitted. \mathbf{I}_{is}^{Age} and \mathbf{I}_s^{Year} are vectors of age and year dummies, respectively. The inclusion of these controls nonparametrically accounts for life-cycle profiles and general time trends, reflecting business cycle variation or changes in federal policy. The model is estimated separately by gender for the parent and grandparent samples. The estimated event time coefficients α_t^g represent the impact of children at time t relative to the year before becoming a parent for our sample of parents and to the year before becoming a grandparent for our sample of grandparents.

In our preferred specification, we consider an event window of $t \in [-5, 10]$ and focus on parents and grandparents who file taxes every year in this window. We include observations outside the event window and bin the endpoints by including two additional dummy variables for event times $t < -5$ and $t > 10$, as recommended in Schmidheiny and Siegloch (2020).

For grandchild penalties, we further add a dummy variable that takes value 1 for individuals who do not have grandchildren in the IID.

As is customary in the literature, when plotting the results, we convert the estimated event-time coefficients into percentages by scaling them by predicted outcomes that omit the contribution of the event-time dummies:

$$P_t^g = \hat{\alpha}_t^g / E[\tilde{Y}_{ist}^g | t] \quad (2)$$

where $\tilde{Y}_{ist}^g = \sum_k \hat{\beta}_k^g 1\{k = age_{is}\} + \sum_y \hat{\gamma}_y^g 1\{y = s\}$ is the predicted outcome based on age and year dummies alone, for each gender. Standard errors are heteroscedasticity-robust.

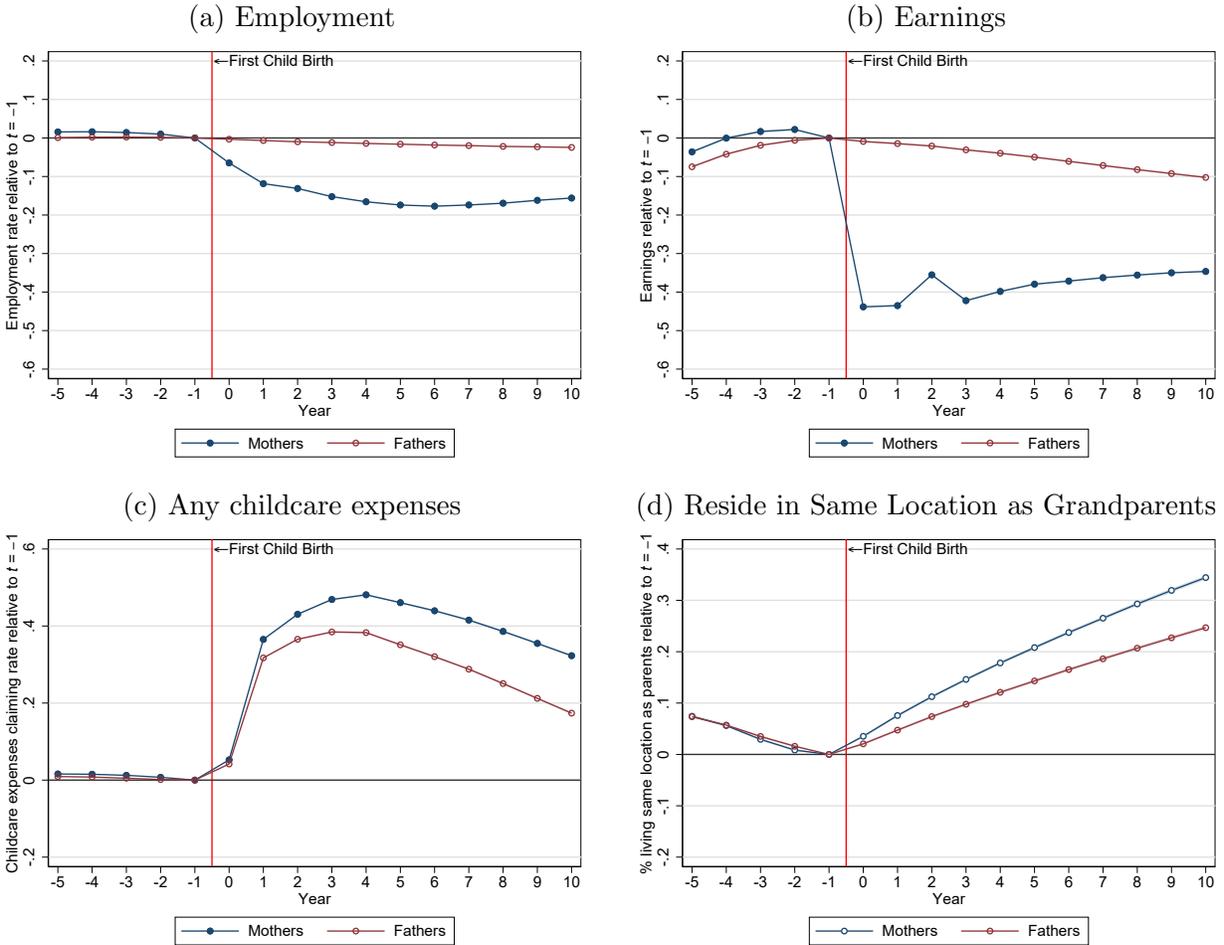
3.2 The Impact of Children on Parents

Figure 2 shows baseline estimates of child penalties on several outcomes for the sample of new parents in the IID. Panels (a) and (b) present results for employment and earnings. They evolve similarly for men and women before having children but diverge sharply in the year the first child arrives. Women’s earnings and employment exhibit an immediate, sizeable, and persistent drop, while men’s earnings and employment decline only gradually. These patterns are comparable to those found in other countries, but the drop in men’s earnings is somewhat larger in Canada, possibly reflecting the impact of children on career decisions in the long run. The long-run earnings penalty (10 years after first childbirth) for mothers relative to fathers is 25%, which is similar to Denmark and Sweden but smaller than Germany, Austria, the U.S., and the U.K. (Kleven et al., 2019b).¹³

Panel (c) shows how formal childcare use changes around the time of childbirth. Childcare take-up jumps sharply at event time 1 when maternity leave expires for most individuals and peaks at 49% at event time 4. Panel (d) examines whether the likelihood of residing in the same census division (CD) as one’s parents changes with childbirth. Before childbirth, both men and women gradually become less likely to live in the same CD as their parents. There is a sharp trend reversal right at event time 0, particularly for mothers, suggesting families value proximity more after the arrival of a child (grandchild). One likely reason is that grandparents may provide informal childcare. These changes can either be driven by new parents moving closer to the grandparents or grandparents moving closer to their new grandchild. We study who moves by examining whether or not parents reside in the same CD at event time t as at birth (see Appendix Figure A6). We infer that in 30% of cases, grandparents are moving because parents continue living in the same CD they resided in at

¹³Since the IID is not a representative sample of the population, we replicate these results in the LAD in Figure A5. The patterns and magnitudes are very similar across datasets and estimation samples.

Figure 2: The Impact of Children on Parents



Notes: This figure reports estimates of child penalties based on equation (1), estimated on a sample of new parents in the Intergenerational Income Database (IID). In panel (a), the outcome is a dichotomous variable for any T4 earnings, and in panel (b) it is T4 earnings. In panel (c) the outcome is an indicator for any childcare expenses (claimed either by the individual or their spouse). Coefficients for that outcome are not normalized since childcare expenses are necessarily equal to zero prior to becoming a parent. In panel (d), the outcome is an indicator for residing in the same Census Division (CD) as one's own parents, and take value 1 if the person reside either in the same CD as their mother or their father. Shaded areas show 95% confidence intervals.

childbirth. Conversely, in 70% of cases, parents move as the CD at event time t differs from the one at childbirth.

Heterogeneous Effects of Children on Parents. To further probe grandparents' role for child penalties, we estimate the impact of children separately for mothers who do and do not live in the same CD as their parents at event time 0.¹⁴ These two subgroups are quite different in several ways. For instance, mothers who live in a different CD than their parents earn more pre-childbirth than those in the same CD. To account for these differences, we apply an Inverse Probability Weighting correction. We construct the weights by running a probit regression of living in a different CD at event time 0 on a set of dummies for the CD and the age at first birth of the mother, an indicator for being employed in the year before birth, and the log of earnings plus one at event times -4 to -1. We then estimate child penalties on these subsamples weighting mothers in the same CD as their parents by $E(\pi_i)/\hat{\pi}_i$ and mothers in a different CD than their parents by $E(1 - \pi_i)/1 - \hat{\pi}_i$, where π_i is the probability of being in the same CD as the parents.

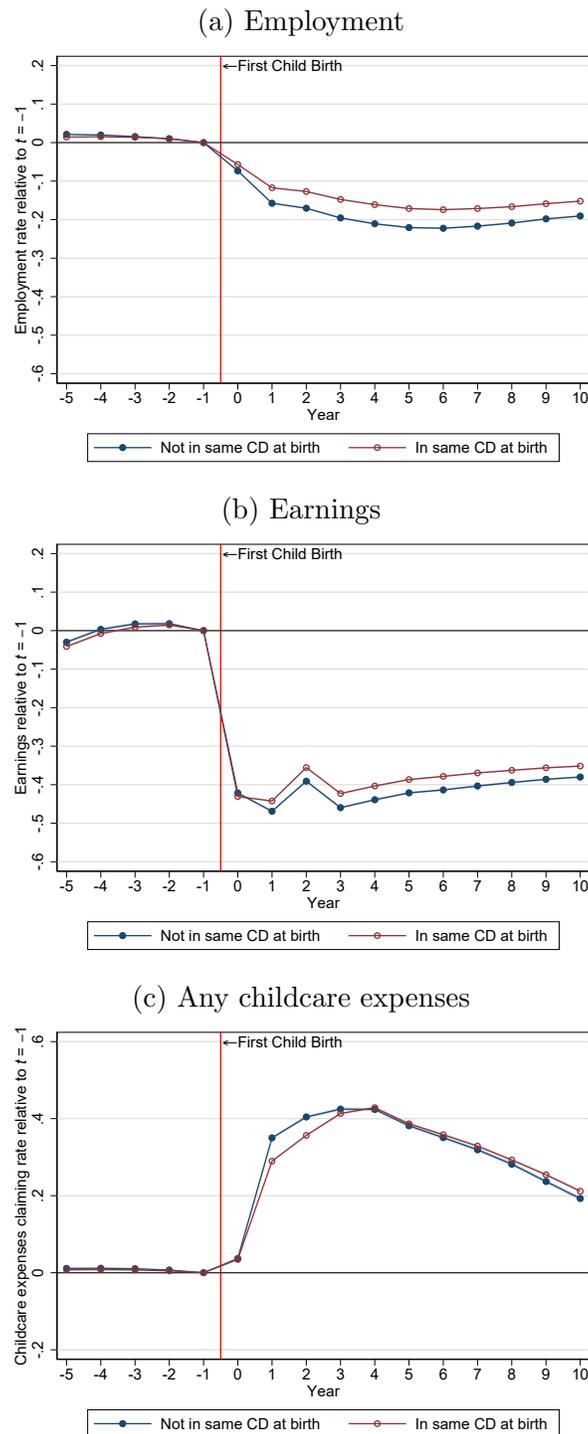
The results reported in Figure 3 show that motherhood effects are significantly lower for mothers in the same CD as their parents. Their employment rate (panel a) and earnings (panel b) are about 4 percentage points higher five years after the first child's birth, and these effects are persistent in the long run. Moreover, panel (c) shows that mothers in the same CD are less likely to have any childcare expenses in the first three years after the first child's birth, consistent with the idea that these mothers can rely on informal childcare by their parents. This gap vanishes at event time 4, when children can start preschool, or full-time kindergarten in Ontario. Many preschools and schools offer after-school programs. The costs for these programs are included in childcare expenses.

In Appendix Figure A7, we additionally examine heterogeneity by whether a mother lives in an urban or rural area and by the number of children a mother ends up having. To account for differences in pre-birth characteristics across subsamples, we perform the same inverse-probability-weighting approach as in Figure 3. We find that mothers in urban areas have slightly smaller motherhood earnings effects. A likely explanation for the more minor impact of children in urban areas is easier access to formal childcare. Consistent with this idea, we find higher take-up rates of formal childcare among mothers in urban areas, especially after the first child enters preschool (event time 4) or kindergarten (event time 5).

The analysis across mothers with 1, 3, or 3 or more children reveals a significant motherhood effect in all three family types. Patterns are similar until event time 1 when maternity

¹⁴We drop CDs in Quebec to make sure that the introduction of subsidized childcare does not confound our comparison.

Figure 3: Heterogeneous Impacts of Children on Mothers by Proximity to Grandparents at Childbirth



Notes: This figure reports estimates of the child penalty for mothers, separately for those who do and do not live in the same Census Division as their own parents at childbirth (at $t = 0$). The sample consists of new mothers in the Intergenerational Income Database (IID). Inverse probability weights are used to account for observable differences in pre-birth characteristics between the two samples. Shaded areas show 95% confidence intervals.

leave typically expires: earnings and employment rates drop by 41-45% and 11-14%, respectively. Earnings start to diverge sharply after event time 1 as more children arrive in larger families. The long-run impact of motherhood on earnings at event time 10 increases by about 10% per additional child, similar to the estimate by Kleven et al. (2019a) for Denmark. Interestingly, the long-run employment impact is similar for mothers with 1 and 2 children but twice as large for mothers with 3 children, suggesting that these mothers anticipated to have many children and decided to withdraw from the labour market permanently. Differences in childcare take-up rates by family types tell a similar story: Mothers with 3 or more children are significantly less likely to take up formal childcare, even at early event times. In contrast, mothers with 1 or 2 children have similar take-up rates.

3.3 The Impact of Grandchildren on Grandparents

Figure 4 shows the effect of grandparenthood on earnings and employment. The trend in employment is perfectly flat for grandparents before grandparenthood but turns negative precisely after the arrival of the first grandchild. Here, we find evidence of a long-run grandchild employment penalty: employment at event time 10 drops by 8% for grandmothers and by 6% for grandfathers. The trend in earnings of grandmothers and grandfathers is similar and relatively flat before parenthood but turns negative after the first grandchild's birth. The decline is slightly steeper for grandmothers, particularly after maternity leave expires at event time 1, but the gap gradually closes again. The long-run earnings impact of grandparenthood at event time 10 is 16% for grandmothers and 15% for grandfathers. The magnitude of our estimated impact of grandchildren on earnings is larger than the estimates in Gørtz et al. (2020) for Denmark. For example, they estimate that grandmothers' earnings at event time 10 drop by 10% compared to 16% in Canada. Moreover, they find that the earnings impact is about four times larger among grandmothers than grandfathers.

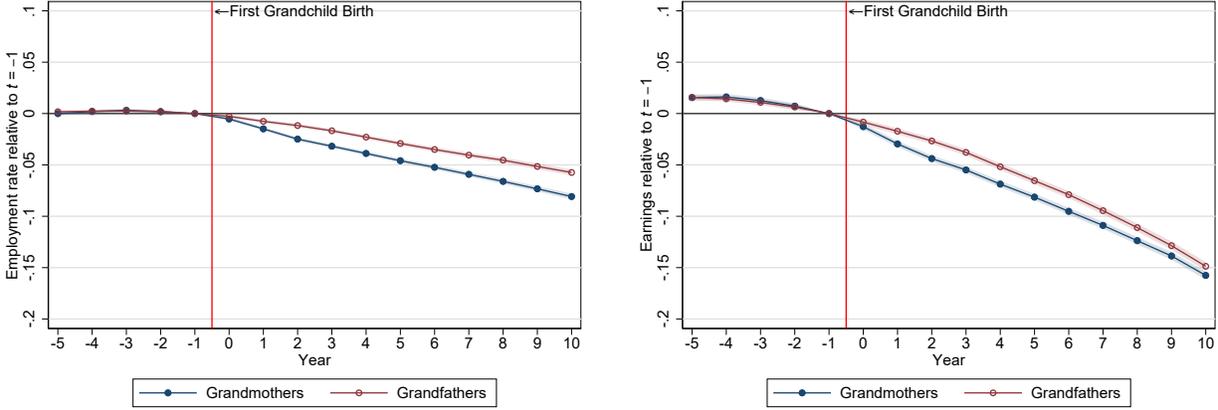
We also perform a robustness check of the grandparents' penalties using a difference-in-differences event study. This design uses individuals who we do not observe having grandchildren as a control group. Following Kleven et al. (2019a), we assign placebo grandchildren to individuals without grandchildren, drawing from the observed distribution of age at first grandchild among grandparents.¹⁵ As Appendix Figure A8 shows, the difference-in-

¹⁵The distribution of age at first grandchild is approximated by a log-normal distribution within cells of birth cohorts. The mean and the variance are estimated from the actual age distributions within each cohort cell. Individuals without grandchildren get a random draw from this distribution, censored from below at age 40 and from above at age 75. We then estimate the following difference-in-differences event study

$$Y_{ist}^g = \alpha^g \mathbf{I}_t^{Event} \cdot T_i + \beta^g \mathbf{I}_t^{Event} + \gamma T_i + \delta^g \mathbf{I}_{is}^{Age} + \eta^g \mathbf{I}_s^{Year} + v_{ist}^g,$$

where T_i is an indicator for grandparents.

Figure 4: The Impact of Grandchildren on Grandparents
 (a) Employment (b) Earnings



Notes: This figure reports estimates from equation (1), estimated on a sample of potential grandparents in the Intergenerational Income Database (IID). In panel (a), the outcome is a dichotomous variable for any T4 earnings, and in panel (b) it is T4 earnings. Models are estimated separately by gender. Shaded areas show 95% confidence intervals.

differences event study estimates resemble our baseline results very closely: The long-run employment penalty (at event time 10) is 8% for grandmothers and 5% for grandfathers. The long-run earnings penalty is 19% for grandmothers and 18% for grandfathers.

As grandparenthood is associated with large and persistent earnings and employment reductions, a natural question to ask is whether grandparents compensate for some of the associated income losses by taking up pension benefits. Appendix Figure A9 shows event study estimates where the outcome variable is a dummy for any pension income (panel a) and public pension income (panel b). Coefficients for these outcomes are not normalized because pension take-up is low in our sample as many grandparents are too young to be eligible for a public pension. The figure shows that both pension take-up and pension income steadily increase after becoming a grandparent.

Heterogeneous Effects of Grandchildren on Grandparents. For most grandchildren in our sample, we can observe maternal and paternal grandparents as both parents belong to the IID child generation. Kleven et al. (2019a) find that female child penalties strongly correlate with the labor supply history of maternal grandparents but not paternal grandparents. Given this asymmetry, we might expect that grandchild penalties differ for maternal and paternal grandparents. Indeed, the estimates reported in Appendix Figure A10 show that short-run employment and earnings penalties in the first years after the arrival of a grandchild are larger for maternal than paternal grandmothers. But the gap closes over time, and by event time $t = 10$, maternal and paternal grandmothers experience the same

penalty. In contrast, penalties are similar for maternal and paternal grandfathers. If at all, earnings penalties are slightly greater for paternal than maternal grandfathers, but the difference is not statistically significant.

In Appendix Figure A11, we further explore heterogeneity by estimating grandmothers' penalties separately for various subgroups. Overall, we find effect sizes to vary little across different sets of grandmothers. We find that grandmother penalties are similar independent of whether the child who first became a parent is a daughter or a son. We see more significant employment penalties for single grandmothers and grandmothers with fewer children, but the earnings penalties are the same. The characteristic along which effect sizes vary the most is whether grandmothers reside in the same CD as parents or not. Grandmothers not residing in the same CD experience larger employment and earnings penalties, possibly reflecting the impact of longer commuting times or grandmothers moving closer to the parents.

3.4 Spatial Differences in the Impact of Children

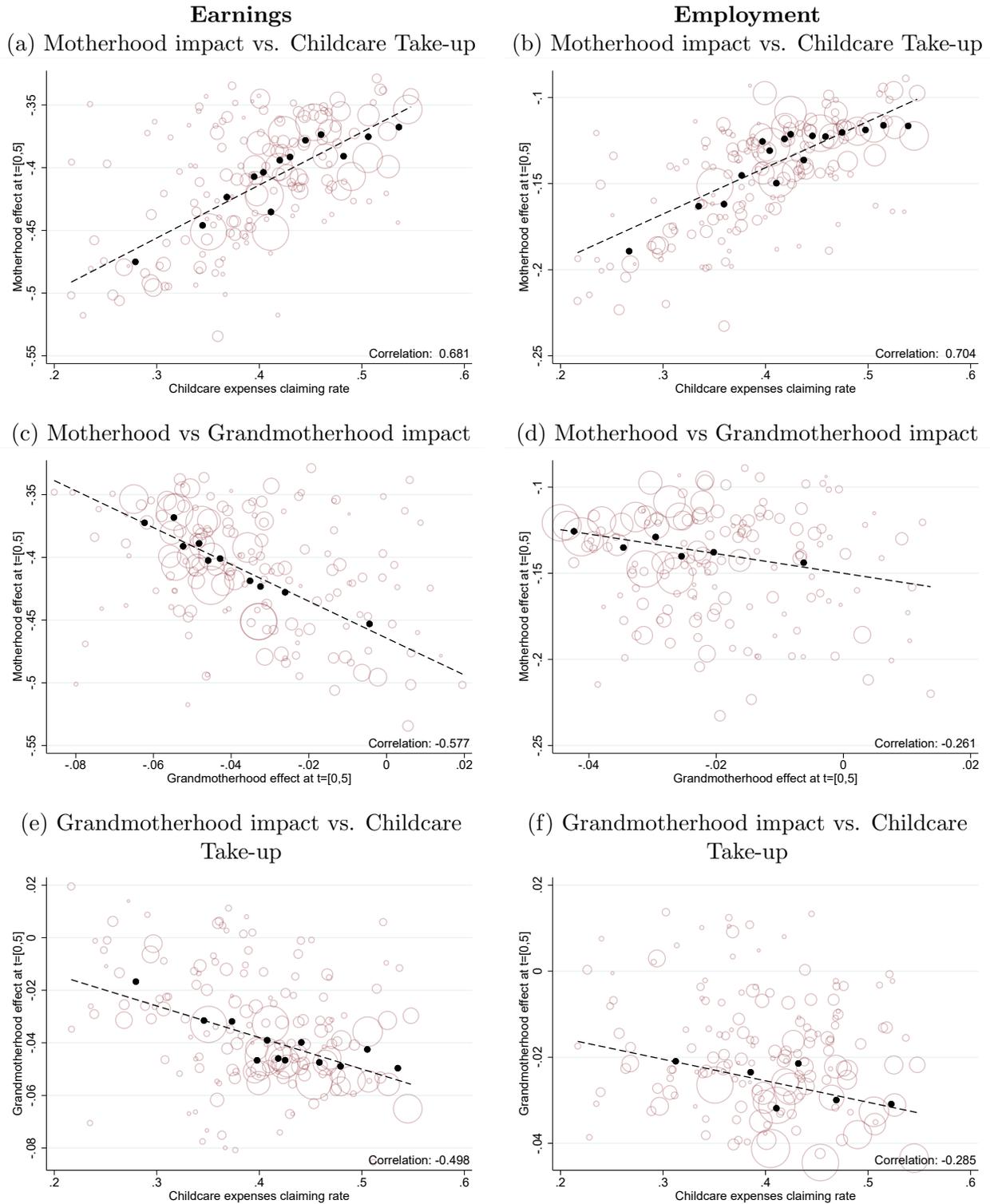
We conjecture that larger earnings and employment drops around the arrival of the first (grand)child reflect a greater allocation of time towards child care. If parent- and grandparent-provided care are substitutable, one would expect the magnitude of the impact on mothers and grandmothers to be negatively correlated. To test this hypothesis, we estimate summary measures of the impact of children on earnings and employment separately for each census division (CD) in Canada. To do so, we slightly modify equation (1), replacing the 15 event-time dummies with 3 dummies pooling event-times -5 to -2, 0 to 5, and 6 to 10 (the omitted category being event-time -1). Because the impact of children and grandchildren is considerably larger on women than men, we focus on mothers and grandmothers moving forward.

Figure 5 presents area-specific estimates for mothers at event times 0 to 5. In panels (a) and (b), these motherhood effects are plotted against the share of families claiming childcare expenses. In panels (c) and (d), motherhood effects are plotted against grandmotherhood effects. In all cases, we only show CDs outside of Quebec to make sure patterns are not driven by the Quebec childcare policy.¹⁶ Each hollow circle represents one CD, and the size of the circle indicates the number of observations. For visual clarity, we overlay binscatter plots (black dots), where the size of bins is selected using methods developed by Cattaneo et al. (2019).

As predicted, places with greater formal childcare use have considerably smaller motherhood effects, on average. For earnings, doubling the childcare expenses claiming rate from

¹⁶For completeness, we show corresponding graphs that include Quebec in Figure A12. We also show maps for each of the three variables in Figures A13, A14, and A15.

Figure 5: Spatial Correlations of the Impacts of Children



Notes: This figure presents estimates of motherhood and grandmotherhood effects for all Canadian census divisions, excluding those in Quebec. In panels (a), (b), (e) and (f), the variable on the horizontal axis is the average childcare expenses claiming rates by families at event times 0 to 10. Each red circle represents one Census Division, and the size of the circle is proportional to the number of observations. Black dots represent a binscatter plot. For visual clarity, outliers census divisions (values below the 1st percentile or above the 99th percentile of the distribution) are dropped.

25% to 50% is associated with a reduction in the motherhood impact from 48 percent to 37 percent. For employment, the same increase in formal childcare use is associated with a drop in the motherhood effect from 18% to 11%.

Consistent with the idea that grandparent-provided care can reduce the impact of children on mothers, we find that motherhood and grandmotherhood effects are negatively correlated across CDs. Here, reducing the impact of grandmothers' earnings from 6 percent to 0 percent is associated with an increase in mothers' earnings of 9 percentage points, from 37 percent to 46 percent. The relationship is considerably weaker for employment.

Finally, we plot grandmotherhood effects against childcare expenses claiming rates in panels (e) and (f). While there is no relationship for employment, a weak negative association emerges between childcare claiming rates and impacts on grandmothers' earnings. In other words, in places where families use more formal childcare, grandmothers exhibit slightly greater earnings reductions following the birth of a grandchild. This pattern suggests that formal and informal care by grandparents could be complementary modes of care (Gathmann and Sass, 2018).

4 The Effects of Childcare Subsidies With Multiple Modes of Care

Having established that the arrival of a (grand)child reduces both parents' and grandparents' labour supply, we now develop a framework to predict how childcare subsidies impact labour supply in a setting with multiple modes of care. It shows that the effects of childcare subsidies are heterogeneous and depend on the counterfactual mode of care in the absence of the subsidy. We later test the theoretical predictions using the Quebec childcare reform.

4.1 A Framework for Analyzing the Impact of Childcare Subsidies

Suppose the main caregiver of a child is either a parent (p), a grandparent (g), or a formal daycare centre (c). Let $S_i \in \{0, 1\}$ denote whether family i is eligible for childcare subsidies (i.e. resides in Quebec after 1997) and $D_i(s) \in \{p, g, c\}$ denote family i 's potential care status as a function of childcare subsidies.

Following Kline and Walters (2016), we assume that $D_i(1) \neq D_i(0) \Rightarrow D_i(1) = c$. The restriction implies that families who switch modes of care in response to childcare subsidies must switch to formal childcare. It rules out that childcare subsidies induce families to switch between parental and grandparental care or to switch from formal to informal childcare.

Under this restriction, families can be partitioned into five groups defined by their potential modes of care with and without childcare subsidies:

1. p -compliers: $D_i(1) = c; D_i(0) = p$,
2. g -compliers: $D_i(1) = c; D_i(0) = g$,
3. p -never takers: $D_i(1) = D_i(0) = p$,
4. g -never takers: $D_i(1) = D_i(0) = g$,
5. always takers: $D_i(1) = D_i(0) = c$.

This partition illustrates that families eligible for the childcare subsidies who opt for parental care are p -never takers and those who use grandparental care are g -never takers. In contrast, families who use formal childcare when eligible for the subsidy consist of always takers, p -compliers, and g -compliers. As different groups of families use formal childcare under the subsidy, the population average impact depends on two factors: the group-specific response to the subsidy and the size of each group.

We first discuss group-specific effects, and describe how we estimate group sizes in the next section. First, assuming daycare is a fixed cost of working, subsidies can draw counterfactual caretakers who otherwise stay home into the labour force, increasing their employment and earnings (e.g., mothers for p -compliers and grandmothers for g -compliers). Second, subsidies produce an income effect for always takers, possibly reducing mothers' hours of work and earnings. Third, if daycare is not perfectly reliable (e.g., sick children cannot attend daycare and must remain in parental or grandparental care), then subsidies may reduce the earnings of *secondary* caretakers in complier families who have to take more days off work to care for a sick child. For example, p -complier families may use informal care provided by grandmothers as a complement for imperfect formal care, reducing grandmothers' earnings in p -complier families. Thus, formal care and informal care by grandparents are substitutes for g -compliers (by definition), but can complement each other in p -complier families.

Table 1 summarizes our predictions on the *sign* of the employment and earnings effects of childcare subsidies. The average employment effect is unambiguously positive for mothers and grandmothers. Its magnitude depends on the share of p - and g -compliers.¹⁷ In contrast, the overall impact on mothers' and grandmother's earnings is ambiguous. For grandmothers,

¹⁷Some grandparents may have to withdraw entirely from the labour force to provide infrequent care in p -compliers families. The associated employment effects are likely small given that the supply of childcare hours by relatives only goes up at 5 hours per week or less, as we show below.

Table 1: Predicted Effects of Childcare Subsidies on Employment and Earnings

	Mothers'		Grandmothers'	
	Employment	Earnings	Employment	Earnings
<i>p</i> -compliers	+	+	0	-
<i>g</i> -compliers	0	-	+	+
always takers	0	-	0	0
<i>p</i> -never takers	0	0	0	0
<i>g</i> -never takers	0	0	0	0
Average	+	? (+)	+	? (-)

Notes: The sign in parentheses indicates our best guess as discussed in the main text.

we expect earnings to decline because there are likely more *p*-compliers than *g*-compliers.¹⁸ For mothers, the sign of the earnings response also depends on the share of always takers. We expect a positive net effect if the income effect for always-takers is small.

Table 1 also allows drawing testable predictions about the *magnitude* of the effects as a function of the group shares. First, the magnitude of mothers' employment response should increase with the share of *p*-compliers, but decrease with *g*-compliers. Second, the effects should be closer to zero the larger the shares of *p*- and *g*-never takers. We test these predictions in section 6 by estimating the employment and earnings response separately for each census division within Quebec. We then correlate these responses with the estimated group shares in each census division.

4.2 Estimating Complier Shares Using the Quebec Childcare Reform

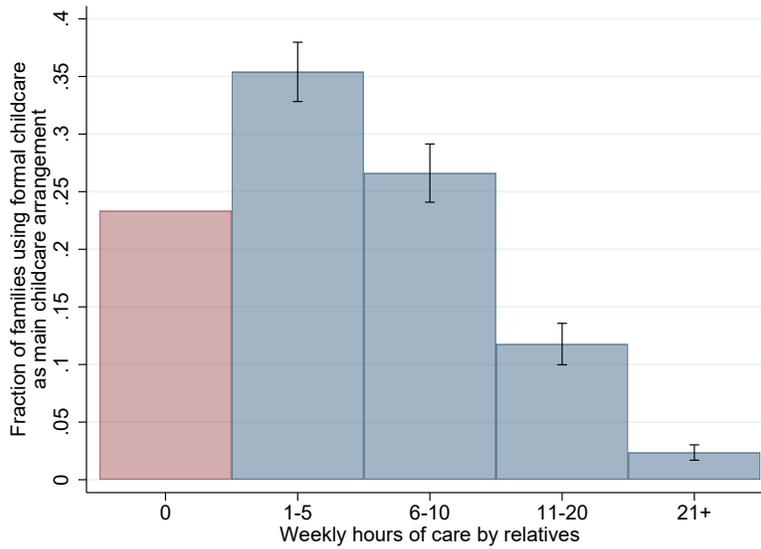
We use data from the National Longitudinal Survey of Children and Youth (NLSCY) to estimate the share of *p*- and *g*-compliers.¹⁹ NLSCY survey participants are asked whether they use any of several different types of (non-parental) childcare arrangements and list the weekly number of hours of care received under each arrangement.

We exploit the weekly number of hours of care provided by relatives (most likely grandparents) to examine the complementarity and substitutability between formal and informal

¹⁸Moreover, if families select counterfactual main caregivers based on their opportunity costs of caring for young children, we would expect grandmothers in *g*-complier families to have a lower earning potential than those in *p*-complier families.

¹⁹These data have previously been used by Baker et al. (2008) and Haeck et al. (2015) to study the effect of Quebec's childcare reform on mothers' labour market participation and children's cognitive and behavioral outcomes. Description of these data and of the empirical specifications implemented below are described in Appendix B.

Figure 6: Joint Use of Formal Care and Informal Care by Relatives

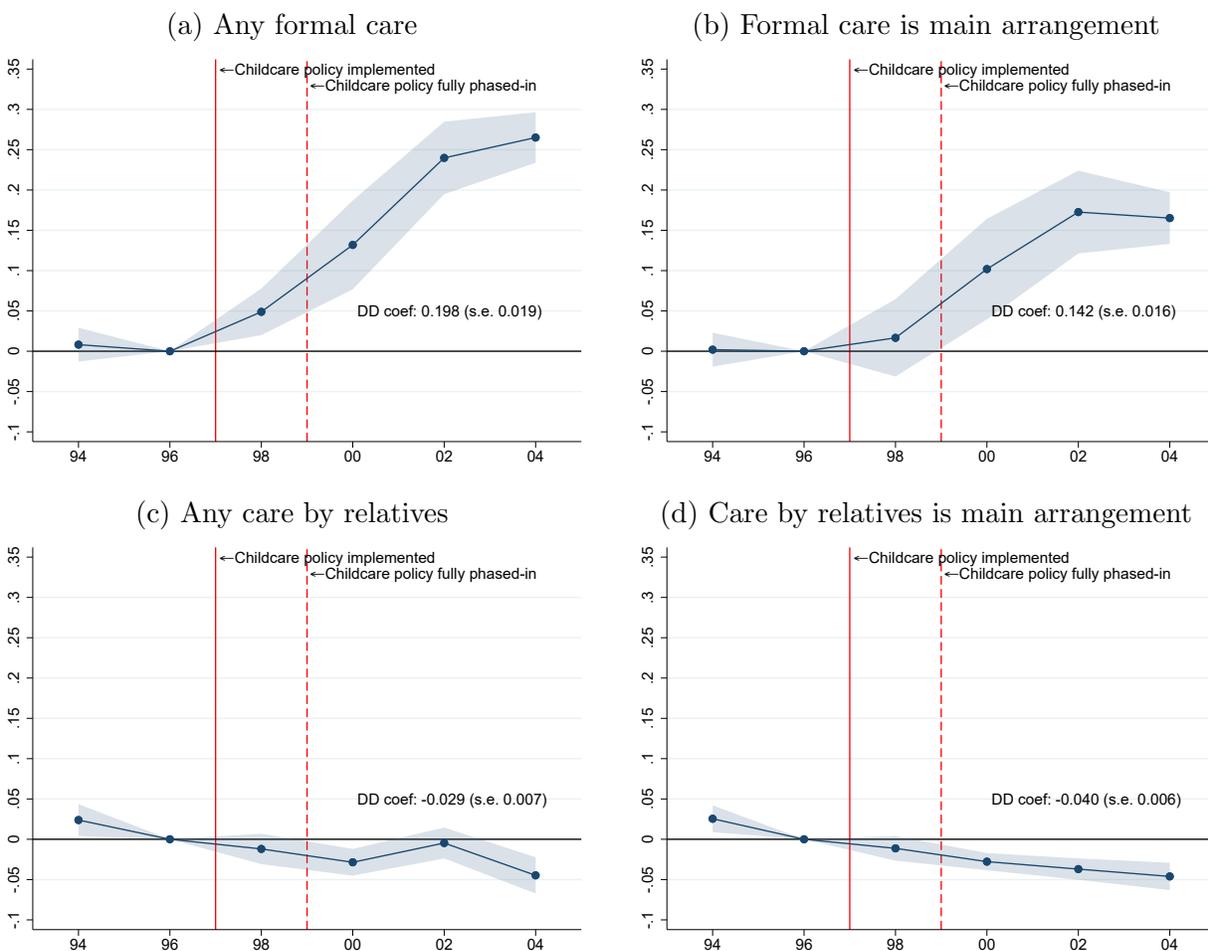


Notes: The figure plots the fraction of families of children aged 0-5 whose main childcare arrangement is either a formal childcare centre or a dayhome, as a function of the weekly number of care provided by relatives. The statistics are based on data pooling all first 6 waves of the NLSCY (covering years 1994 to 2006). Whiskers represent 95% confidence intervals for the difference between a given group and the baseline category of 0 hours of care by relatives.

care. Figure 6 shows how the propensity to use any formal childcare varies with the number of hours of informal care provided by relatives. The relationship is non-monotonic. Going from zero to 1-5 weekly hours of care by relatives increases the likelihood of using formal childcare, but any further increase in the hours of care by relatives reduces formal childcare use. These patterns suggest that formal care complements infrequent care provided by grandparents, but substitutes intensive care provided by grandparents.

We next examine how modes of cares for children aged 0-5 change around the time of the reform in Quebec relative to the Rest of Canada. These changes are informative of the share of p and g -compliers. Figure 7 plots event study graphs of the Quebec-RoC differential for different modes of care. Panels (a) and (b) show changes in the likelihood of using any formal childcare and using formal childcare as the main care arrangement. The policy effects on these outcomes approximate the combined share of p and g -compliers. We estimate that 14 to 20% of the sample are complier families. Panels (c) and (d) reproduce these analyses for care provided by relatives. Here, the policy effect reflects the fraction of g -compliers. We find that about 3 to 4% of the sample are g -complier families, with the caveat that there is a slight negative pre-trend. Overall, we find that that there are about four times more p -complier families than g -compliers.

Figure 7: Policy Effects on Choice of Mode of Care



Notes: Outcomes in panels (b) and (d) are based on survey questions asking parents what is their main childcare arrangement. Outcomes in panels (a) and (c) are based on questions pertaining to the number of weekly hours of care from different sources. In panel (a), the outcome is an indicator for any use of either centre-based care or a licensed dayhome. In panel (c), the outcome is an indicator for any use of care by relatives. Shaded areas show 95% confidence intervals, where standard errors are clustered by census metropolitan areas. The estimating equation is described in further detail in Appendix B.

Moreover, Appendix Figure A16 plots difference-in-differences estimates of how the Quebec reform affects the frequency of informal care by relatives. All policy effects are negative, except for the 1 to 5 hours per week category, for which the effect is positive. Thus, the introduction of childcare subsidies in Quebec increased the frequency of low-intensity use of relative-provided care. In contrast, the policy decreased the supply of childcare by relatives in the range of 6 or more hours per week. These results corroborate that childcare policies change both parents’ and grandparents’ supply of informal care and that formal care can substitute or complement informal care.

5 The Impact of Childcare Policies on Mothers and Grandmothers

This section examines the impact of Quebec’s childcare subsidies program on women’s labour market outcomes. We first use the LAD to document the effect of the policy on mothers’ earnings and employment and validate that our results are consistent with prior studies. We then use the IID to document whether the policy affects grandmothers’ labour market outcomes.

5.1 Average Impact on Mothers’ Earnings and Employment

Empirical Specification. Our main specification is a triple-difference design, where we compare across provinces, time, and parenthood status. We estimate the following equation

$$\begin{aligned}
 Y_{ist} = & \alpha^{Qs} (P_t \times QC_{is} \times \mathbf{I}_s^{Year}) + \alpha^s (P_t \times \mathbf{I}_s^{Year}) \\
 & + \gamma^p (\mathbf{I}_s^{Year} \times QC_{is}) + \alpha^p (P_t \times QC_{is}) + \beta \mathbf{X}_{is} + v_{ist}.
 \end{aligned} \tag{3}$$

where QC_{is} indicates residing in Quebec in year s , and $P_t = \mathbf{I}\{t \geq 0\}$ is an indicator for being a parent (i.e., being at event time 0 or later). \mathbf{I}_t^{Year} is a vector of calendar year dummies and \mathbf{X}_{is} is a vector of covariates that includes age and census division dummies. We restrict the sample to observations at event times $t \in [-5, 10]$. Individuals who never have children are therefore excluded. We focus on the years 1990 to 2013 to ensure we observe a sufficient number of mothers at all event times in all calendar years. We also restrict the sample to individuals who file taxes at least 90% of the time.²⁰

²⁰Tax filing incentives shifted in the early 1990s when the Canada Child Tax Benefit was introduced. This change may have affected parents’ tax filing behavior differently in Quebec and the rest of Canada. Appendix A.3 discusses endogenous tax filing in more detail.

The coefficients of interest, α_s^{Qs} , measure how the impact of children—the differences in outcomes between mothers and yet-to-be mothers—for mothers living in Quebec in year s differs from mothers living in the rest of Canada. These year-specific treatment effects are relative to 1996, which is omitted. We express coefficients in percent changes, re-scaling them by predicted outcomes $E[\tilde{Y}_{ist}|P_t = 1, QC_{is} = 1]$. Standard errors are clustered at the census division level.²¹

Our specification differs from difference-in-differences designs in prior work (e.g., Haeck et al., 2015; Baker et al., 2008) in that we use yet-to-be mothers as an additional comparison group. The identification relies on differential trends between women who have children and could benefit from childcare subsidies and women who do not have a child but will eventually have one. This approach allows us to account for differential trends in women’s employment between Quebec and the rest of Canada (i.e., changes common to women with and without children).

Main Results and Robustness. Figure 8 presents our main estimates of the post-childbirth employment and earnings impacts of Quebec’s childcare policy.²² Mothers see weak increases in earnings and employment during the phase-in years, consistent with many not yet being eligible for childcare subsidies. Once the policy is fully phased-in in 2000, earnings and employment of mothers increase by roughly 2.5 percent in Quebec relative to the rest of Canada. The positive effect on employment further grows over time, reaching close to 5 percent by 2013. The impact on earnings stabilizes at around 4 percent. Pre-policy trends are flat for earnings, but there is bump in employment rates in 1993. Appendix A.3 shows that the bump is likely not a real employment response but is rather due to differential changes in tax filing between mothers and yet-to-be mothers when the 1993 Child Tax Benefit was introduced.

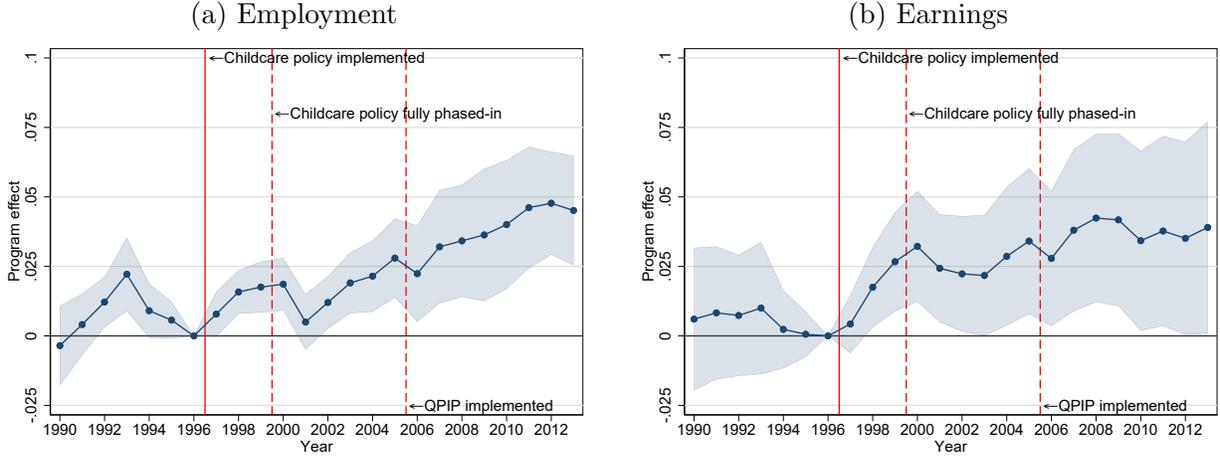
One concern is that families may strategically relocate to Quebec post-birth to become eligible for the subsidies. To address the endogeneity of residence after childbirth, we consider an alternative specification that recodes QC_i as a time-invariant dummy based on residing in Quebec at childbirth. Appendix Figure A18 shows that results are robust to this alternative coding.

Our main outcome variables are based on T4 earnings, capturing earnings from employment. Lloyd (2020) shows that parenthood is associated with an increase in the likelihood of

²¹Analytical standard errors clustered at the province level are likely downward biased since there are only 10 provinces in Canada. Given large differences in motherhood effects across census divisions, we instead cluster at that level to account for serial correlation in error terms.

²²Appendix Figure A17 shows that the policy increased formal childcare take-up after the birth of the first child by about 30 percentage points in Quebec. In the rest of Canada, formal childcare take-up increased by only 10 percentage points over the same period.

Figure 8: Effect of Quebec’s Childcare Policy on Mothers’ Employment and Earnings



Notes: This figure reports the estimated policy effect of Quebec’s childcare program on mothers’ earnings and employment, based on equation (3). All estimates are based on samples of mothers (including yet-to-be mothers) in the Longitudinal Administrative Database (LAD). All regression coefficients are scaled by predicted values and represent percent changes relative to the baseline year 1996. Shaded areas show 95% confidence intervals.

self-employment. To allow for this additional margin of adjustment, Appendix Figure A19 shows estimates of the policy effects on any employment (including self-employment) and total work earnings (from both employment and self-employment). The employment response including self-employment is larger, while the earnings effects are largely unchanged.

Anticipatory and Cohort Effects. We next exploit the longitudinal aspect of the data to evaluate anticipatory effects, i.e., whether mothers respond to the policy change before childbirth.²³ For this purpose, we modify the event-study specification in equation (1) and allow the event-time dummies to vary flexibly by year and treatment status (Quebec vs. Rest of Canada). The estimating equation is

$$\begin{aligned}
 Y_{ist} = & \alpha^{Qs} (I_t^{Event} \times I_s^{Year} \times QC_{is}) + \alpha^s (I_t^{Event} \times I_s^{Year}) \\
 & + \alpha^Q (I_t^{Event} \times QC_{is}) + \beta X_{is} + v_{ist}.
 \end{aligned} \tag{4}$$

The coefficients of interest, α_{ts}^{Qs} , indicate how outcomes in Quebec at event time t and year s differ from outcomes in the rest of Canada at that same event time and year. This stacked approach is similar to estimating separate difference-in-differences models for each

²³Prior studies document anticipatory responses to maternal leave policies in California (Baum and Ruhm, 2016; Byker, 2016) and Switzerland (Girsberger et al., 2021).

event time but allows us to express coefficients in percentages by re-scaling them by predicted outcomes $E[\tilde{Y}_{ist}^g | t, QC_{is} = 1]$. It relies on a stronger identifying assumption than the triple difference approach. The estimates are only valid under the usual parallel trend assumption that outcomes for mothers at event time t would have evolved the same way in Quebec and in the rest of Canada in the absence of the policy. Note that estimating year-by-event time coefficients is equivalent to estimating year-by-birth cohort coefficients. Hence, when tracing out the evolution of outcomes at event time t across years, we effectively compare women who became mothers in different years (different childbirth cohorts). In Appendix C, we examine whether results are driven by cohort effects by converting our estimate of α_{ts}^{Qs} in birth cohort groups rather than event time groups.

Appendix Figure A20 presents the results, pooling event-times $t \in [-5, -1]$, $t \in [1, 5]$, and $t \in [6, 10]$.²⁴ We also show estimates for event-time $t = 0$ to examine the potential confounding effects of QPIP, which should mostly affect earnings in the year mothers are eligible for parental leave.

Employment rates of yet-to-be mothers in Quebec ($t \in [-5, -1]$) increase by about 2 percent by 2000, suggesting many women change career plans in response to the childcare policy even before they become mothers. This also suggests that previous results based on the triple-difference specification, which differences out gains for yet-to-be mothers, may understate the total effect of the policy on employment. We find no apparent employment effect of the policy in the year of childbirth ($t = 0$). Employment then raises by 4 percent in the 2000-2005 period and increases further in later years.

We find no clear anticipatory effects on earnings, but the impact of QPIP on earnings at childbirth is visually striking. Earnings at $t = 0$ decline significantly after 2005. QPIP likely also contaminates post-childbirth earnings estimates as most mothers have more than one child and consequently claim parental leave benefits at event times $t > 0$. Earnings at event times $t \in [1, 5]$ and $t \in [6, 10]$ increase by about 4 percent between 2000 and 2005, but they are substantially attenuated once QPIP is introduced, particularly at event times $t \in [1, 5]$. Overall, given the discernible confounding impact of QPIP, we only consider years 2000-2005 as the relevant post-treatment period when summarizing the impact of the childcare policy in later sections.

Overall, our results are consistent with prior evaluations of Quebec’s childcare policy. We find substantial employment and earnings gains for mothers with young children.²⁵ What

²⁴That is, we estimate coefficients separately for each event-time, but report linear combinations of these coefficients for conciseness.

²⁵Our estimates for the extensive margin appear somewhat smaller than those reported in Baker et al. (2008) and Haeck et al. (2015). A likely reason is that our employment measure is based on having worked at any time during a fiscal year, whereas previous estimates are for point-in-time participation rates, which

do our estimates imply for the overall motherhood effect? Appendix Figure A21 takes the average effect between 2000 and 2005 for each event time t and adds those estimates to the pre-policy (1982-1996) motherhood effect for Quebec mothers. Childcare subsidies do attenuate the earnings impact of children: Counterfactual earnings between event times 1 to 10 are roughly 3.3 percentage points smaller with the added program effect than the pre-policy estimates, equivalent to a 8% reduction relative to the baseline earning drop of 43 percentage points. Subsidies do also attenuate the employment impact of children: the policy increases employment at event times $t \in [6, 10]$ by roughly 4 percentage-points, a 12% reduction of the baseline employment drop of 33 percentage points.²⁶

5.2 Impact on Grandmothers' Earnings and Employment

Empirical Specification. To estimate the impact of childcare subsidies on grandmothers' outcomes, we consider a slightly modified version of equation (3). The model is essentially a triple-difference design that exploits differences over time, across provinces, and between women of the same age that differ in whether they currently have at least one young grandchild or not. The estimating equation is:

$$Y_{ist} = \alpha^{Qs} (GP_{ist} \times \mathbf{I}_s^{Year} \times QC_{is}) + \alpha^s (GP_{ist} \times \mathbf{I}_s^{Year}) + \gamma^p (\mathbf{I}_s^{Year} \times \mathbf{I}_i^{Prov}) + \alpha^p (GP_{ist} \times \mathbf{I}_i^{Prov}) + \beta \mathbf{X}_{is} + v_{ist}. \quad (5)$$

where GP_{ist} is a dummy that takes value 1 if $t \in [1, 10]$ and the grandparent lives in the same province as their first grandchild in year s , and zero otherwise. We impose the residence-based condition because among grandparents residing in Quebec only those whose grandchild also lives in Quebec should be affected by the program. \mathbf{I}_i^{Prov} is a vector of province dummies, which we interact with a vector of calendar year dummies \mathbf{I}_i^{Year} to account for province-specific changes in retirement policies. \mathbf{X}_{is} is a vector of covariates (census division dummies, age dummies, dummies for age at first childbirth, and pairwise interactions of these age dummies with year dummies).²⁷ We restrict the sample to women aged 50-70 who file taxes at least 90% of the time. As before, the coefficients of interest are the α_s^{Qs} ,

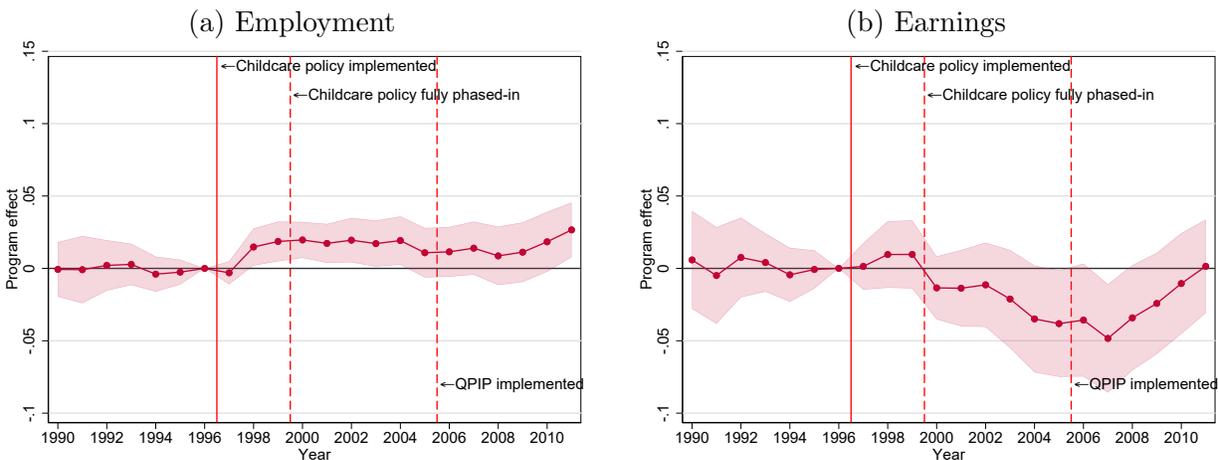
have much lower base rates.

²⁶It is also worth noting that these estimates may understate the positive effects of childcare subsidies on earnings and employment if there were positive effects of the program on fertility, which other work suggests was the case (e.g. Lee and Liu (2022), Zhao (2021) and Lacroix et al. (2017)).

²⁷Conditional on age dummies, potential grandparents who became parents earlier are more likely to become grandparents earlier. The indicators for age at first childbirth account for this source of cross-sectional heterogeneity.

indicating how the impact of having a first grandchild between age 1 and age 10 in year s differs between Quebec and the rest of Canada. We report the results in percentages terms by re-scaling the estimated coefficients by predicted outcomes $E[\tilde{Y}_{ist} | GP_{ist} = 1, QC_{is} = 1]$.

Figure 9: Effect of Quebec’s Childcare Policy on Grandmothers’ Employment and Earnings



Notes: This figure reports the estimated policy effect of Quebec’s childcare program on grandmothers’ earnings and employment, based on equation (5). All regression coefficients are scaled by predicted values and therefore represent percent changes relative to year 1996. The treatment group is grandmothers whose oldest grandchild is between the age of 1 and 10 and reside in the same province as their grandchild. Shaded areas show 95% confidence intervals.

Main Results and Robustness. Figure 9 shows estimates of the effect of Quebec’s childcare program on grandmothers’ earnings and employment. As before, we scale regression coefficients by the counterfactual outcomes so that effects are expressed in percent. In line with our predictions, employment increases by about 2 percent following the policy’s implementation and remains at that level in the long run.²⁸ In contrast, earnings start declining when the program is fully phased-in, consistent with low-intensity informal care complementing formal care. After the implementation of QPIP, grandmothers’ earnings recover, plausibly because some working parents remain on parental leave longer, reducing the need for infrequent grandparent-provided care.

In Appendix Figure A22, we split the grandparent dummy into two: one dummy for event times $t \in [1, 5]$ and one dummy for event times $t \in [6, 10]$. The pre-policy trends are very imprecise at $t \in [6, 10]$ because of the IID’s poor coverage of later event times in the early 1990s. Nevertheless, the patterns are qualitatively similar for both sets of event

²⁸The counterfactual employment rate of grandmothers is lower than it is for mothers. A 2 percent increase for grandmothers roughly corresponds to a 1 percentage point increase in levels.

times, showing sustained increases in employment post-policy, as well as gradual declines in earnings between 1999 and 2006.

6 Heterogeneous Effects of Childcare Subsidies

In this section, we estimate the impact of the childcare policy separately for each of Quebec’s 98 census divisions. Having separate census division estimates serves two purposes. First, it allows us to validate that the positive labour supply effects are driven by the childcare policy and not any other possible confounds. Specifically, the places that experience the largest employment and earnings gains should be those that experience the largest increase in daycare take-up. Second, we can test whether the effect sizes vary with the complier, never-taker, and always-taker shares in the way our conceptual framework predicts.

We estimate the effect of the program on mothers’ outcomes separately by census division using the following parsimonious difference-in-differences model:

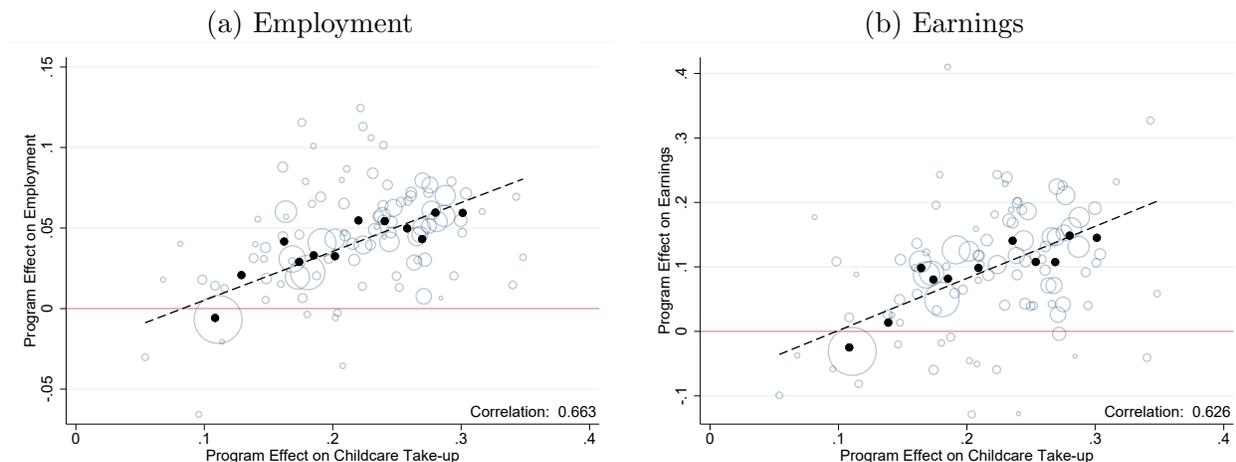
$$Y_{icst} = \alpha^{Qc} (\mathbf{I}_c^{CD} \times QC_i \times Post_s) + \gamma^s \mathbf{I}_s^{Year} + \gamma^t \mathbf{I}_t^{Event} + \gamma^c \mathbf{I}_c^{CD} + \beta \mathbf{I}_{is}^{Age} + v_{icst}. \quad (6)$$

where Y_{icst} is an outcome for mother i , residing in census division c in year s and event time t . We define $Post_s = 1\{Year_s \geq 2000\}$, and restrict the LAD sample to years 1990-1996 and 2000-2005. We also limit the sample to frequent tax filers and only include mothers at event times $t \in [0, 10]$. \mathbf{I}_c^{CD} is a vector of indicator variables for each census division in Canada. The coefficients of interest are α^{Qc} , which are CD-specific policy effects. The outcomes are employment, earnings, and childcare take-up. Standard errors are clustered at the census division level

Figure 10 plots CD-specific employment and earnings effects against the formal childcare take-up effects (the share of compliers). The correlation is always positive, confirming that places with more compliers (a stronger “first-stage”) experience greater employment and earnings gains (a larger “reduced-form”). The effects are positive in most census divisions, but the intercept is slightly below zero, suggesting that in the absence of childcare take-up, employment and earnings would likely have declined in Quebec relative to the rest of Canada.

Next, we extract data on time spent on unpaid childcare by age group from the Canadian Census to estimate census-division-specific shares of compliers, always-takers, and never-takers. Let π_g^{cd} denote the share of group g in census division cd , where groups are p -compliers (PC), g -compliers (GC), always takers (A), p -never takers (PN), and g -never takers (GN).

Figure 10: Heterogeneous Effects of Quebec’s Childcare Policy and Heterogeneous Take-up



Notes: The figure plots program effects on mothers’ employment (panel a) and earnings (panel b) against program effects on childcare take-up. Each dot represents a census division in Quebec. The size of the dots is proportional to the sample size. Black dots represent a binscatter plot. Estimates are based on equation (6).

The estimated program effect on childcare take-up is equal to $\pi_C^{cd} = \pi_{PC}^{cd} + \pi_{GC}^{cd}$. We use the measured post-policy childcare take-up rate to approximate $\pi_A^{cd} + \pi_C^{cd}$, which allows us to recover the fraction of always-takers and the fraction of never takers $\pi_N^{cd} = 1 - \pi_A^{cd} - \pi_C^{cd}$. Let $G_i = 1\{D_i = g\}$ be an indicator for grandparents being the main caretakers for family i . Then, the shares of g -compliers and g -never takers are

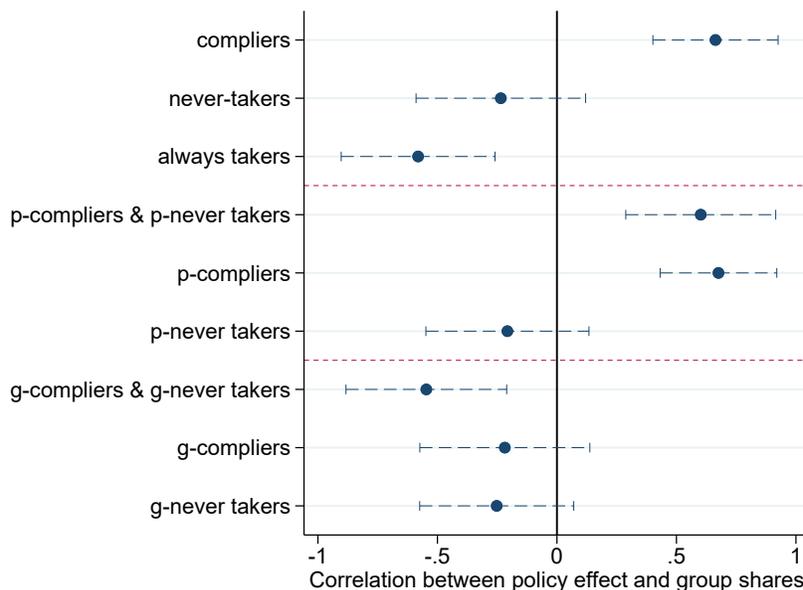
$$\begin{aligned}\pi_{GC}^{cd} &= P[D_i(1) = c, D_i(0) = g | cd] = E[G_i | S_i = 0, cd] - E[G_i | S_i = 1, cd] \\ \pi_{GN}^{cd} &= P[D_i(1) = g, D_i(0) = g | cd] = E[G_i | S_i = 1, cd],\end{aligned}$$

where $S_i \in \{0, 1\}$ denotes whether a family is eligible for childcare subsidies and $D_i(s) \in \{p, g, c\}$ denotes the potential care status as a function of childcare subsidies.

Based on the NLSCY, the share of g -compliers across the province of Quebec is roughly 3.5% (see Figure 7). Unfortunately, the NLSCY does not record the census division of residence and the sample size is relatively small, preventing us from estimating CD-specific shares of g -compliers and g -never takers. Instead, we use a proxy for frequent grandparent-provided care from the Census. The long-form Census asks respondents about the weekly hours of unpaid childcare they provide. Statistics Canada publishes the counts of individuals that supply childcare for different numbers of hours (0, 1-4, 5-14, 15-29, 30-59, 60+) by census division, gender, and age group. We extract these data for the years 1996 (pre-policy) and

2006 (post-policy). We create an index $\tilde{G}_{s,cd}$ for each census division cd and time period s , capturing the number of women aged 55 or older who provide at least 30 hours of unpaid childcare per week. We normalize the index by the number of children aged 0-9 so that it measures the intensity of grandparental care per child. We approximate the share of g -compliers by $\pi_{GC}^{cd} \approx \tilde{G}_{1996,cd} - \tilde{G}_{2006,cd}$ and the share of g -never takers by $\tilde{G}_{2006,cd}$. We can then recover the shares of p -compliers (i.e. $\pi_{PC}^{cd} = \pi_C^{cd} - \pi_{GC}^{cd}$) and p -never takers.²⁹

Figure 11: Heterogeneous Effects of Quebec’s Childcare Policy and Group Shares



Notes: This figure reports correlation coefficients between the effect of childcare subsidies on mother employment and group shares. Correlations are based on 98 observations, where an observation is a census division in Quebec. Dashed blue line show 95% confidence intervals, where standard errors are heteroskedasticity-robust.

In Figure 11, we correlate the effect of childcare subsidies on mothers’ employment with the group shares. Consistent with our framework, effects are larger (more positive) in places with more compliers and smaller in places with more never takers or always takers. The identity of the counterfactual main caretaker among compliers matters greatly: the effect size increases with the fraction of p -compliers, but decreases with the fraction of g -compliers. The combined share of the g -compliers and g -never takers—capturing the pre-policy importance of grandparents as caregivers—strongly predicts the effect size with a correlation coefficient of -0.55.

²⁹Appendix D provides further details on the calculation of group shares using this proxy.

Discussion. What do our results imply for other contexts? Overall, we find that subsidized childcare in Quebec boosted grandmothers’ employment and had a sizable positive effect on mothers’ earnings and employment. Andresen and Nix (2020) find similar results for mothers in Norway, Brewer et al. (2022) find smaller effects in the UK, and Kleven et al. (2020) find no effects for mothers in Austria. Both Brewer et al. (2022) and Kleven et al. (2020) show that formal childcare crowds out informal childcare in their setting.

To reconcile these findings, Appendix Figure A3 shows that the UK and Austria have some of the highest rates of informal childcare arrangements, while Quebec and Norway have some of the lowest rates. Conversely, Austrian women between the age of 55 and 64 are significantly less likely to be working than same-aged men compared to Quebec and Norway. Frimmel et al. (2020) notably show that the arrival of a grandchild significantly increases women’s probability of leaving the labour market in Austria. These patterns are consistent with grandparent-provided care being a more important substitute for formal childcare in countries where childcare has a limited impact on mothers. That is, the share of *g*-compliers is likely much higher in Austria than in Canada or Norway.

To be sure, these countries also differ in terms of gender norms. Appendix Figure A4 shows that Austrians are more likely to believe pre-school children and family life suffer when mothers work. Quebecois and Norwegians are less likely to think that women with pre-school children should stay home.³⁰ But in all six depicted settings—Quebec, rest of Canada, the US, the UK, Norway, and Austria—the fraction of people who agrees that working mothers can have a warm relationship with their child is roughly the same.

While norms can contribute to cross-country differences, the perceived costs of having children do vary too. For instance, Austrians are far more likely to believe children restrict employment and career chances. The fraction of respondents who agree with this statement is also substantially higher in the UK than it is in Norway and Quebec.

Overall, our heterogeneity analysis within Quebec and our international comparisons support the conclusion that the potential effects of childcare subsidies on mothers’ labour market outcomes depend on pre-existing care arrangements. In addition, we find positive effects of childcare subsidies on grandmothers’ employment in a context where informal care is relatively low by international standards, suggesting formal childcare has important implications for older adults’ retirement choices in countries where grandparental care is more important. The existence of such spillover effects implies that evaluating childcare policies requires one to consider a broader definition of the family unit, including relatives.

³⁰Individuals in the rest of Canada are more likely to think so. This was also true in 1994, prior to the childcare reform. Such differences in norms across provinces are unlikely to be the result of the policy itself.

7 Conclusion

In this paper, we examine how children affect earnings and employment trajectories of parents and grandparents and whether childcare subsidies can attenuate the impact of children and grandchildren. A growing literature studies the effect of children on parents while ignoring that a substantial share of childcare is provided informally by grandparents (15% in Canada and 20% in the US). Consequently, existing studies likely underestimate children’s life-cycle earnings and employment impacts.

The first innovation of our analysis is to estimate the multigenerational impact of children by using unique Canadian tax data that link 7 million parents with their grandparents over 40 years. In line with existing literature, we find substantial child penalties for Canadian mothers similar to those in Scandinavian countries, the US, the UK, and Austria. But we also document significant impacts of grandchildren: Grandparents’ earnings 10 years after grandparenthood drop by 16%.

The second innovation of our analysis is to estimate the impact of childcare subsidies on both mothers and grandmothers. Our paper demonstrates that universal childcare policies can help attenuate the impact of children on mothers, but also affects the impact of grandchildren on grandmothers in subtle ways. We find that Quebec’s childcare program increased the employment rate of grandmothers, but had a negative effect on their average earnings. This implies that a comprehensive assessment of family policies on women’s labour market outcomes should also include effects on grandmothers, particularly in settings where grandparents are important childcare providers. Depending on the distribution of counterfactual caretakers in the absence of childcare subsidies, such programs have the potential to create a double dividend over the life cycle in some settings, boosting earnings and employment of mothers and grandmothers. In contrast, policies targeting the grandparent generation could spill over to parents. For example, favoring late retirement to boost employment rates of older workers could indirectly lower mothers’ employment rates (e.g., Kaufmann et al. (2022)). Studying whether different policies to promote labour force participation in one generation have spillover effects onto other generations remains an important area for further research.

Our third innovation is the use of spatial variation in program effect sizes to examine how the importance of grandparent-provided care can modulate the impact of childcare subsidies on mothers. Existing evidence on the impact of childcare subsidies on mothers’ labor supply is mixed, ranging from no effect in Austria (Kleven et al., 2020) to a 23% reduction in earnings drop from motherhood in Norway (Andresen and Havnes, 2019). Our results emphasize the following potential explanation: that policy effects on mothers are

reduced when grandparents provide significantly more informal childcare. In other words, the availability of and substitutability between alternative modes of care is a key driver of effect sizes.

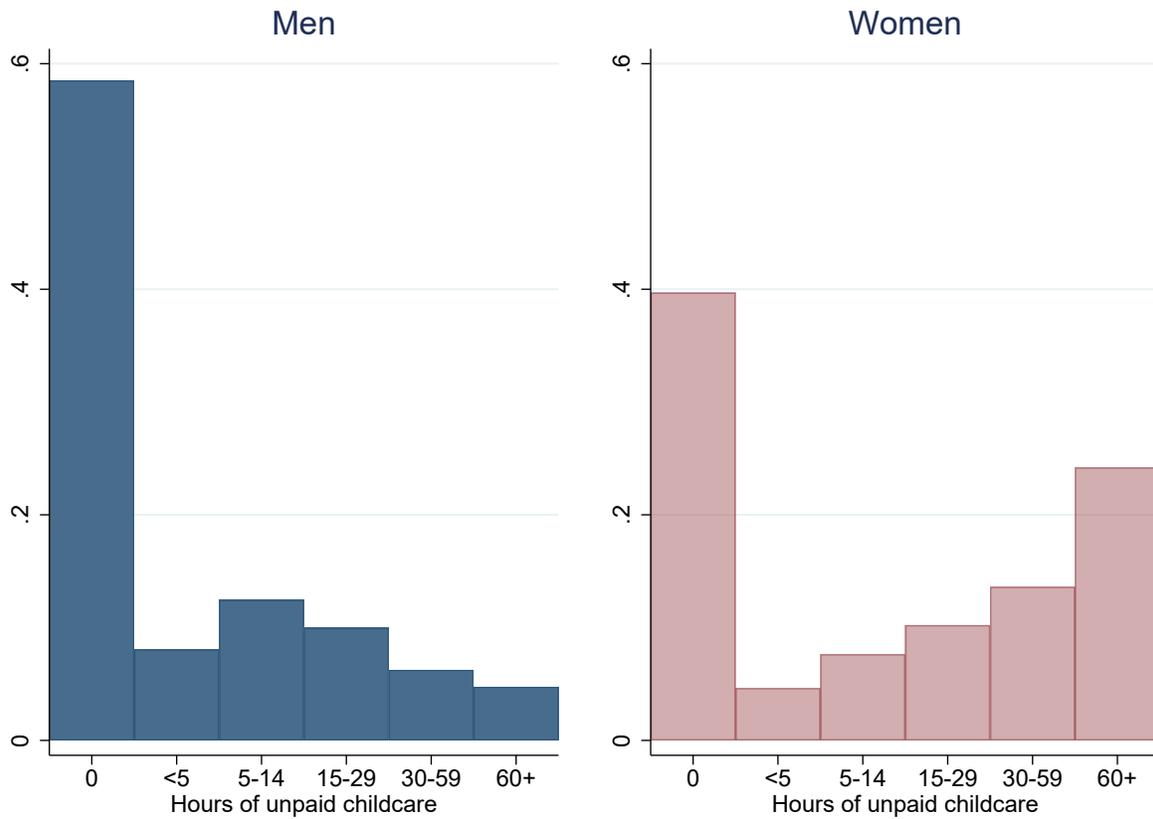
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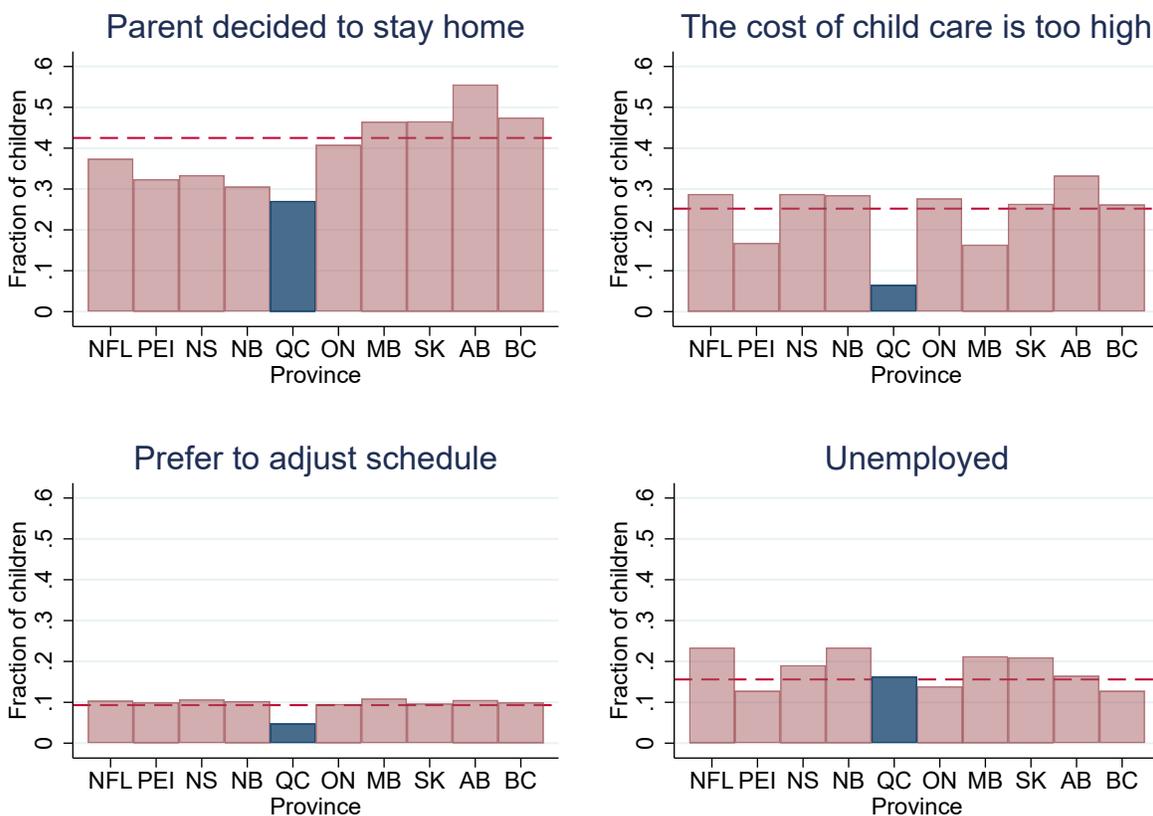
Appendix for Online Publication

Figure A1: Weekly number of unpaid childcare provided by 25-34 years-old, by gender



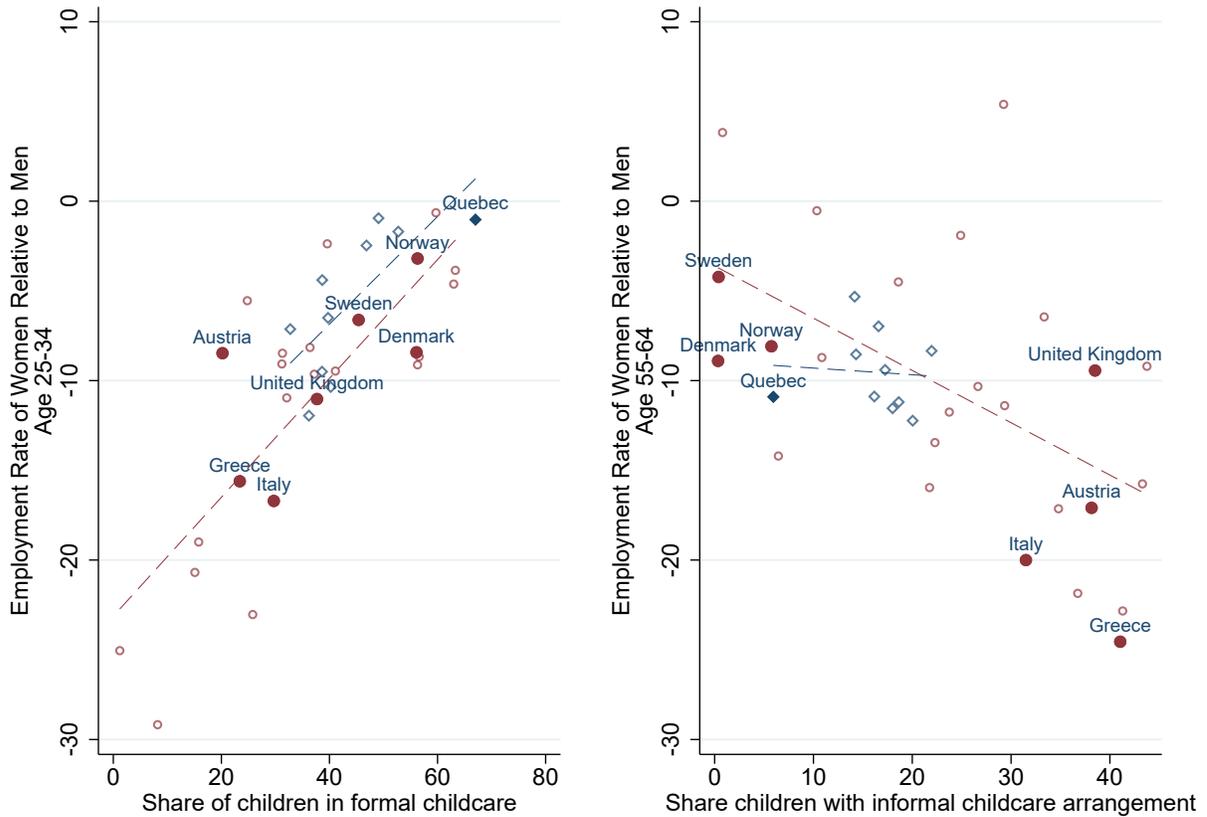
Notes: Data are for year 1996, and are drawn from Statistics Canada Table 95F0239XDB96001.

Figure A2: Reasons for not using any child care arrangement, by province



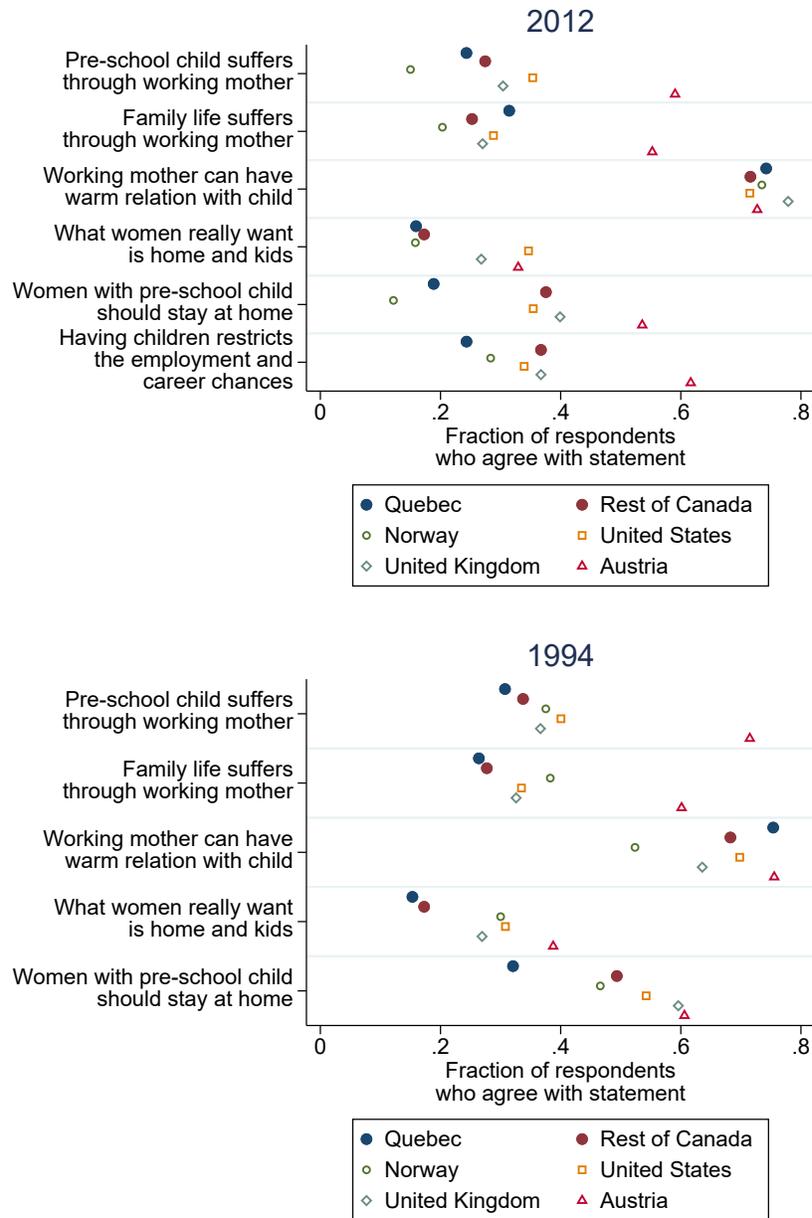
Notes: All statistics are based on data for year 2019 in Statistics Canada Table 42-10-0010-01, and cover the subpopulation of children aged 0-5 not in child care. These summary statistics are produced by Statistics Canada using the Survey on Early Learning and Child Care Arrangements. The horizontal dashed red lines indicate the corresponding fraction of children for all of Canada. Survey respondents could select multiple options. The complete survey entries are "One of the parents has decided to stay home with the child", "The cost of child care is too high", "Prefer to adjust work or study schedules to accommodate care needs", and "Unemployed". Other potential reasons included in the survey but omitted from this figure are "Child is in kindergarten", "Maternity, paternity or parental leave", "Shortage of places or waiting list", and "Other reasons".

Figure A3: Child care arrangements and women’s relative employment rate for different age groups, EU countries and Canadian provinces



Notes: In both panels, blue diamonds are Canadian provinces and red dots European countries. Dashed lines show regression slopes, separately for the sample of Canadian provinces and European countries. Relative employment rates on the vertical axis are women’s minus men’s employment rates. For EU countries, all statistics are for year 2019 and taken from the OECD Family Database (<https://www.oecd.org/els/family/database.htm>). Formal and informal care use is based on Tables PF3.2 and PF3.3, respectively. Employment rates are based on Table LMF1.4. The share of children in formal care is measured as enrollment rates of 0-to-2 years-olds in childhood education services. The share of children in informal care is the proportion of 0-to-5 years-olds using informal childcare arrangements during a typical week. These data are taken from Eurostat’s EU-SILC survey and informal care is defined as care provided by grandparents or other relatives, friends, or neighbours for which the provider did not receive payment. For Canadian provinces, statistics are for year 2019 and extracted from Statistics Canada Tables 42-10-0004-01, 42-10-0005-01, and 14-10-0327-01. Formal care combines daycare centres and family child care homes, whereas informal care is measured by the fraction receiving care by a relative. All Canadian childcare use statistics are for 0-to-5 years old.

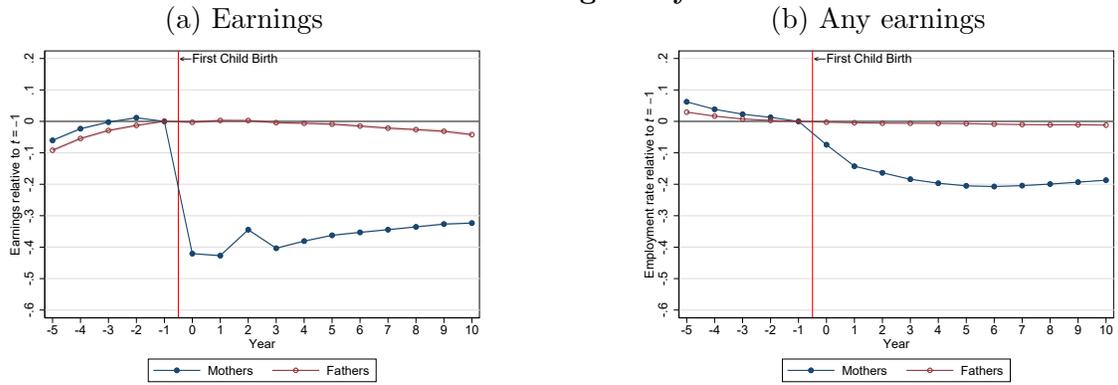
Figure A4: Gender Norms in Quebec, the Rest of Canada and Selected Countries



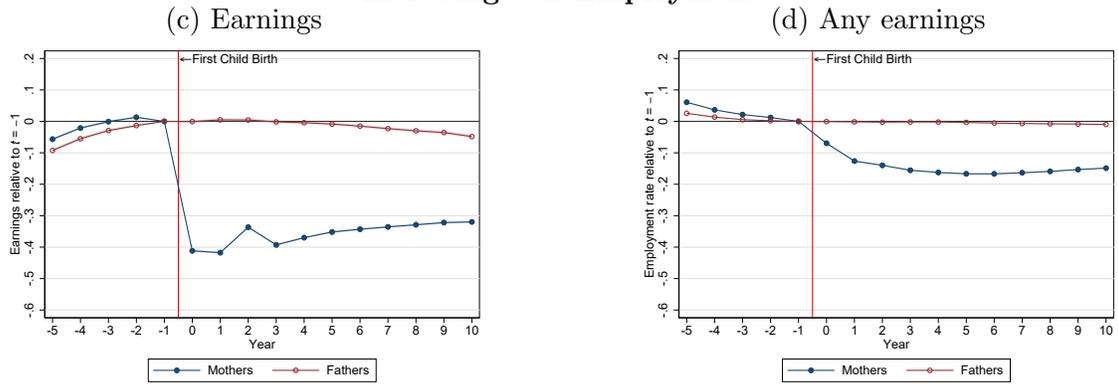
Notes: This figure the fraction of residents of selected countries who agree with specific statements. All statistics are based on the Family and Changing Gender Roles module of the International Social Survey Programme. The top panel shows statistics for the 2012 module, and the bottom panel is for the 1994 module. The question regarding whether children restrict employment and career chances was not asked in 1994. These data are available at <https://www.geis.org/en/issp/home>.

Figure A5: Estimates using LAD

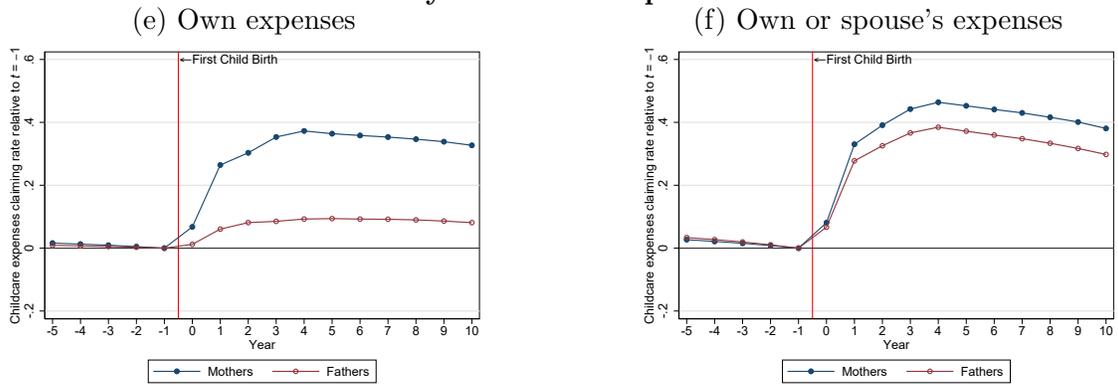
T4 Earnings Only



Including Self-Employment

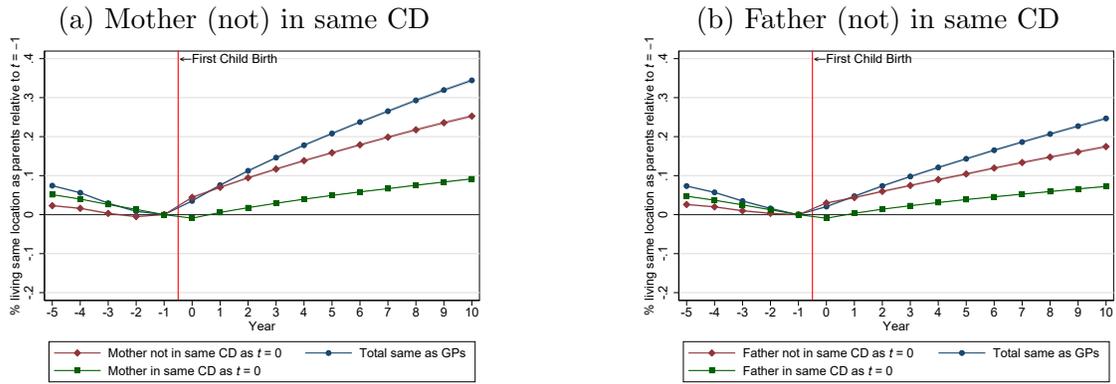


Any Childcare Expenses



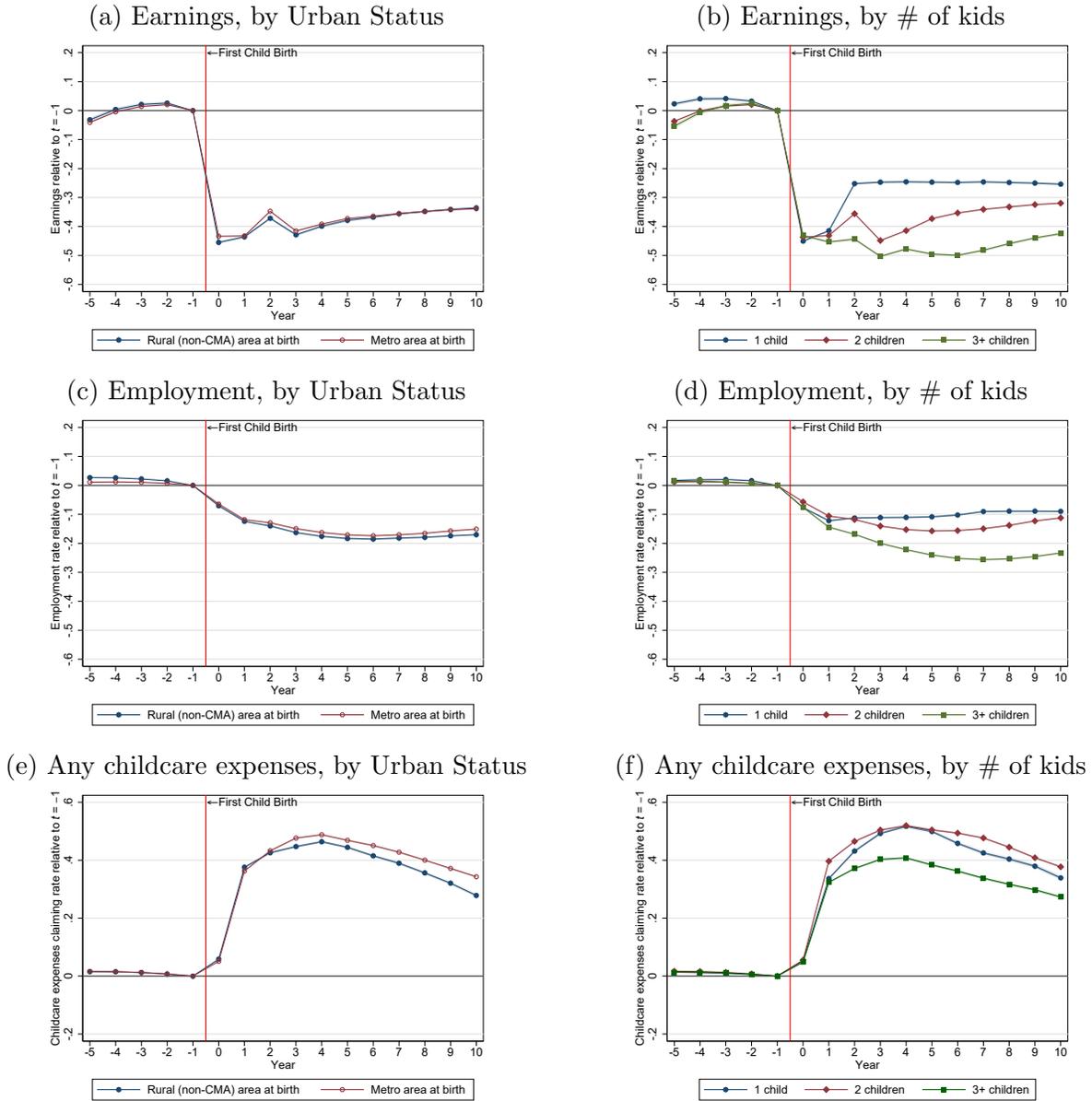
Notes: This figure reports estimates of child penalties based on equation (1), estimated on a sample of new parents in the Longitudinal Administrative Database (LAD). In panels (a) and (b), earnings and employment is based on T4 earnings alone. In panels (c) and (d), earnings also include self-employment income. In panel (e), the outcome is an indicator for having any childcare related expenses on one's own tax declaration. In panel (f), the outcome is an indicator for having any childcare-related expenses on either one's own tax declaration or one's spouse's. Shaded areas show 95% confidence intervals.

Figure A6: Who Is Moving?



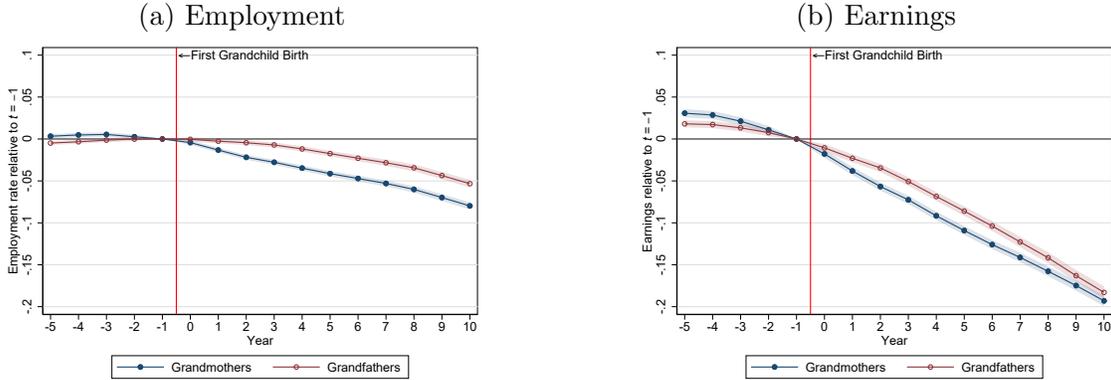
Notes: This figure reports estimates of whether an individual resides in (i) the same Census Division (CD) as one's own parent (blue circles), (ii) the same CD as one's own parent and the same CD as at $t = 0$ (maroon diamonds), and (iii) the same CD as one's own parent and not the same CD as at $t = 0$ (green squares). Shaded areas show 95% confidence intervals.

Figure A7: Heterogeneity in Child Penalties for Mothers



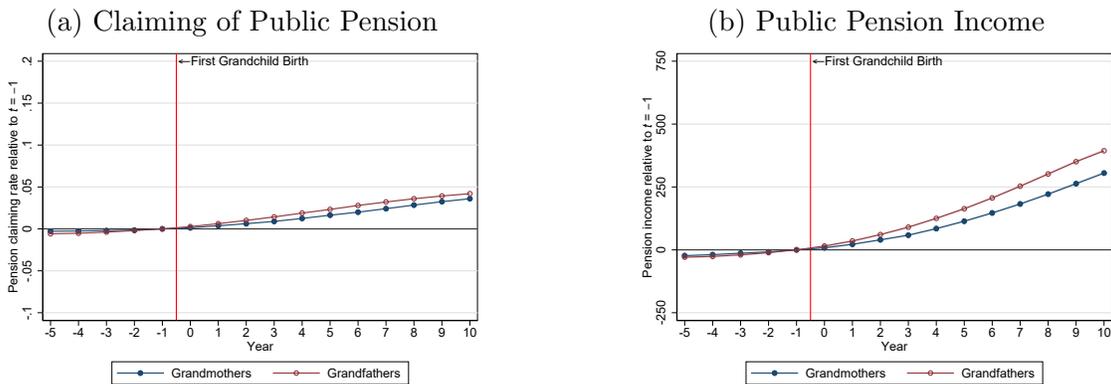
Notes: This figure reports estimates of the child penalty for mothers, separately for those who live in an urban (CMA) or a rural area (panels (a), (c) and (e)), and for those who have either 1, 2 or 3+ children (panels (b), (d) and (f)). The sample consists of new mothers in the Intergenerational Income Database (IID). Inverse probability weights are used to account for observable differences in pre-birth characteristics between the different subsamples. Shaded areas show 95% confidence intervals.

Figure A8: Grandchild Penalties, Difference-in-Differences using Placebo Grandchildren



Notes: This figure reports estimates from a difference-in-differences event study design that compares individuals who have grandchildren to those who never have grandchildren. We follow Kleven et al. (2019a) and assign placebo grandchildren to individuals who never have grandchildren, drawing from the observed distribution of age at first grandchild among those who do have grandchildren. In panel (a), the outcome is a dichotomous variables for any T4 earnings, and in panel (b) it is T4 earnings. Models are estimated separately by gender. Shaded areas show 95% confidence intervals.

Figure A9: Grandparents' Public Pension Take-up

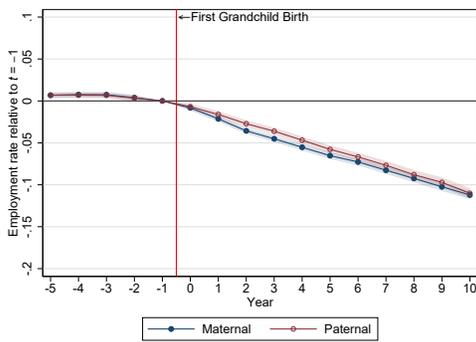


Notes: The figure reports estimates from equation (1). In panel (a), the outcome is a dichotomous variable for any public pension income, and in panel (b) it is public pension income. Models are estimated separately by gender. Shaded areas show 95% confidence intervals.

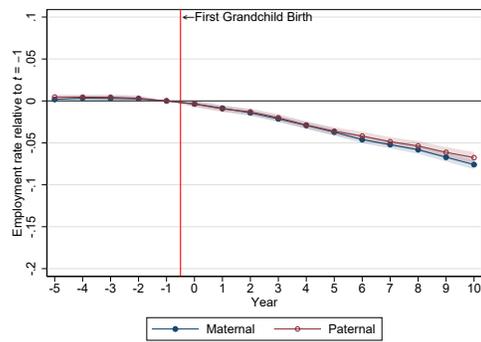
Figure A10: Comparing Maternal and Paternal Grandchild Penalties

Employment

(a) Grandmothers

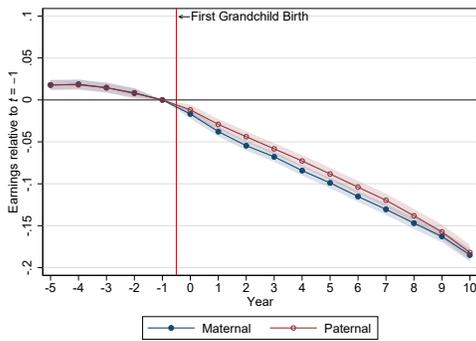


(b) Grandfathers

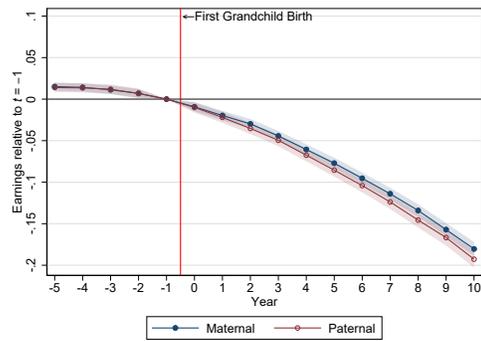


Earnings

(c) Grandmothers



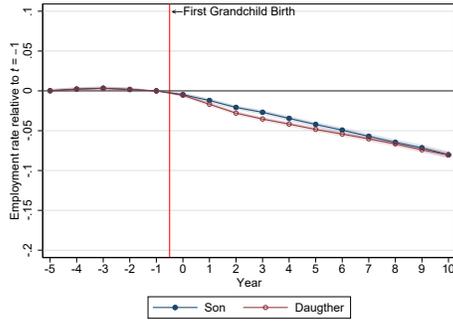
(d) Grandfathers



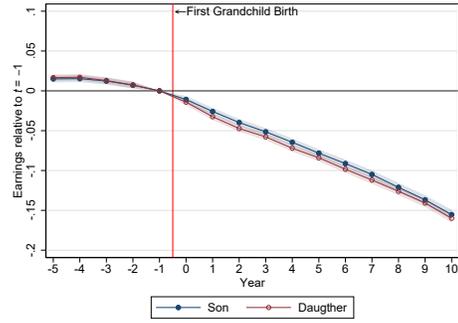
Notes: This figure reports estimates of the grandchild employment and earnings penalty separately for maternal and paternal grandmothers (panels (a) and (c)) and maternal and paternal grandfathers (panels (b) and (d)). Shaded areas show 95% confidence intervals.

Figure A11: Heterogeneity in Grandchild Penalties for Grandmothers

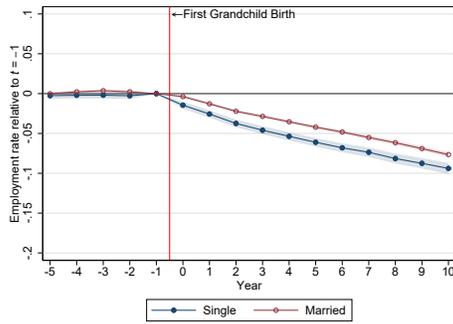
(a) Employment, by gender of child



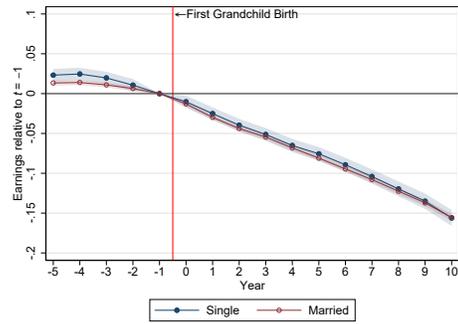
(b) Earnings, by gender of child



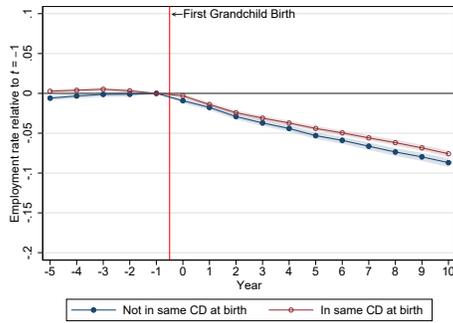
(c) Employment, by marital status



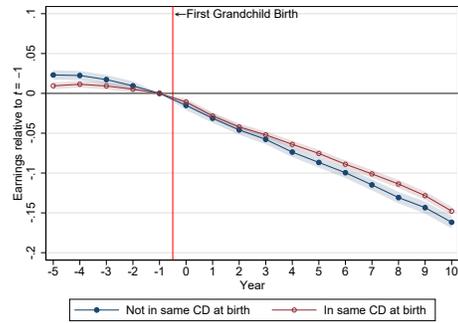
(d) Earnings, by marital status



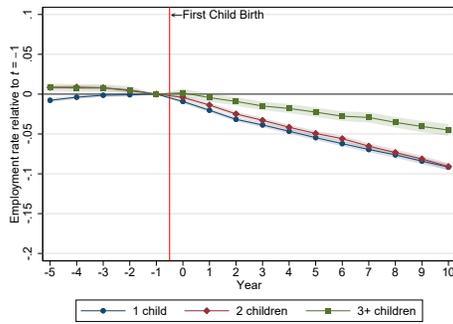
(e) Employment, by proximity to parents



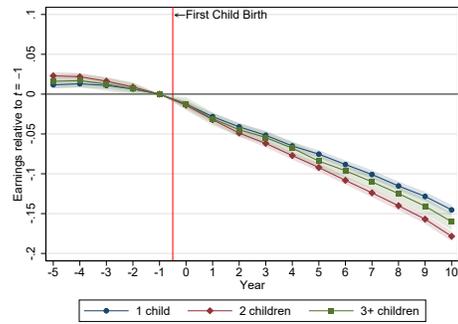
(f) Earnings, by proximity to parents



(g) Employment, by # of kids

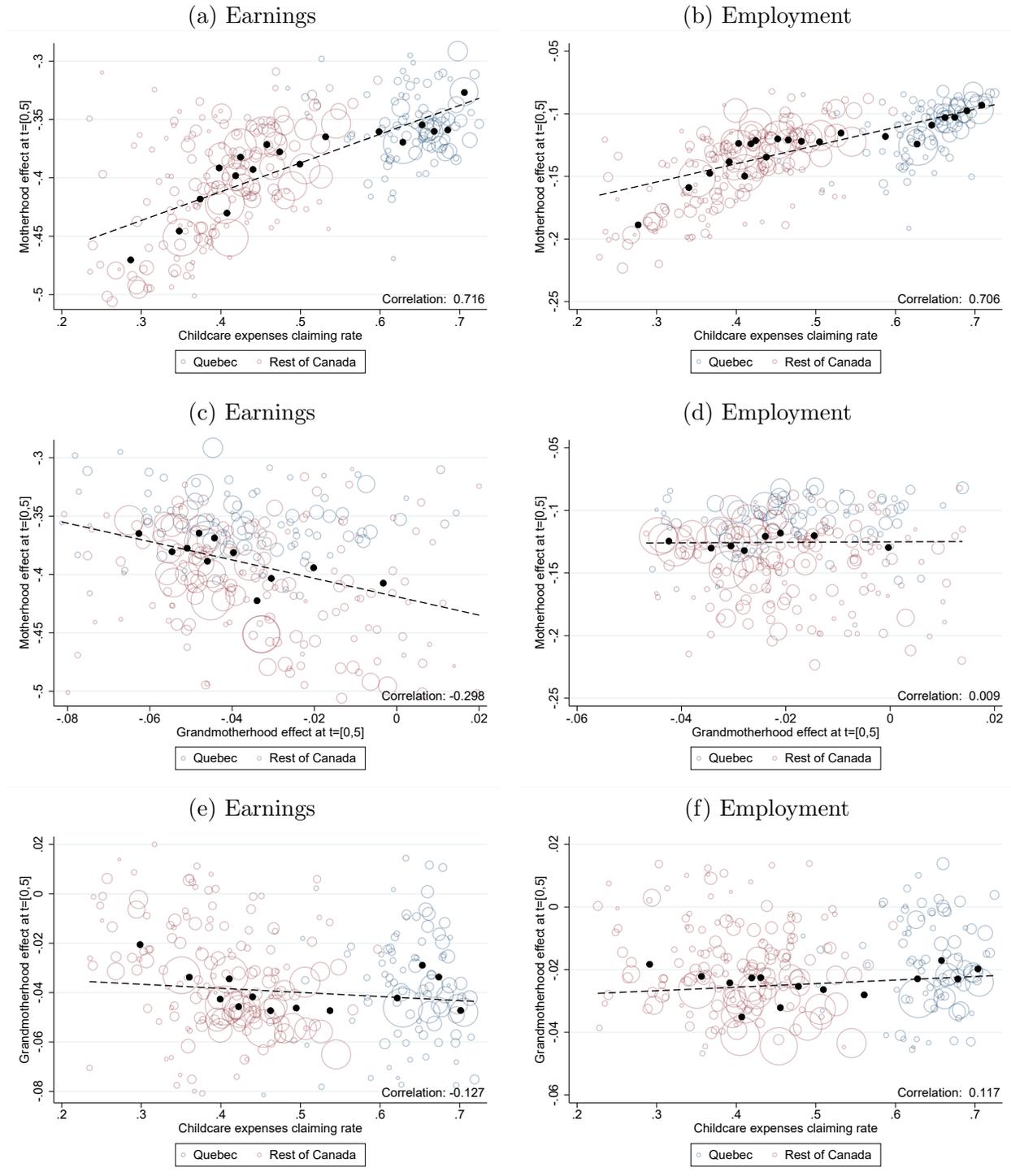


(h) Earnings, by # of kids



Notes: This figure reports estimates of the grandchild penalty for grandmothers, separately for those who have a daughter or a son (panels (a) and (b)), for singles and married (panels (c) and (d)), for those who do and do not live in the same CD as their child (panels (e) and (f)), and for those who have either 1, 2, or 3+ children (panels (g) and (h)). Inverse probability weights are used to account for observable differences in characteristics pre-birth of the first grandchild between the different subsamples. Shaded areas show 95% confidence intervals.

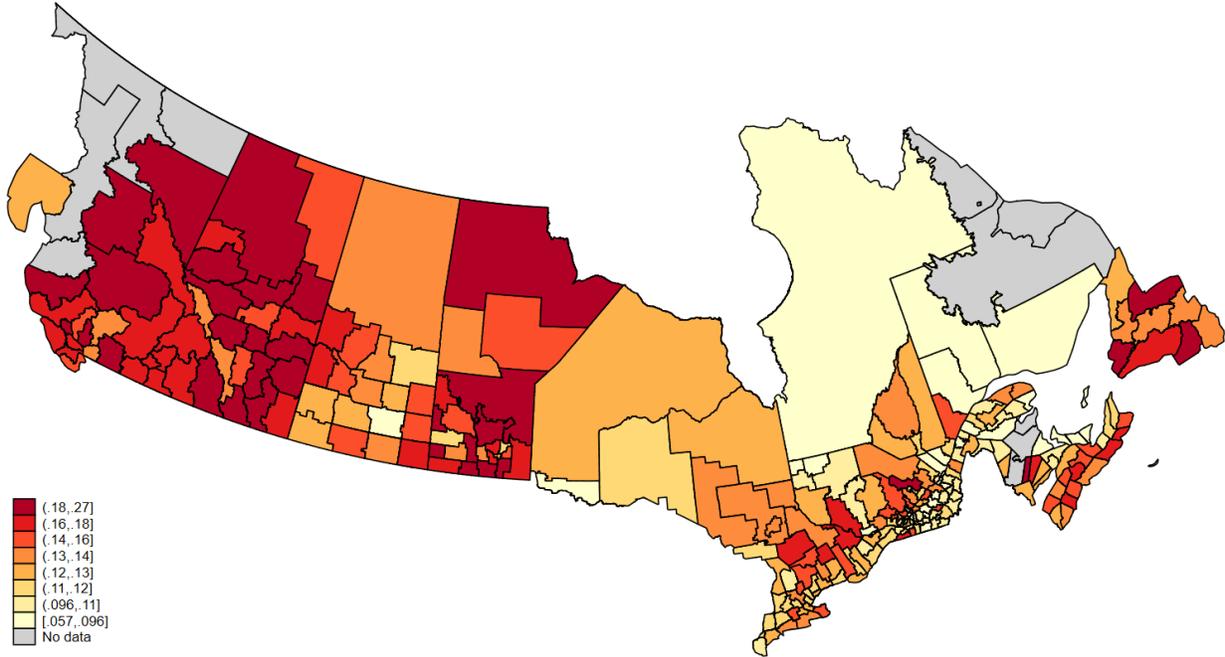
Figure A12: Child Penalties Across Census Divisions, including Quebec



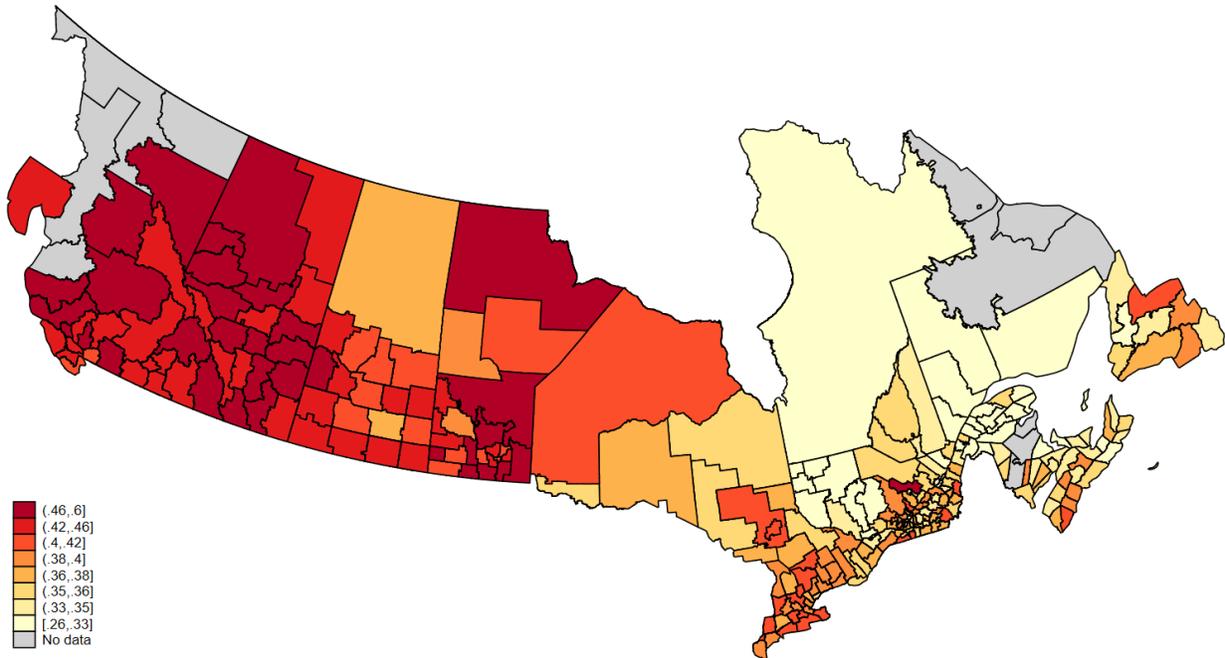
Notes: This figure presents estimates of motherhood and grandmotherhood effects on employment and earnings for all Canadian Census Divisions, including those in Quebec. Each red or blue circle represent one Census Division, and the size of the circle is proportional to the number of observations. Black dots represent a binscatter plot.

Figure A13: The Geography of Motherhood effects

(a) Employment



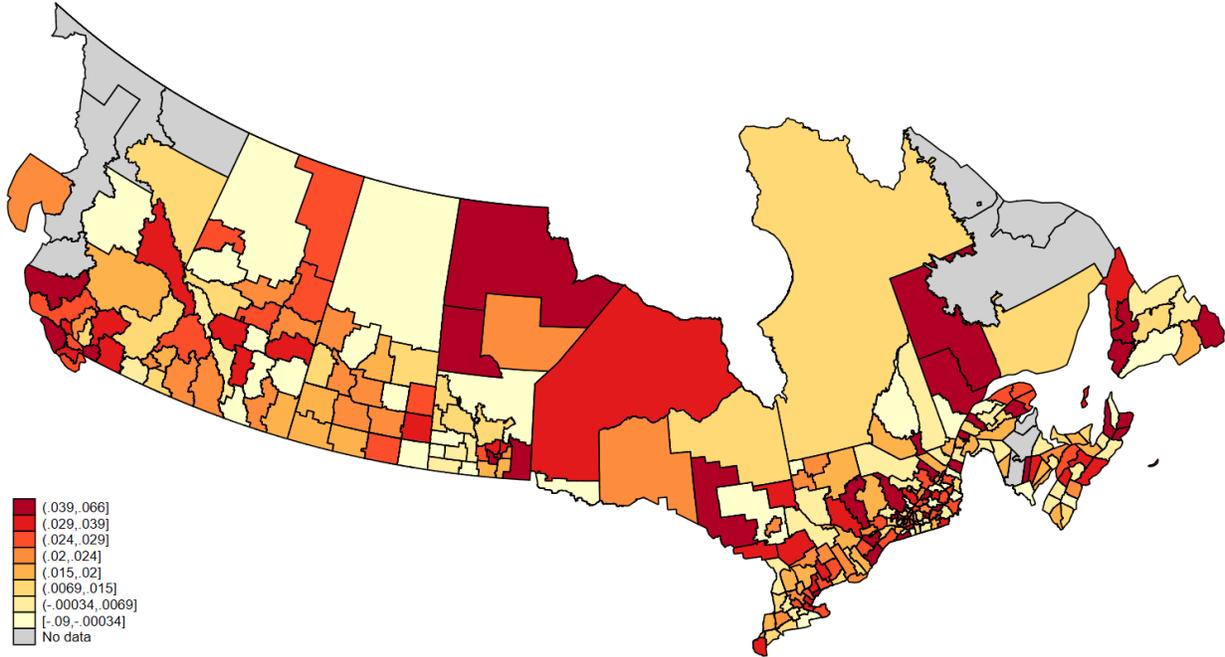
(b) Earnings



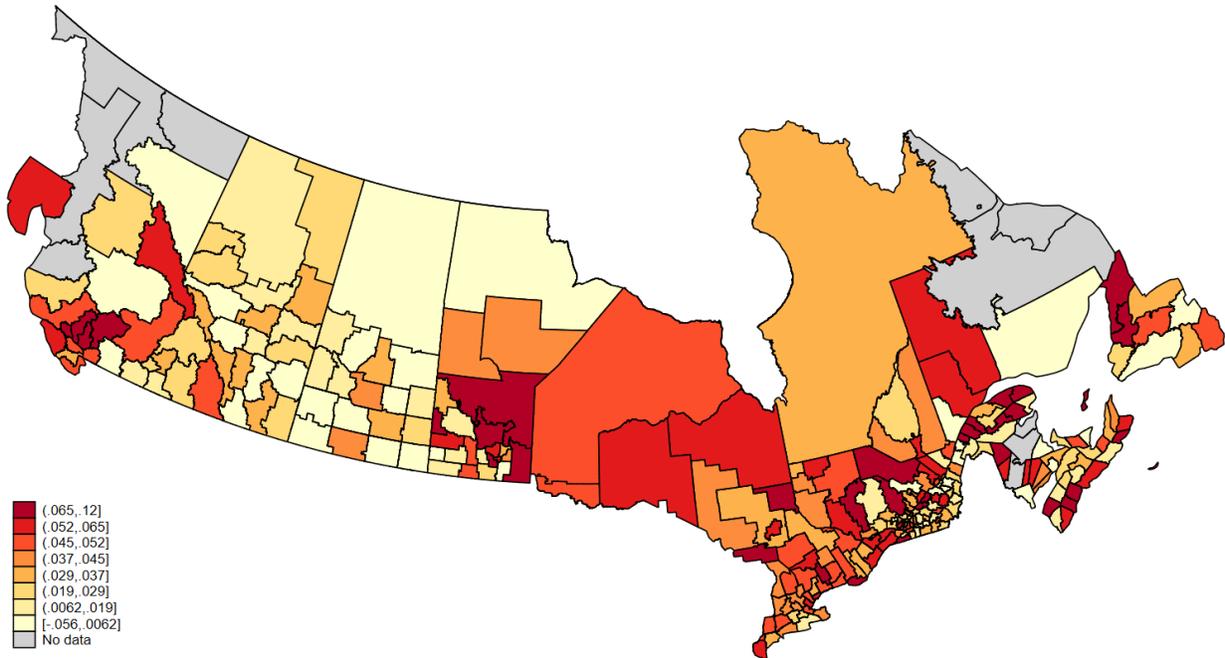
Notes: Census divisions with too few observations are labelled as "No data".

Figure A14: The Geography of Grandmotherhood effects

(a) Employment

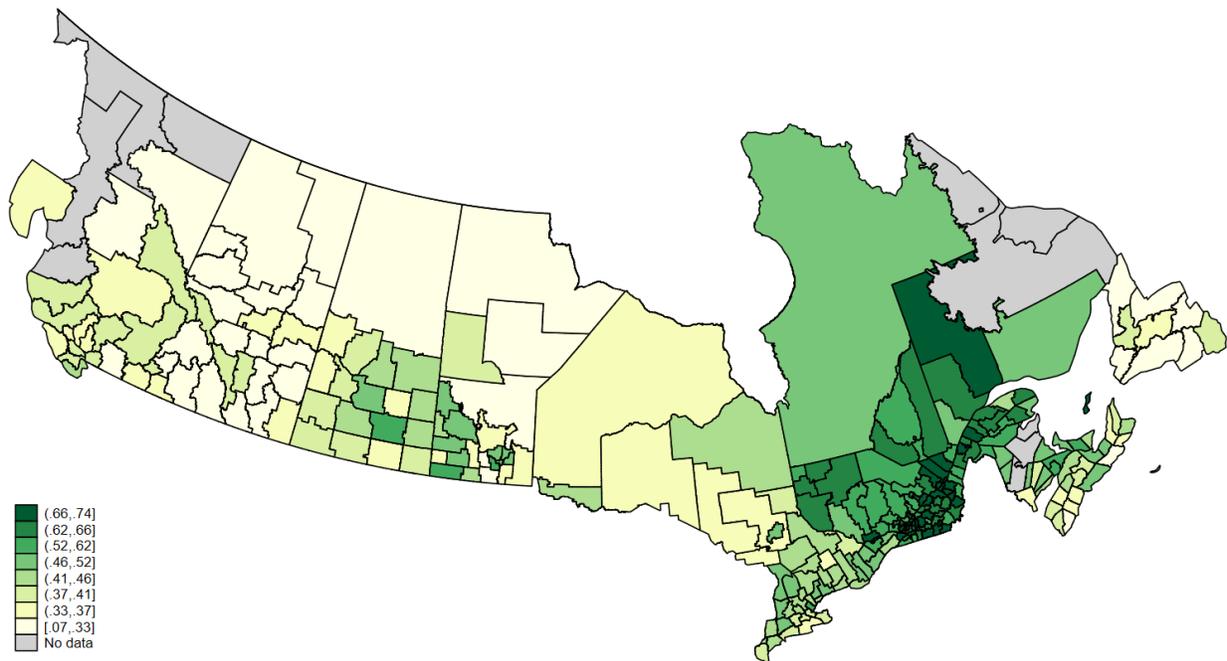


(b) Earnings



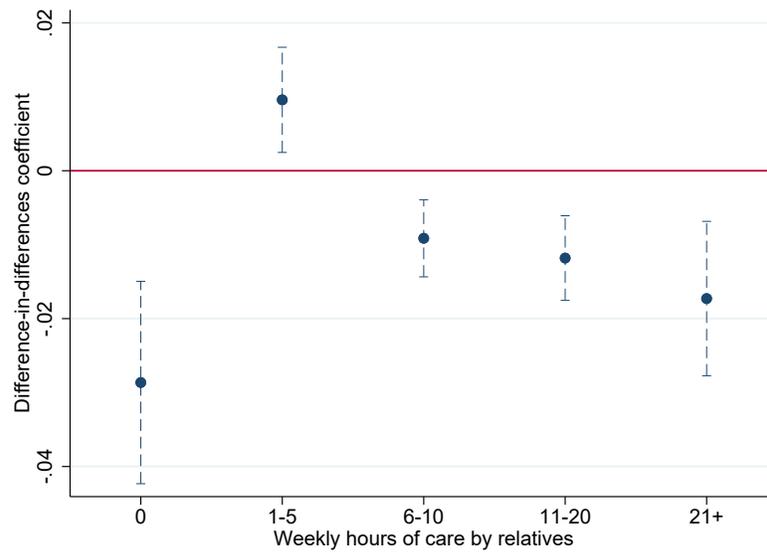
Notes: Census divisions with too few observations are labelled as "No data".

Figure A15: The Geography of Childcare Take-up



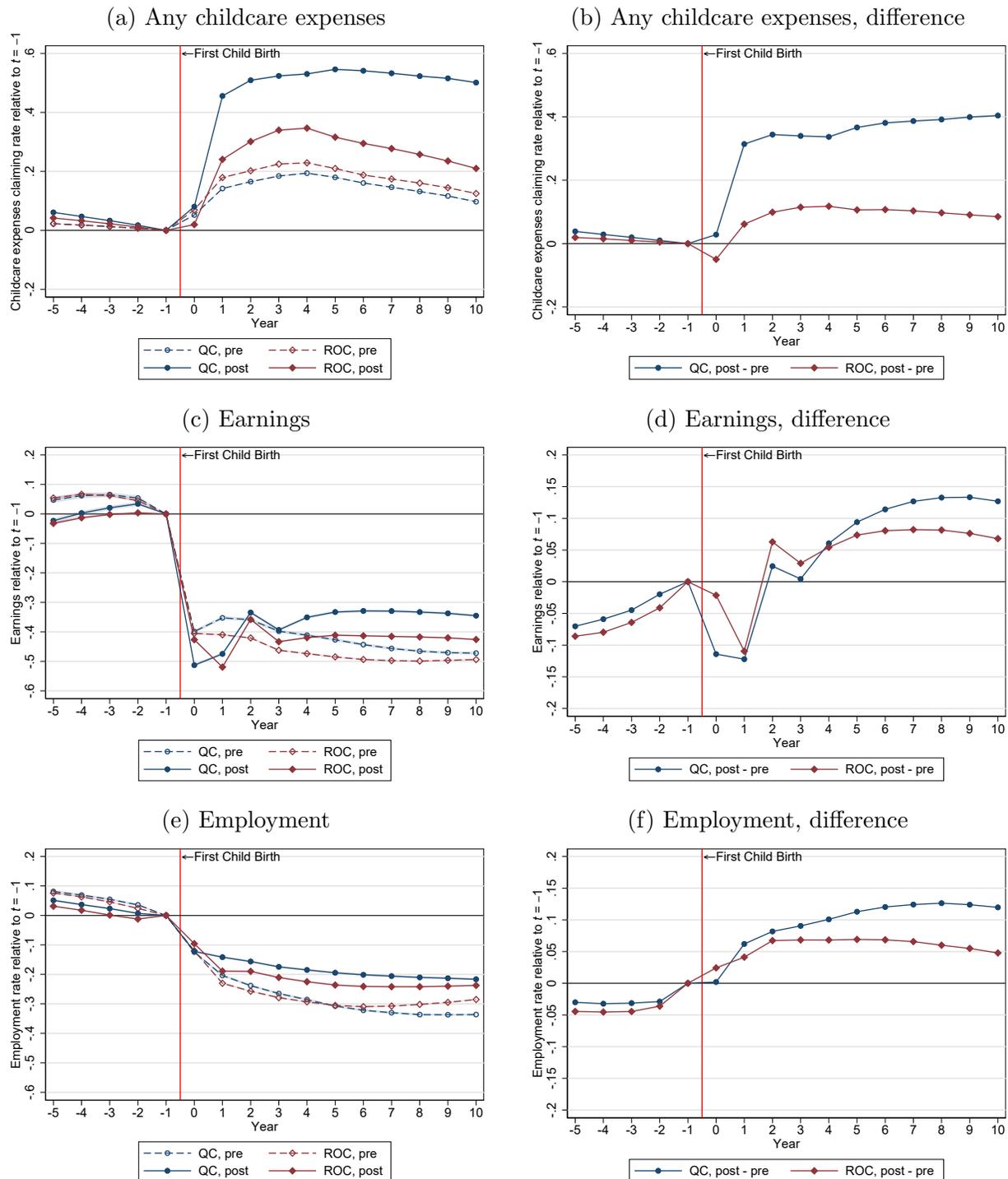
Notes: Census divisions with too few observations are labelled as "No data".

Figure A16: Program Effect on Hours of Care by Relatives



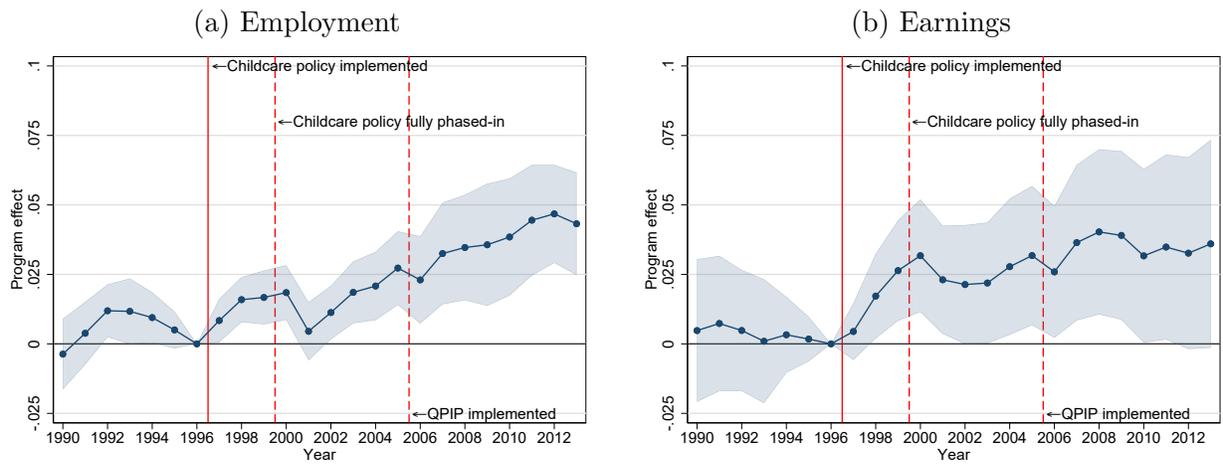
Notes: This figure reports difference-in-differences coefficients of childcare subsidies effects on care provided by relatives. There are 5 separate regressions, for 5 different outcomes. The outcomes respectively are dummies for receiving either 0, 1-5, 6-10, 11-20 or 21+ weekly hours of care by relatives. Dashed lines show 95% confidence intervals.

Figure A17: Impact of Children on Mothers, Quebec vs Rest of Canada, Pre- vs Post-1997



Notes: This figure compares formal childcare and motherhood impacts for mothers across Quebec and the rest of Canada, and across periods before (1982-1996) and after (1997-2018) the implementation of the childcare policy. All estimates are based on samples of new mothers in the Longitudinal Administrative Database (LAD). Shaded areas show 95% confidence intervals.

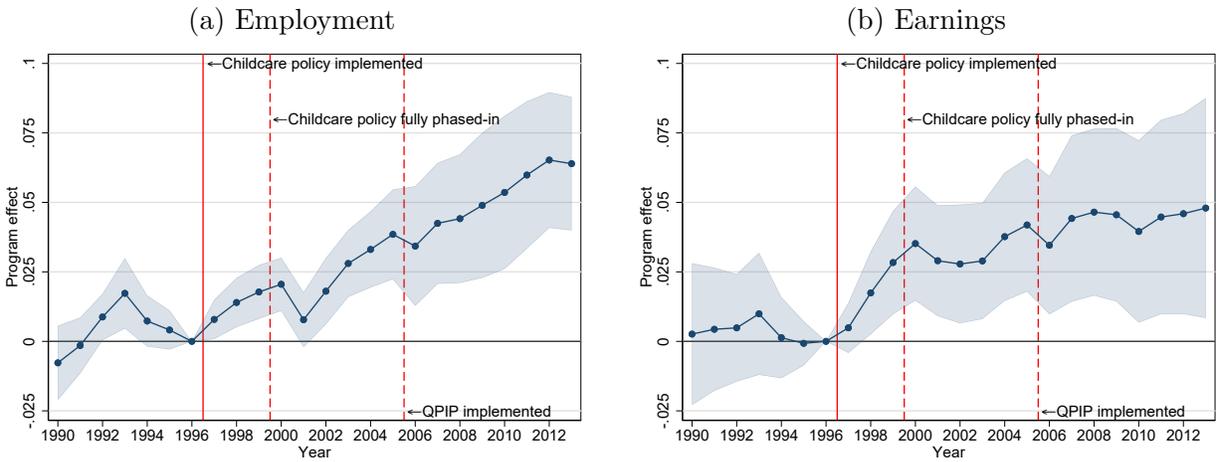
Figure A18: Effect of Quebec’s Childcare Policy on Mothers’ Employment and Earnings, Time-Invariant Place of Residence



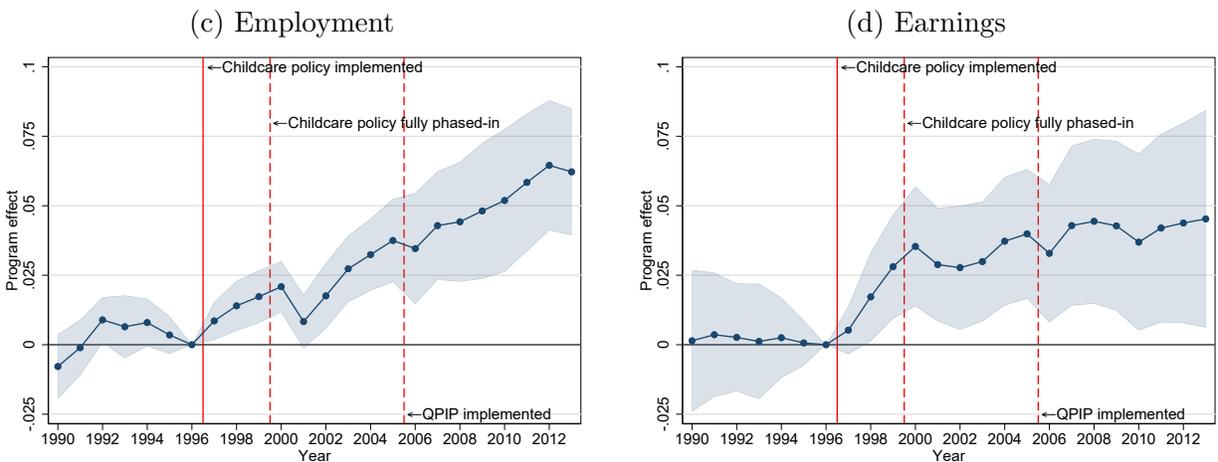
Notes: This figure reports the estimated policy effect of Quebec’s childcare program on mothers’ earnings and employment. All estimates are based on samples of mothers (including yet-to-be mothers) in the Longitudinal Administrative Database (LAD). All regression coefficients are scaled by predicted values and therefore represent percent changes relative to year 1996. Shaded areas show 95% confidence intervals.

Figure A19: Effect of Quebec’s Childcare Policy on Mothers’ Employment and Earnings, Including Self-Employment

Time-Varying Place of Residence

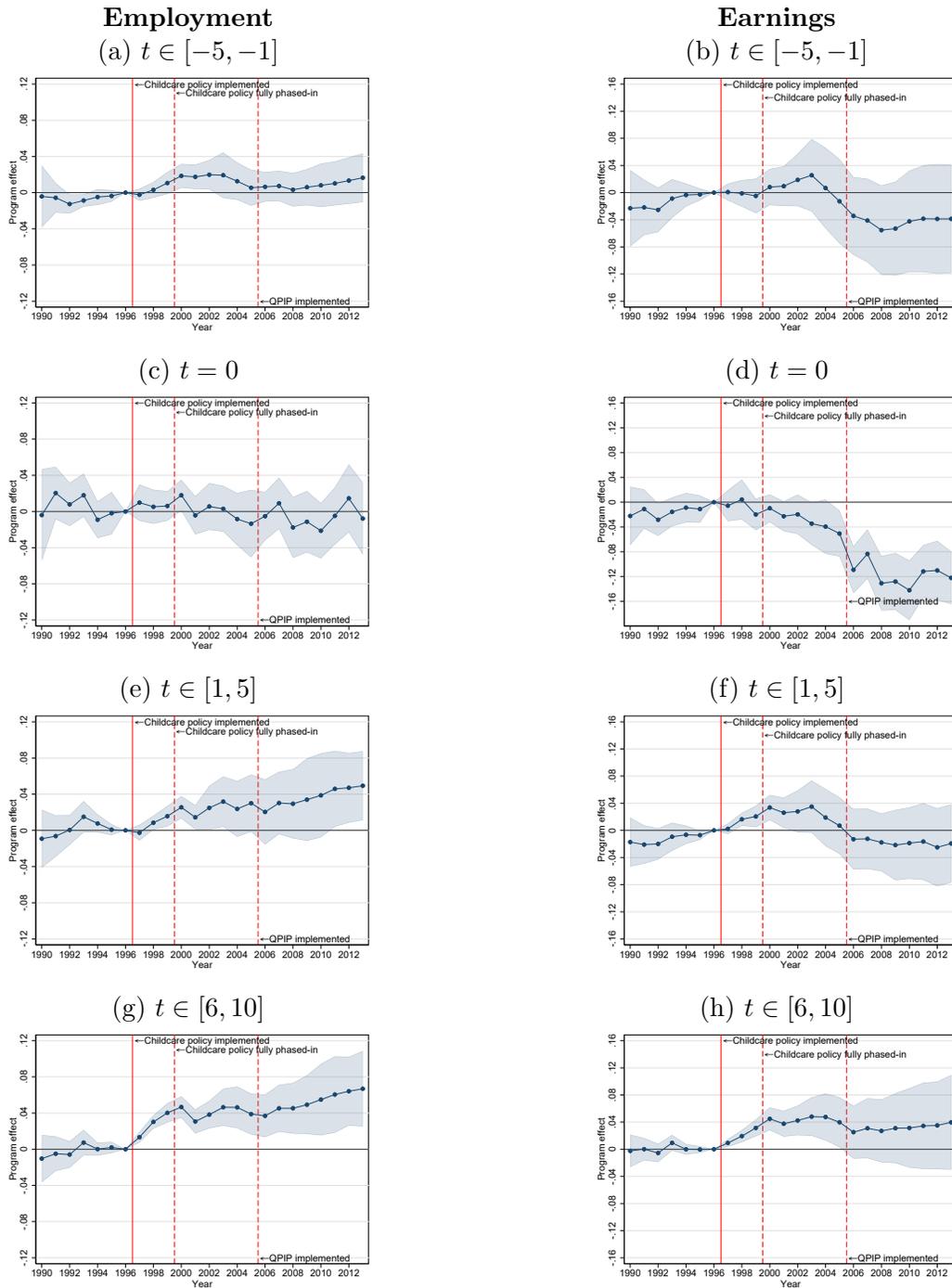


Time-Invariant Place of Residence



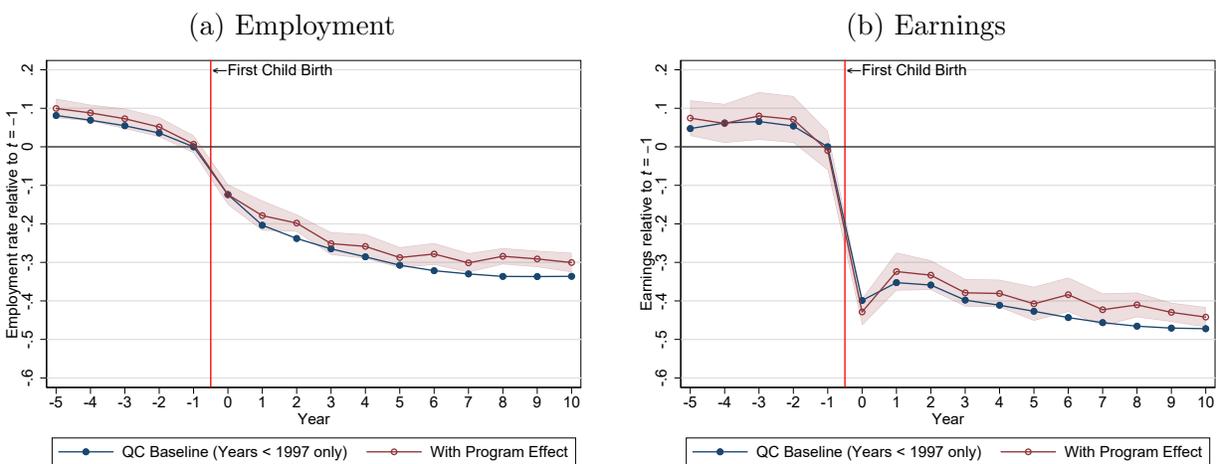
Notes: This figure reports the estimated policy effect of Quebec’s childcare program on mothers’ earnings and employment. All estimates are based on samples of mothers (including yet-to-be mothers) in the Longitudinal Administrative Database (LAD). All regression coefficients are scaled by predicted values and therefore represent percent changes relative to year 1996. Shaded areas show 95% confidence intervals.

Figure A20: Anticipatory and Dynamic Effects



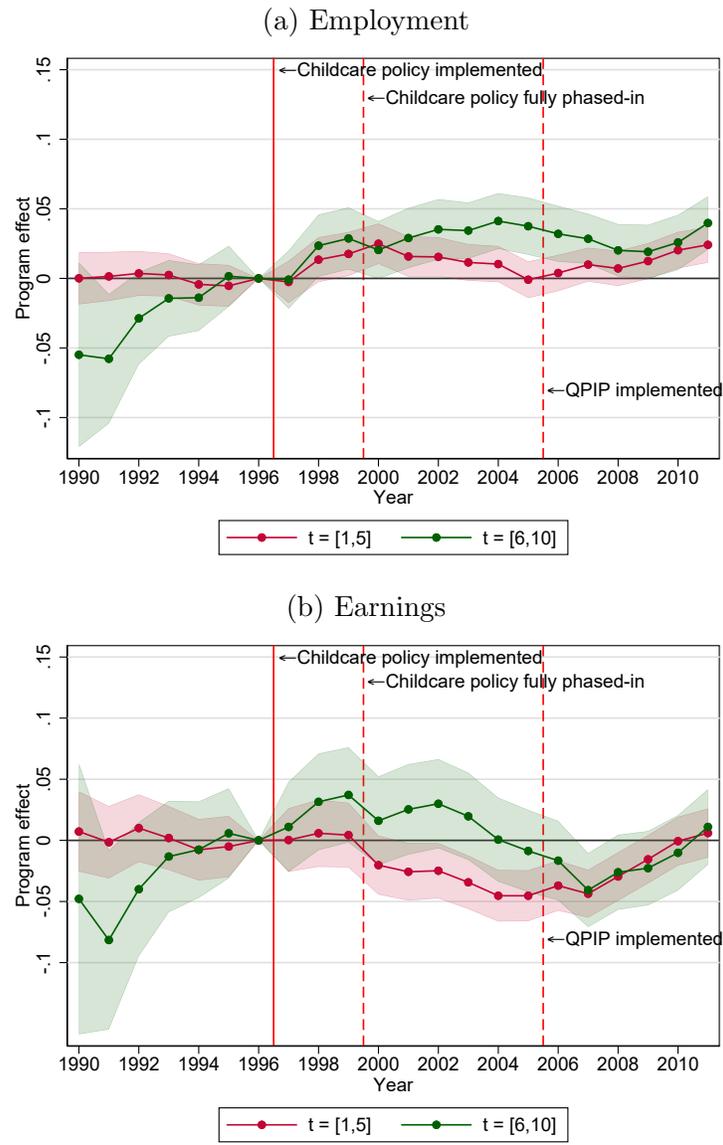
Notes: This figure reports difference-in-differences estimates of the impact of childcare subsidies, separately for different event times. In practice, we estimate the effect separately for each event time $t \in [-5, 10]$ using equation (4), and then take linear combination of coefficients for subgroups of event times (-5 to -1, 0, 1 to 5, and 6 to 10). Shaded areas show 95% confidence intervals.

Figure A21: Effect of Quebec's Childcare Policy on Motherhood Impact



Notes: This figure reports the estimated policy effect of Quebec's childcare program on motherhood impacts. All estimates are based on samples of mothers (including yet-to-be mothers) in the Longitudinal Administrative Database (LAD). The blue dots show event-study estimates for women residing in Quebec between 1982 and 1996. The red circles show program effects, where the treatment period is 2000-2005. Shaded areas show 95% confidence intervals.

Figure A22: Effect of Quebec’s Childcare Policy on Grandmothers’ Employment and Earnings



Notes: This figure reports the estimated policy effect of Quebec’s childcare program on grandmothers’ earnings and employment, based on equation (5). All regression coefficients are scaled by predicted values and therefore represent percent changes relative to year 1996. Coefficients are allowed to vary by event time periods $t \in [1, 5]$ and $t \in [6, 10]$. Shaded areas show 95% confidence intervals.

Appendix A: Data Appendix

The first two parts of this appendix section provides a detailed explanation of how we select our samples and define the individual- and household-level variables we use and construct from our two main datasets. The third part provides information on tax filing in Canada.

A.1 Intergenerational Income Database (IID)

A.1.1 Sample selection

The IID contains two groups of birth cohorts, which Statistics Canada refers to as panel A and panel B. Panel A contains child-parent linkages for any child aged 16-19 and living with a parent in the years 1982, 1984, and 1986, which covers birth cohorts 1963 to 1970. Panel B includes similar linkages for any child aged 16-19 and living with a parent in the years 1991, 1996, and 2001, covering birth cohorts 1972 to 1985 (excluding birth cohorts 1976 and 1981). Tax files (T1FFs) cover years 1978 to 2016 for panel A, and years 1981 to 2016 for panel B.³¹ For each birth cohort, the IID includes a family file linking children to their parents with unique IDs for both the child and the parents. The family files are unique in children.

Our sample of new parents is based on individuals who appear in the child generation of the IID. We use the dependent birthdate information from T1FFs to infer parenthood status, and include anyone who became a parent before age 40 in our parent sample. We discuss the way we infer parenthood through dependent birthdates in the next part of this section where we describe the definition of individual-level variables.

Our grandparent sample is based on individuals who appear in the parent generation of the IID. Note that due to the structure of panel A, the same child can appear twice in both panels (e.g., a child can appear as a 16-year-old in the 1982 cohort and as an 18-year-old in the 1984 cohort). In these cases, we use the earliest linkage available. Any individual who appears in IID's parent generation is a potential grandparent. We then use a potential grandparent's linked children's parental status to infer their grandparenthood status.

A.1.2 Individual-level variables

Parenthood and grandparenthood: The tax data contains the exact birthdates of dependents registered by individual tax filers. One immediate issue is that only one parent registers a dependent while the other does not. We utilize the spouse files IID provides, linking each

³¹Statistics Canada originally generated a first vintage of the IID that only included panel A. Panel B was produced many years later to meet different requirements, which is why the coverage of tax years differ across panels.

individual from the IID’s child generation to their spouse, and combine the dependent data of both spouses. In addition, we assign dependents registered by a spouse up to 2 years prior to the spousal linkage being observed, implicitly coding the dependent as born to the couple.

A second issue with dependent birthdates is that older siblings can register a younger sibling as a dependent for tax purposes. We introduce an age limit of 18 years for parenthood to avoid mis-assignment of siblings as parents. That is, we drop any dependent with an age difference of less than 18 years between them and their claimant.

Each person from the IID’s child generation who has at least one dependent is considered a parent, where the first year of their parenthood is the earliest dependent birthdate.

For grandparenthood, we utilize the IID’s parent-kid linkages along with the parenthood status of those from the child generation. Individuals from the parent generation (potential grandparents) often have multiple children from the IID birth cohort. For each potential grandparent, we gather the dependent birthdates of each of their own children. We sort these grandchild birthdates and code the date of grandparenthood as the birthdate of the first grandchild. Potential grandparents for whom no grandchild is observed are kept in the analytical datasets, and flagged as non-grandparents, although they may actually be grandparents via some unobserved children (i.e. children belonging to birth cohorts not included in the IID). . We impose a minimum age restriction of 36 years for grandparenthood assuming that the minimum age of parenthood is 18.

Marital status: We identify the marital status of parents and grandparents based on their self-reported marital status on the tax returns. If an individual did not file taxes in a given year, we use the latest information prior to the year in question to fill out the marital status.

Location: The IID contains longitudinal geography data down to the census subdivision they live in (roughly equivalent to a municipality). We use the unique province, census division (CD) and census subdivision (CSD) IDs to infer the location of each tax filer in a given year. If an individual did not file taxes in a given year, we use the latest available information prior to the year in question to fill in the location data.

We insure that geographic units are consistent over time. That is, we construct a consistent geography file that maps each year-specific CSD to its corresponding CD in the 2016 census atlas.

Income: We use (pre-tax) earnings reported in T4 slips as our primary income measure. We winsorize earnings by the 99th percentile in each year and for each gender to minimize the role of outliers. If an individual did not file taxes in a given year, we impute 0 as their annual earnings for the year in question. We define employment in a given year as having non-zero T4 earnings.

We prefer to use T4 earnings as opposed to total taxable income because any income

measure containing self-employment income comes with an additional endogeneity problem related to the adjustment of income for tax purposes. For instance, the owners of a family-owned business can split the business income across family members entirely for tax purposes even if the family members commit different hours or resources for the business. Our results are largely insensitive to the inclusion of self-employment income.

Pension benefits: We use claimed CPP/QPP benefits reported as our primary pension benefit measure. If an individual did not file taxes in a given year, we impute 0 as their annual pension benefits for the year in question.

Although the data contains information on other pension benefits, such as Registered Retirement Savings Plan (RRSP), Old Age Security (OAS), and several other pension benefits, we prefer to use CPP/QPP benefits. This is because CPP/QPP covers the entire working population whereas the alternatives are either self-selected by the individuals or are mean-tested programs. We define receiving any pension benefits as claiming non-zero CPP/QPP benefits in a given year.

A.1.3 Household-level variables

Household income: Household income contains four components: incomes of the parent, the parent’s spouse, and the two grandparents. The IID contains data on select variables for the spouses of individuals from the child generation (e.g., the “parents”), from which we recover the spouses’ income. We then link the data for these four individuals and sum up their T4 incomes to construct the total household income.

Childcare expenses: The IID contains data on the calculated amount of childcare expense deductions. We create a dummy indicating whether an individual claimed any childcare expenditures in a given year as a proxy for formal childcare use.

In Canada, each childcare expense item can be claimed only by one of the spouses, who is generally the spouse in the lower income bracket. We combine the childcare expense data of both spouses to construct the total household childcare expense deductions. We then define using any (formal) childcare as having non-zero household childcare expense deductions.

One important note is that the childcare expense data for IID’s panel A is available only for a limited time period (from 2000 onwards) while panel B’s data is available for our entire analysis period. Therefore, we restrict our sample to individuals from panel B whenever using childcare expenses as an outcome variable in the IID. (The LAD is not subject to this problem)

A.2 Longitudinal Administrative Databank (LAD)

A.2.1 Sample selection

LAD covers a 20% representative sample of tax filers in Canada and does not differentiate across birth cohorts or citizenship status. It covers years 1982 to 2018. Even though LAD’s sample size is substantially smaller compared to that of the IID, LAD’s coverage of earlier birth cohorts enables us to capture pre-policy trends more accurately compared to the IID. However, the absence of intergenerational linkages restricts our usage of LAD for policy analyses purposes.

Our LAD sample consists of individuals who became parents before the age of 40 and after 18. We determine the age at becoming a parent using the children’s age data from LAD, which is described below. For each individual, we fill in the non-filing years, if there are any, with the latest available information prior to the non-filing year for demographics. We fill in zeros for earnings and income measures if the individual did not file taxes.

A.2.2 Individual-level variables

Parenthood: LAD contains the age of a tax filer’s children up to 7 children sorted from young to old, calculated by subtracting each child’s birth year from the tax year. In addition, the linked “kids” file contains the ages of each child within a household.

We code individuals older than 18 and younger than 40 who have at least one kid as a parent. To ensure that our parenthood indicator is accurate, we also utilize the reference mother and father IDs available in LAD’s kids file. The kids file contains the reference mother and father ID for each family. We only code an individual as a parent if they are referenced as a mother or father in these files.

Once we determine the parenthood status of all tax filers, we recover the birthdate of their children by subtracting the children’s age from the given tax year and picking the earliest birth year as the date the tax filer becomes a first-time parent.

Income: Similar to the IID, we use (pre-tax) T4 earnings as our primary income measure. Also similar to the IID, we winsorize earnings by the 99th percentile in each year, separately for men and women. If individuals do not file taxes in a given year, we fill the missing years as if they had zero T4 earnings in the given year. Estimates reported in Appendix Figure A5 use total employment earnings data from LAD, which also includes self-employment earnings.

Location: Each annual LAD file contains data on the tax filer’s province, CD, CSD, and postal code of residence. Similar to our IID sample, we harmonize the geography data across different census years to the 2016 census.

A.2.3 Household-level variables

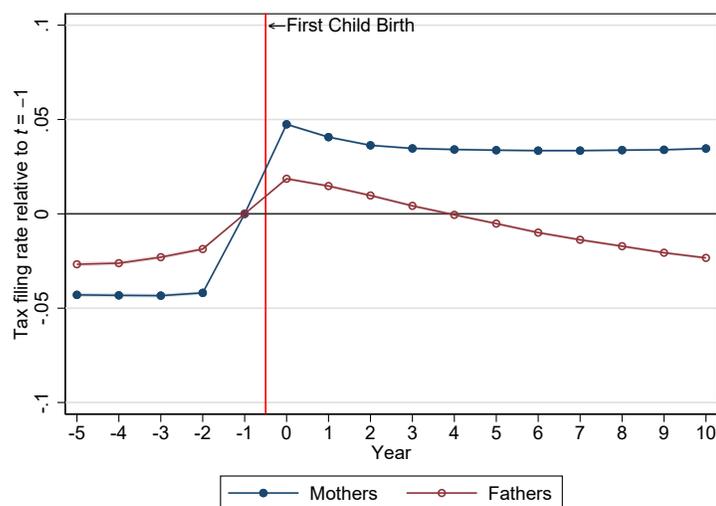
Childcare expenses: For childcare expenses, we directly use the total household childcare expense deduction data from LAD. We code an individual as using childcare if their household has positive childcare expense deductions in a given year.

A.3 Tax Filing in Canada

One potential issue with tax-based administrative datasets is that they only include individuals who file tax returns. For our purpose, there are two main sources of concern.

First, individuals are incentivized to file taxes to obtain social benefits. In particular, parents have stronger incentives than yet-to-be parents to file tax returns in order to claim child benefits. Indeed, tax filing rates increase precipitously around the time of childbirth. Figure A23 shows event-studies based on equation (1), where the outcome is a dummy for filing taxes and the sample includes all individuals aged 20-54 who ever file taxes in the LAD. This indicates that sample selection issues are likely more severe pre-childbirth. To address this issue, in most event-study plots of the impact of children (section 3), we only include individuals who file taxes at all event-times $t \in [-5, 10]$, as in Kleven et al. (2019a).

Figure A23: Impact of Children on Tax Filing



Notes: This figure reports estimates of child penalties based on equation (1), estimated on a sample of new parents in the Longitudinal Administrative Database (LAD). The sample includes all tax filers, and the outcome is an indicator for filing taxes in a given year. Shaded areas show 95% confidence intervals.

Second, in 1993 the federal government introduced a new Canada Child Tax Benefit, which replaced three other child tax credits. Quebec, however, maintained the administration

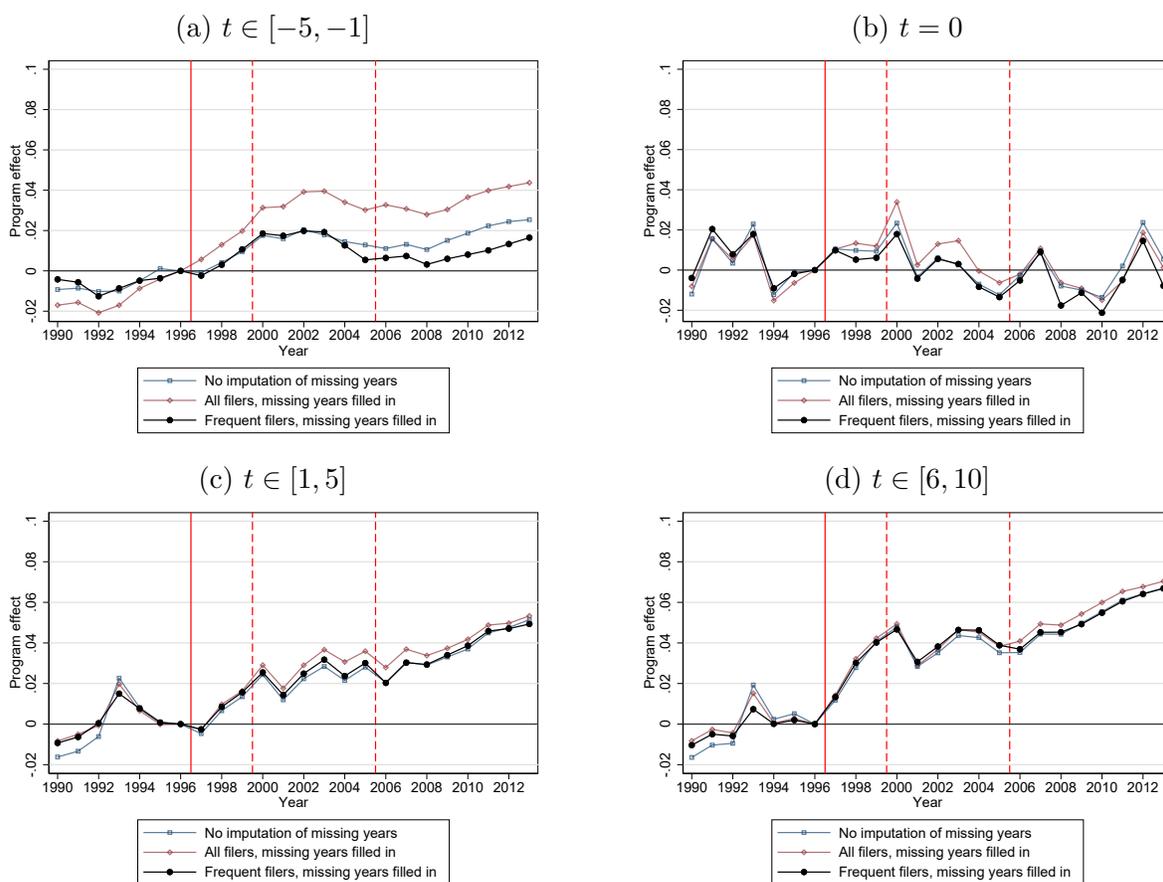
of their family allowance program. This policy change reinforced incentives for parents to file tax returns, but more so in the rest of Canada than in Quebec. Such differential trends in tax filing behavior could contaminate our estimates of pre-trends in the analysis of the impact of Quebec's childcare policy. To verify whether this is the case, we reproduce stacked difference-in-differences estimates of the effect of the program on employment at different event times for different subsamples. For visual clarity, we do not show confidence intervals.

Figure A24 shows results for three different subsamples. Red diamonds are used to depict results with no data imputation – that is, years in which individuals do not file taxes are excluded from the estimation sample. Blue squares show results based on a filled-in panel, where income in years individuals do not file a tax return is imputed a value of zero. The series in black is our preferred specification, which uses the filled-in panel, but only keep people who file taxes at least 90% of the time.

Imputing missing tax years dramatically affects Quebec-RoC differences in pre-childbirth years relative to no imputation. In contrast, imputing missing years but restricting the sample to frequent filers produces trends that are very similar to those based on no imputation, except for post-QPIP years where some divergence occurs. Our interpretation of these patterns is that imputation can produce fairly misleading results in pre-childbirth years, unless one conditions on frequent filers.

For post-childbirth years, imputation of missing tax years is mostly inconsequential. One exception is for the period 1990-1993, that is prior to the implementation of the 1993 Canada Child Tax Benefit. Here, failing to impute missing years generates considerable variation in Quebec-RoC differences in the first 4 years of the study window. In contrast, our preferred specification that restricts the scope for endogenous tax filing behavior shows much more stability in the early years. We take this as evidence that the 1993 "bump" in program effects is mostly an artefact of differential tax filing responses to the introduction of the Canada Child Tax Benefit.

Figure A24: Anticipatory and Dynamic Effects of Quebec’s Childcare Policy, Robustness to Tax Filing



Notes: This figure reports difference-in-differences estimates of the impact of childcare subsidies on employment, separately for different event times, and for three subsamples. In practice, we estimate the effect separately for each event time $t \in [-5, 10]$ using equation (4), and then take linear combination of coefficients for subgroups of event times (-5 to -1, 0, 1 to 5, and 6 to 10).

Appendix B: National Longitudinal Survey of Children and Youth (NLSCY)

For analyses reported in section 4, we rely on the National Longitudinal Survey of Children and Youth (NLSCY). These data were used in Baker et al. (2008), Baker et al. (2019), and Haeck et al. (2015) to study the Quebec childcare program. The NLSCY is a biennial survey, which was conducted the first time in 1994-95. We use the first 6 waves, covering the period 1994 to 2004. We do not use later years to avoid conflating the effect of the childcare policy with QPIP, which was put in place in 2006. In these data, the unit of observation is a child. We restrict the sample to children aged 0-5 years old.

Estimates shown in Figure 7 are based on the following estimating equation:

$$Y_{ips} = \alpha^{Qs} (\mathbf{I}_s^{Year} \times QC_i) + \gamma^s \mathbf{I}_s^{Year} + \gamma^c \mathbf{I}_c^{CMA} + \beta \mathbf{X}_{is} + v_{ips}.$$

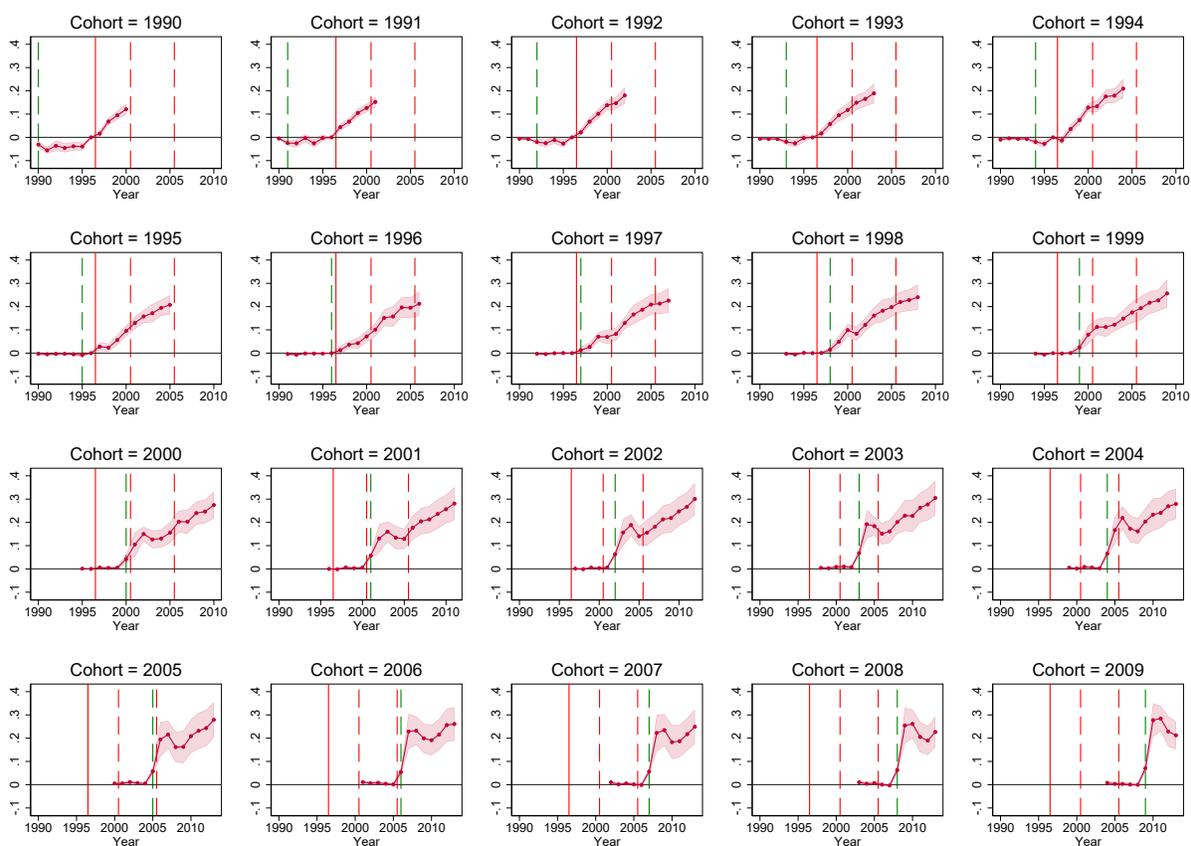
where Y_{ips} is an outcome variable indicating the use of different modes of care for child i residing in province p in calendar year s . The model includes year fixed effects and Census Metropolitan Areas (CMA) fixed effects, as well as a vector of control variables which includes gender and age. Standard errors are clustered at the CMA-level. The difference-in-differences coefficients reported on the figure are obtained by replacing the interaction terms $\mathbf{I}_s^{Year} \times QC_i$ with $Post_s \times QC_i$, where $Post_s = 1\{Year \geq 2000\}$. Figure A16 is based on that same estimating equation.

Appendix C: Cohort Effects

Estimating year-by-event time coefficients is equivalent to estimating year-by-birth cohort coefficients. For instance, the coefficient for event time $t = 4$ in year 2002 can also be interpreted as the coefficient for birth cohort 1998 in calendar year 2002. To further examine pre-trends around the time of the policy implementation, we convert our estimate of α_{ts}^{Qs} in birth cohort groups rather than event times. This way, we can plot cohort-specific trends in outcomes to verify whether changes around the time of the policy are driven by cohort effects. Cohort-specific coefficients are shown in Appendix Figures C.1, C.2, and C.3 for childcare expenses, earnings and employment, respectively. The first-stage impact on the childcare expenses claiming rate is particularly salient. For instance, early 1990s birth cohorts, which represent women who became mothers prior to the policy and so are observed with children both in the pre- and post-policy periods, evolve in parallel in Quebec and the Rest of Canada until 1997, when a sharp divergence emerges.

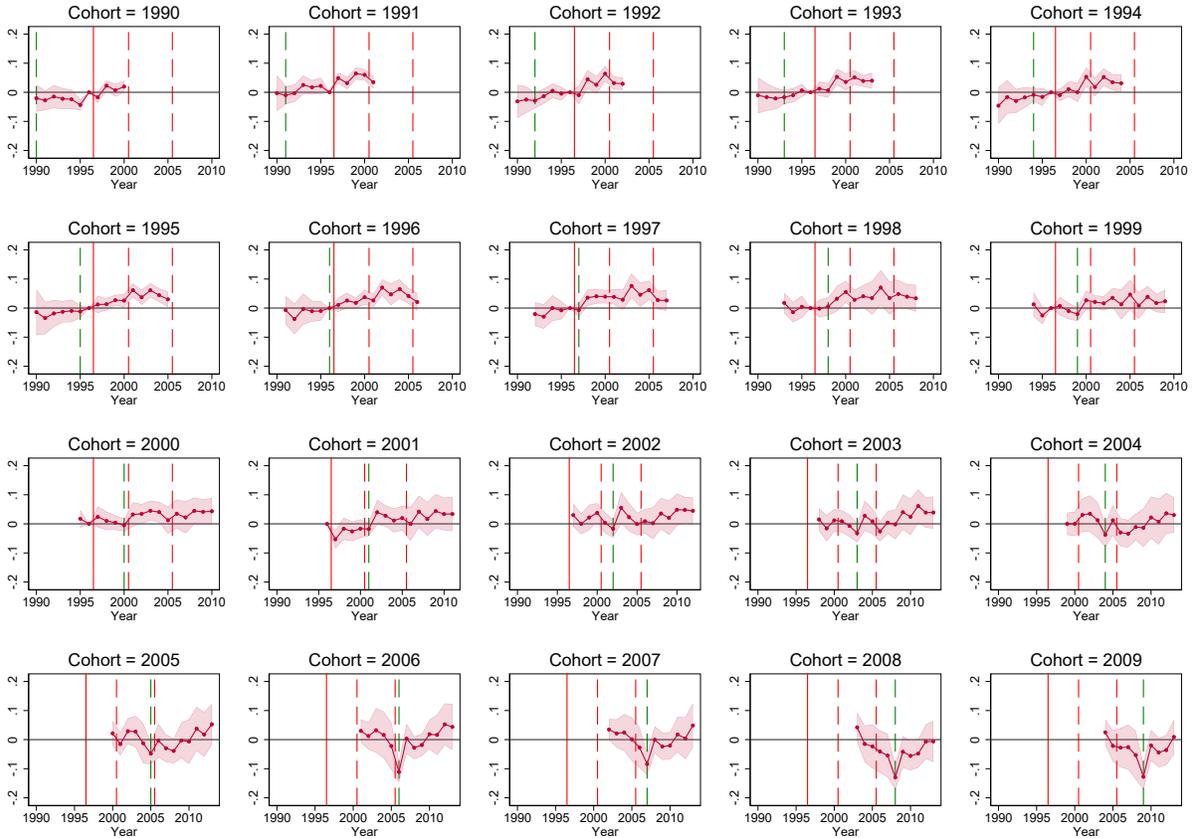
Cohort-specific trends for employment and earnings are considerably noisier, but some interesting patterns emerge. Birth cohorts of the early 1990s see substantial earnings increases after 1997. Among cohorts of women who became mothers between 1997 and 2002, a significant positive earnings differential appears in post-birth years relative to pre-birth years, consistent with the policy reducing the impact of children on earnings in Quebec. For later cohorts (2006-2010), there is a large earnings drop at $t = 0$ in Quebec relative to the rest of Canada that results from the implementation of QPIP. Patterns for employment are roughly similar, with one key difference: among later birth cohorts, employment rates of mothers are higher in Quebec both pre- *and* post-birth. This suggests that the policy may have permanently shifted the earnings and employment profiles of cohorts of women who became mothers after the policy was implemented.

Figure C.1: Effect of Quebec’s Childcare Policy on Childcare Expenses Claiming Rate, by Cohort



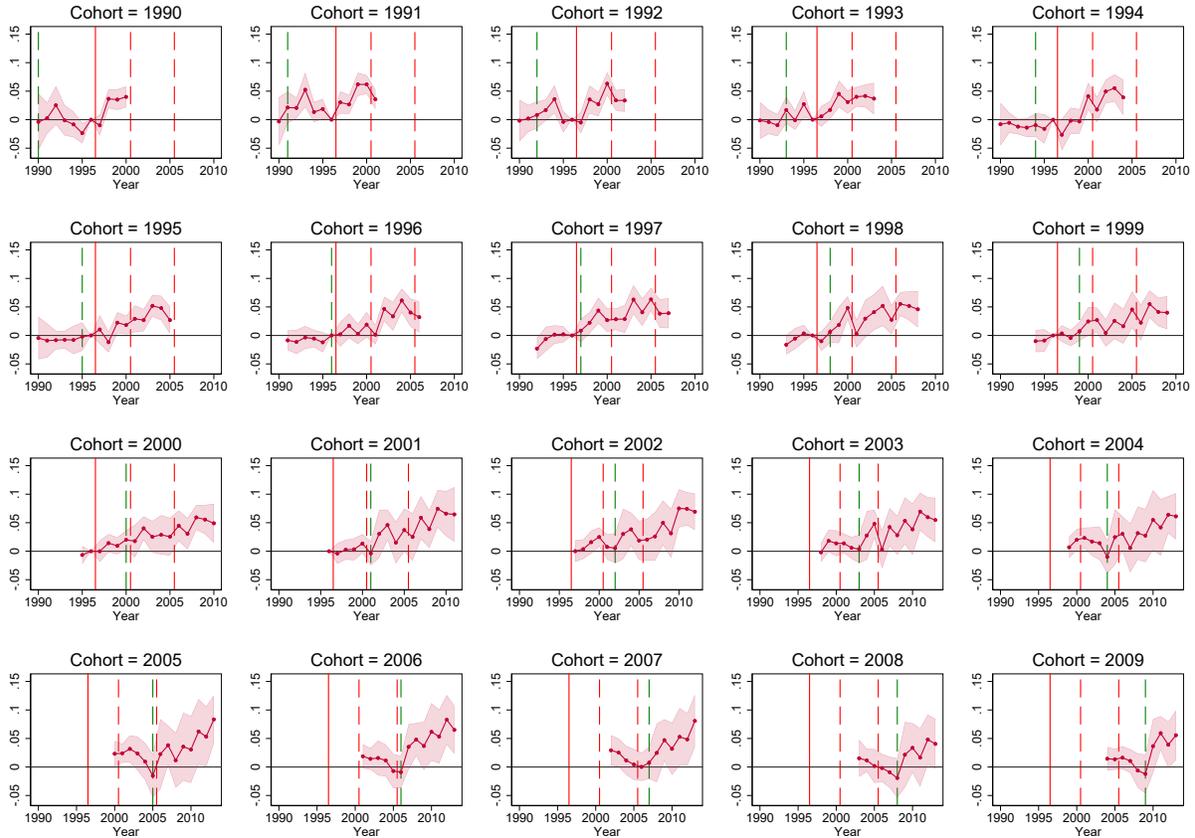
Notes: This figure presents estimates from equation (4) separately by cohort (year when women became mothers). All estimates are based on a sample of new mothers in the Longitudinal Administrative Databank (LAD). The solid red line indicates when Quebec’s childcare policy was implemented. The left-most dashed red line indicate when the policy was fully phased-in, and the right-most dash red lines indicate when QPIP was put in place. Green dashed lines indicate the cohort year. Shaded areas show 95% confidence intervals.

Figure C.2: Effect of Quebec’s Childcare Policy on Mothers’ Earnings, by Cohort



Notes: This figure presents estimates from equation (4) separately by cohort (year when women became mothers). All estimates are based on a sample of new mothers in the Longitudinal Administrative Databank (LAD). The solid red line indicates when Quebec’s childcare policy was implemented. The left-most dashed red line indicate when the policy was fully phased-in, and the right-most dash red lines indicate when QPIP was put in place. Green dashed lines indicate the cohort year. Shaded areas show 95% confidence intervals.

Figure C.3: Effect of Quebec's Childcare Policy on Mothers' Employment, by Cohort



Notes: This figure presents estimates from equation (4) separately by cohort (year when women became mothers). All estimates are based on a sample of new mothers in the Longitudinal Administrative Databank (LAD). The solid red line indicates when Quebec's childcare policy was implemented. The left-most dashed red line indicate when the policy was fully phased-in, and the right-most dash red lines indicate when QPIP was put in place. Green dashed lines indicate the cohort year. Shaded areas show 95% confidence intervals.

Appendix D: Group Shares

Using childcare takeup as the dependent variable in equation (6), we obtain estimates of the shares of compliers π_C^{cd} , which includes both p -compliers and g -compliers. Using post-policy average childcare take-up rates, we back out the share of always takers π_A^{cd} , and finally calculate the share of never takers as $\pi_N^{cd} = 1 - \pi_C^{cd} - \pi_A^{cd}$.

To break-down the shares of compliers into p -compliers and g -compliers, and the shares of never takers into p -never takers and g -never takers, we use a proxy for the fraction of families in which grandparents are the main caretakers. Ideally, we'd directly measure $G_i = 1\{D_i = g\}$, an indicator for grandparents being the main caretakers in family i , to calculate $\bar{G}_{cd,s} = E[G_i|S_i = s, cd]$ separately for each census division. Being unable to do so, we instead rely on a proxy $\tilde{G}_{cd,s} = E[\tilde{G}_i|S_i = s, cd]$. We assume that $\tilde{G}_i = (\frac{1}{\alpha}) G_i + \nu_i$, where ν_i is random measurement error that is unrelated to the childcare policy (i.e. $E[\nu_i|S_i = 0, cd] = E[\nu_i|S_i = 1, cd] = 0$). Under this assumption, we can estimate the share of g -compliers up to a scaling factor α :

$$\begin{aligned} \pi_{GC}^{cd} &= E[G_i|S_i = 0, cd] - E[G_i|S_i = 1, cd] \\ &= E\left[\alpha\left(\tilde{G}_i - \nu_i\right) | S_i = 0, cd\right] - E\left[\alpha\left(\tilde{G}_i - \nu_i\right) | S_i = 1, cd\right] \\ &= \alpha\left(E[\tilde{G}_i|S_i = 0, cd] - E[\tilde{G}_i|S_i = 1, cd]\right) \\ &= \alpha(\tilde{G}_{cd,0} - \tilde{G}_{cd,1}) \end{aligned}$$

In practice, we calculate $\tilde{G}_{cd,s}$ as the number of women aged 55 or older who provide at least 30 hours of unpaid childcare per week in census division cd in period s (where $s = 0$ is 1996, and $s = 1$ is 2006), normalized by the number of children aged 0-9 in census division cd in period s .³² Since shares cannot be negative, we add a scalar to measured differences $\tilde{G}_{cd,0} - \tilde{G}_{cd,1}$ so that the census division with the smallest (most negative) change has zero g -compliers. We then find the value of α such that $E[\pi_{GC}^{cd}] = 0.035$, the overall share of compliers for the province of Quebec reported in Figure 7. We then calculate the share of g -never takers as $\pi_{GN}^{cd} = \alpha\tilde{G}_{cd,1}$, and back out the share of p -never takers $\pi_{PN}^{cd} = \pi_N^{cd} - \pi_{GN}^{cd}$.

³²These data are compiled in Statistics Canada Tables 97-559-XCB2006015, 97-551-XCB2006006, 95F0186XDB96001 and 95F0239XDB96001.