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THE MULTIGENERATIONAL IMPACT OF CHILDREN AND CHILDCARE POLICIES

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ABSTRACT

This paper examines the multigenerational impact of children and whether the public provision of formal childcare lessens the earnings and employment impacts of children. We find that the arrival of a firstborn reduces employment and earnings of mothers and employment of grandmothers. Studying a universal childcare program in Quebec, we find 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 positive effects on mothers' employment and the pre-policy supply of informal childcare by grandmothers.

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A data appendix is available at http://www.nber.org/data-appendix/w32204

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.[2] 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 (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' labour supply are mixed, with estimates varying considerably across countries.[3] For example, Andresen and Nix (2022) finds that expanding access to child care in Norway reduced the child penalty by 23%, while Kleven et al. (2022) 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 the mixed findings?[4]

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. To put these analyses in context, we note that, compared to other OECD countries, Canada has a relatively high gender pay gap (among full-time earners) of 17%, well above the OECD average of 12% (OECD, 2023a). In contrast, Canada's gender gap in labour force participation (7 percentage points) is be-

[1] See Figure A1 for details.

[2] We use the terms "impact of children on women" and "motherhood effect" interchangeably. Both refer to changes in women's outcomes following the arrival of a firstborn. We use the term "child penalty" to refer to the impact of children on women relative to the impact on men.

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

[4] Appendix Figure A3 shows that the use of informal childcare is significantly larger in Austria than in Norway, where informal care providers are grandparents but could also include other relatives, friends, or neighbours.

low the OECD average of 11 percentage points. Formal childcare use in Canada is pretty high, whereas informal care use (including by grandparents) is relatively low (see Figure A3). However, these averages mask significant heterogeneity across Canadian provinces in both gender gaps and childcare use.

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 multigenerational impact of children on their parents and grandparents.[5] We find that the child earnings penalty in Canada on mothers relative to fathers 10 years after first childbirth is 25%, comparable to the penalty in Scandinavian countries (21% in Denmark and 26% in Sweden) and lower than in the US (31%), the UK (44%), Austria (51%), and Germany (61%) (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 the provision of childcare by grandparents. Consistent with this idea, we document a substantial impact of grandchildren on the 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.[6] 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 on mothers' and grandmothers' labour market outcomes, we exploit the introduction of a universal childcare program in the province of Quebec. Prior evaluations of this program have found significant concurrent increases in

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

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

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 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 "motherhood effect" as our main outcome of interest.

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 relative to yet-to-be 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 reduced their average earnings. 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 critical correlate is the *pre-policy* intensity of unpaid care provided by grandmothers. Mothers' employment increases far more in locations where grandparents play a less important role before the introduction of childcare subsidies. This finding suggests the potential effect of childcare subsidies on mothers' labour market outcomes depends on pre-existing care arrangements, and international comparisons of family policies should consider grandparents.

Our paper contributes to several streams of the vast literature on gender earnings inequality. Using data from several countries, a growing body of work has shown that the impact of children on earnings is significant, persistent, and falls disproportionately on women (Andresen and Nix, 2022; Cheng, 2020; Connolly et al., 2023; Lloyd, 2020; Kleven et al., 2019b; Sieppi and Pehkonen, 2019; Kuziemko et al., 2018; Bertrand et al., 2010). The 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' labour supply, suggesting that the total impact of children on women over the life cycle is larger than previously thought.

Our main contribution is to the literature on the impact of family policies on mothers' labour market outcomes. Findings are 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; Nollenberger and Rodríguez-Planas, 2015).

The papers closest to ours are Andresen and Nix (2022) and Kleven et al. (2022), both of which consider some version of the "motherhood effect" as their main outcome, as we do.[7] 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, encompassing 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. In Austria, childcare subsidies likely have no impact on mothers because grandparents are often the primary care providers. In contrast, Norway (like Quebec) has one of the lowest rates of informal childcare, and childcare subsidies induce large reductions in the motherhood effect. Thus, childcare policies *can* reduce the motherhood effect, but the extent to which they do depends on pre-existing childcare arrangements and the interplay between formal childcare and informal grandparent care.

Our work also relates to a smaller literature on the role of grandparents as caregivers and the implications for labour market outcomes. Most studies examine how the availability of grandparents as childcare providers affects maternal labour supply (Marcos, 2023; 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.

[7] Andresen and Nix (2022) estimate the impact of a childcare reform directly on the "child penalty," which is the relative impact of children on women versus men.

We additionally show that the grandmotherhood penalty varies considerably across places and correlates negatively with the motherhood penalty.

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. 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 maternity 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. Thus, QPIP is more generous than the federal plan in other provinces, incentivizing 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. 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.[8]

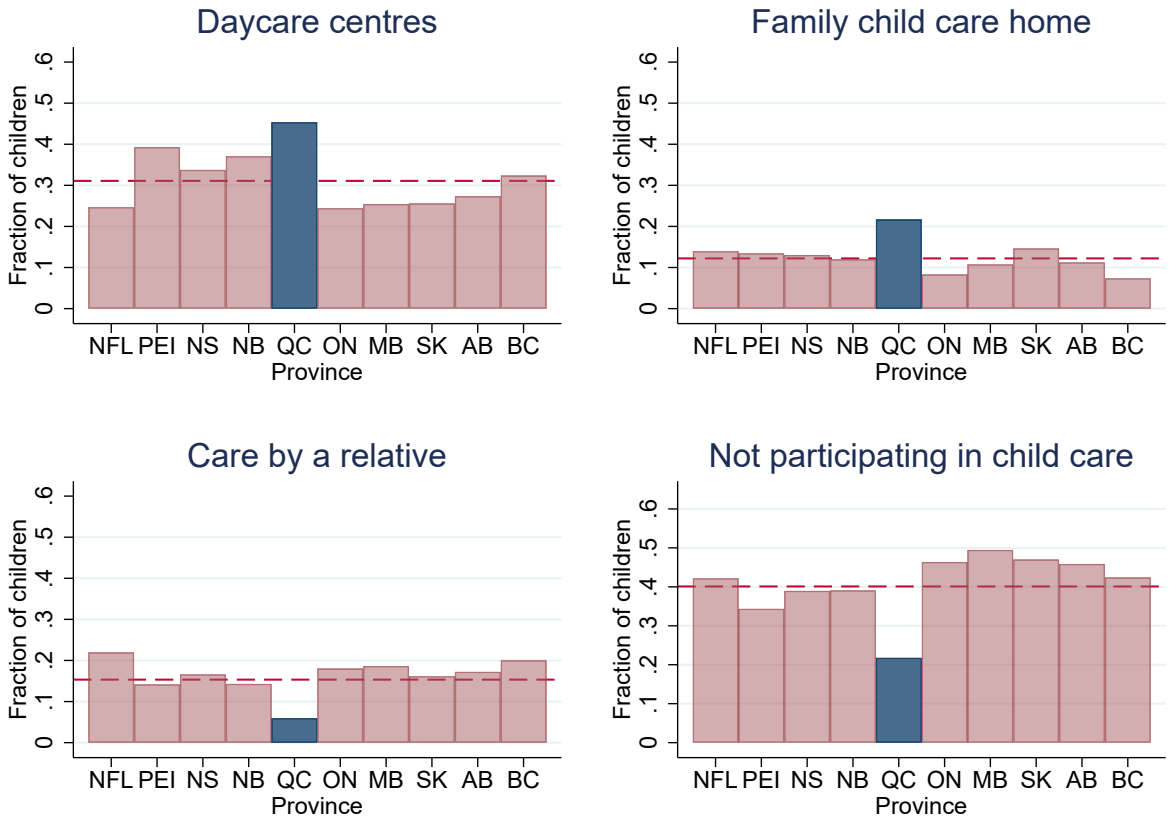
Quebec's Universal Childcare Program. First introduced in September 1997, Quebec's childcare policy provided generous subsidies to eligible childcare providers. Under this program, the out-of-pocket price charged to parents for a subsidized place was 5$ per day with the government covering the remaining costs. The program was gradually phased in for different age groups, and the number of subsidized places has steadily increased. Still, 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.[9] When reporting summary estimates of the program's impact, we only consider 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 for the year 2019. 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 the costs of childcare are important. Appendix Figure A2 shows that significantly fewer families in Quebec report not

[8]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, returning to 1970s levels by 2020 (Statistics Canada, 2022b).

[9]Haeck et al. (2015), Lefebvre and Merrigan (2008), and Baker et al. (2008) provide further details regarding the program's implementation.

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



Notes: All statistics are based on data from the Survey on Early Learning and Child Care Arrangements (Statistics Canada, 2022a) for year 2019. 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 childcare 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.

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.[10] 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 (ISSP, 2022) indicate that people in Quebec and the Rest of Canada are equally likely to think children suffer when the mother works and 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 the Intergenerational Income Database (IID) (Statistics Canada, 2020c). 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 (generation t), as well as their parents (generation $t-1$).[11] 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 and 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 from generation t of the IID. This sample includes individuals born between 1963 and 1985 who became parents before age 40 between 1981 and 2016. We infer the timing of parenthood using the date of birth of their dependents.[12]

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

[11] Except for birth cohorts 1971, 1976, and 1981, which are not included in the IID.

[12] Dependents are generally claimed in tax files by only one parent. We therefore use spousal linkages to identify the parenthood status of non-claiming parents. In the vast majority of cases, a woman claims the

To estimate the impact of grandchildren on grandparents, we consider a sample of potential grandparents. This sample includes all individuals from generation $t-1$ of the IID aged 40-85 between 1981 and 2016. For each individual, we identify grandparenthood status using each of their adult children's previously defined parenthood status. Note that grandparenthood status and its timing are likely measured with some error since we may not observe all children of individuals in generation $t-1$.[13] That is, all individuals in generation $t-1$ have at least one child, 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 generation $t-1$, independent of whether they become grandparents or not. 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 Canada, covering all birth cohorts as well as immigrants (Statistics Canada, 2020d). 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.[14] 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).[15] 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

dependent. The Data Appendix provides details on the use of spousal links.

[13]For instance, suppose an individual had a child in 1961 and another one in 1963. This person will be part of the IID's generation $t-1$ data through their child born in 1963, but the existence of the child born in 1961 would be unknown to us since that birth cohort is not covered by generation t of the IID.

[14]The 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.

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

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.[16]

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.[17]

Note that our administrative datasets 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 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 D.3.

3 The Impact of Parenthood and Grandparenthood

We start by estimating the impact of first childbirth on parents and grandparents in Canada, pooling all provinces and years together. Our empirical approach largely follows the methodology developed in Kleven et al. (2019a).

[16]The 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.

[17]Generally, 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.

3.1 Econometric framework

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

$$Y_{ist}^{g} = \boldsymbol{\alpha}^{\boldsymbol{g}} \boldsymbol{I}_{t}^{Event} + \boldsymbol{\beta}^{\boldsymbol{g}} \boldsymbol{I}_{is}^{Age} + \boldsymbol{\gamma}^{\boldsymbol{g}} \boldsymbol{I}_{s}^{Year} + v_{ist}^{g} \tag{1}$$

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

In our preferred specification, we consider an event window of $t \in [-5, 10]$ and focus on parents and potential 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 a value of 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:

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

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 reports estimates of the impact of parenthood on several outcomes for the sample of new parents in the IID. Panels (a) and (b) shows that earnings and employment 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 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 penalty 10 years after first childbirth for mothers relative to fathers is 25%, similar

to Denmark (21%) and Sweden (26%) but smaller than in the US (31%), the UK (44%), Austria (51%), and Germany (61%) (Kleven et al., 2019b).[18]

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 CD as one's parents changes with childbirth. Before childbirth, men and women gradually become less likely to live in the same CD as their parents. The trend reverses sharply at event time 0, particularly for mothers, suggesting families value proximity more after a (grand)child arrives. One likely reason is that grandparents may provide informal childcare. This trend break can be driven by changes in the new parent's or the grandparents' residential location decision. To study whose location decision contributes the most to these patterns, Appendix Figure A11 examines whether or not parents reside in the same CD at event time t as at birth. We conjecture that in 30% of cases, grandparents are revising their location decision because parents continue living in the same CD as at childbirth. Conversely, in 70% of cases, parents move as the CD at event time t differs from the one at childbirth.

To further probe grandparents' role for child penalties, Appendix B.2 estimates the impact of children separately for mothers who do and do not live in the same CD as their parents at event time 0.[19] Appendix Figure A7 shows that motherhood effects are significantly lower for mothers in the same CD as their parents.[20] Moreover, 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.

3.3 The Impact of Grandchildren on Grandparents

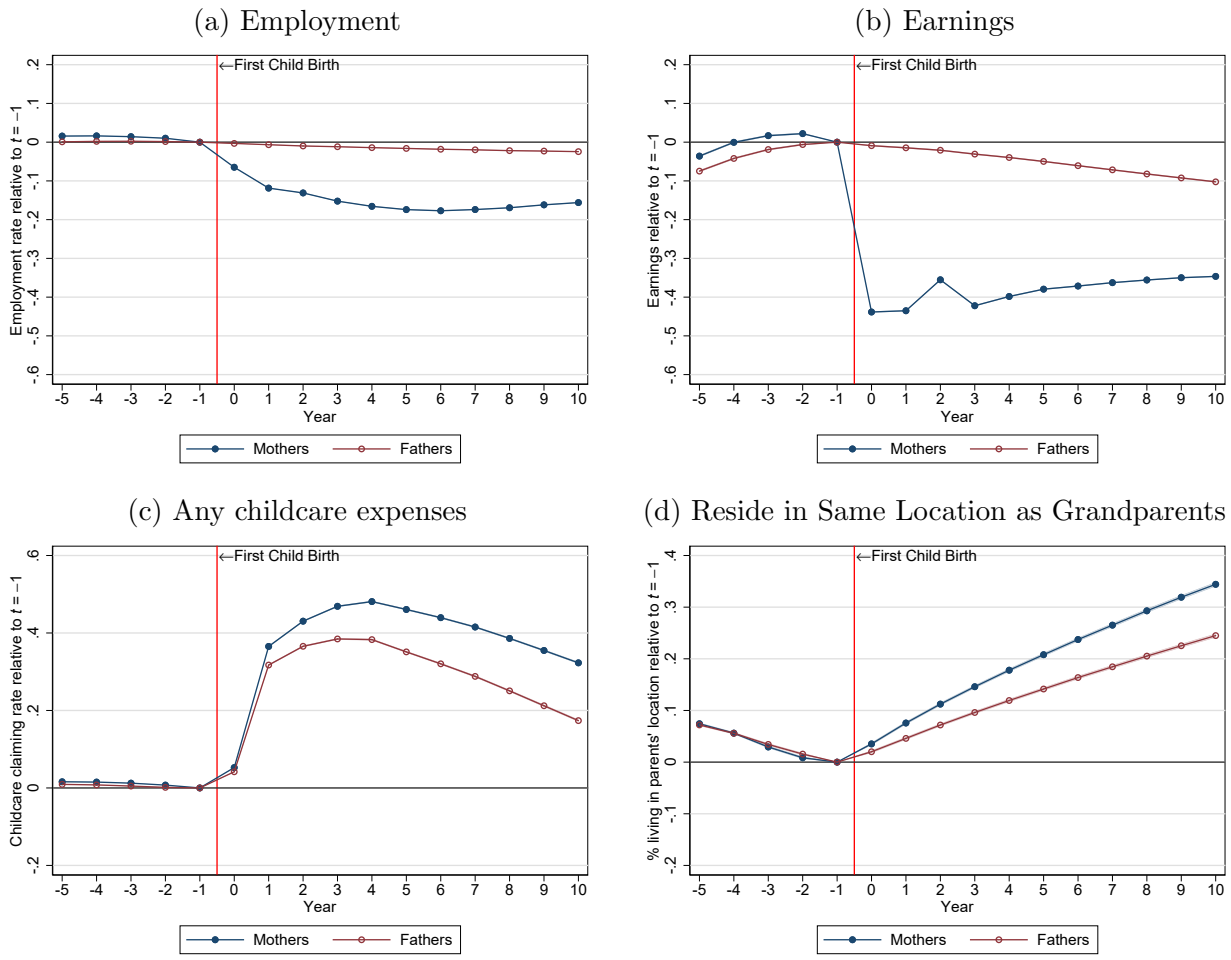
Figure 3 shows the effect of grandparenthood on earnings and employment, estimated on the sample of potential grandparents in the IID. The trend in employment is flat for grandparents before grandparenthood but turns negative precisely after the arrival of the first grandchild. Employment at event time 10 drops by 8% for grandmothers and by 6% for grandfathers. The trend in earnings of grandmothers and grandfathers is similar but is already declining

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

[19]We further report analyses of heterogeneity by (1) urban or rural area, and (2) number of children in Appendix B.2.

[20]The employment and earnings drop of mothers in the same CD as their parents are 4 percentage points lower five years after the first child's birth. The earnings gaps when we do not scale event time coefficients by the baseline earnings absent children are larger since mothers not in the same CD have higher baseline earnings than those in the same CD. The un-scaled earnings drop is \$3,300 smaller for mothers in the same CD relative to those not in the same CD (about 18.5% of the earnings drop for mothers in the same CD.)

Figure 2: The Impact of Children on Parents

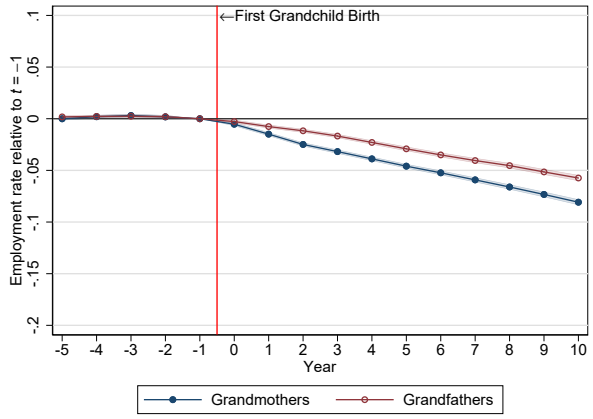


Notes: This figure reports estimates of child penalties based on equation (1), estimated on a sample of new parents in the Intergenerational Income Database (IID). In panel (a), the outcome is a dichotomous variable for any T4 earnings, and in panel (b) it is T4 earnings. In panel (c) the outcome is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

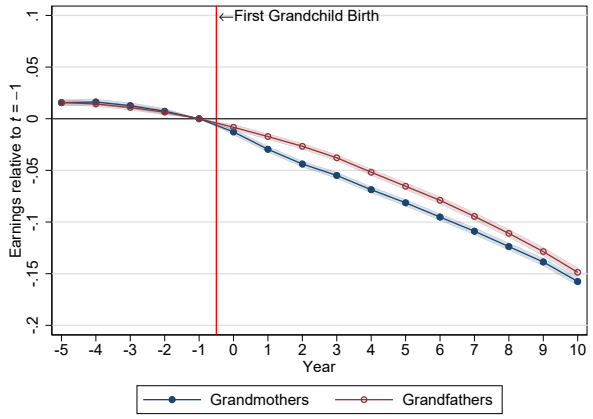
before grandparenthood. It turns negative after the first grandchild's birth. The decline is slightly steeper for grandmothers, particularly after maternity leave expires at event time 1, but the gap gradually closes again. The long-run earnings impact of grandparenthood at event time 10 is 16% for grandmothers 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.[21]

Figure 3: 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 (they are barely visible because point estimates are extremely precise).

As grandparenthood is associated with large and persistent labour supply reductions, a natural question is whether grandparents compensate for some of the associated income losses by taking up pension benefits. Appendix Figure A12 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 pension take-up and pension income steadily increase after

[21] As Kleven et al. (2019a) discuss, the event study estimates in Figure 3 will be biased if we do not fully control for non-grandchild-related labour supply determinants (e.g., age, ability, and preferences). Capturing these determinants is challenging as the impact of grandchildren on grandparents is more gradual and grows over time. Appendix B.1 explores the robustness of our estimates when controlling more flexibly for life-cycle and time trends, and when using a difference-in-differences event study in which we assign placebo grandchildren to individuals without grandchildren. Both robustness checks produce impacts of grandchildren that are similar to our baseline event study in Figure 3.

becoming a grandparent.

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 of children 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. Since coefficient estimates at positive event times are relative to pre-childbirth outcomes, we refer to those as *motherhood effects* and *grandmotherhood effects*.

Figure 4 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 we later study.[22] 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 25% to 50% is associated with a reduction in the motherhood impact from 48% to 37%. 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 grandmotherhood effect from 6% to 0% is associated with an increase in the motherhood effect of 9 percentage points, from 37% to 46%. The relationship is considerably weaker for employment.

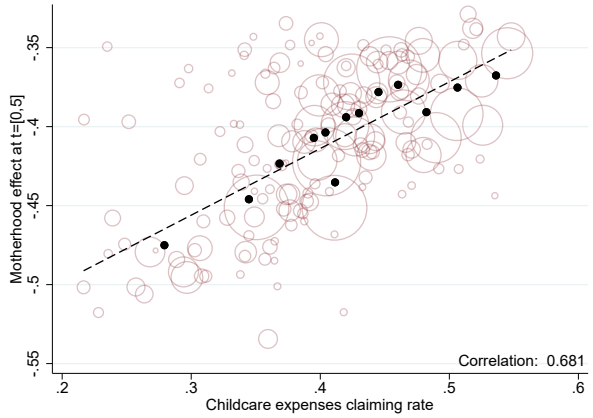
Finally, we plot grandmotherhood effects against childcare expense claiming rates in panels (e) and (f). While there is no relationship for employment, a weak negative association

[22]For completeness, we show corresponding graphs that include Quebec in Figure A13. We also show maps for each of the three variables in Figures A14, A15, and A16.

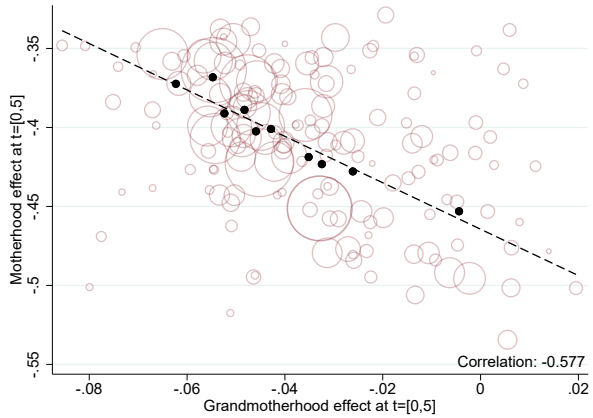
Figure 4: Spatial Correlations of the Impacts of Children

Earnings

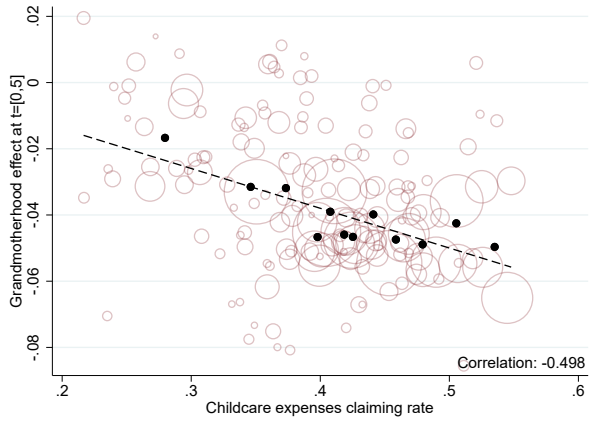
(a) Motherhood effect vs. Childcare Take-up



(c) Motherhood vs Grandmotherhood effect

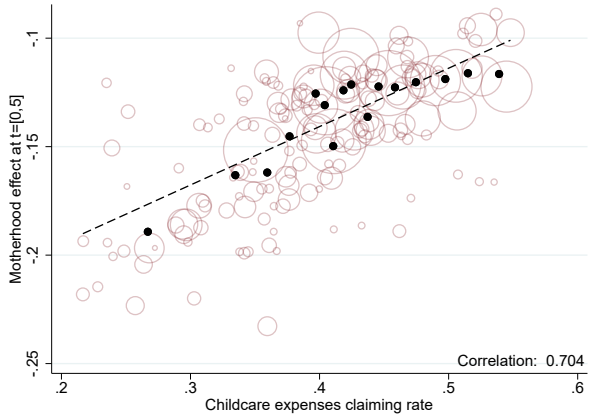


(e) Grandmotherhood effect vs. Childcare Take-up

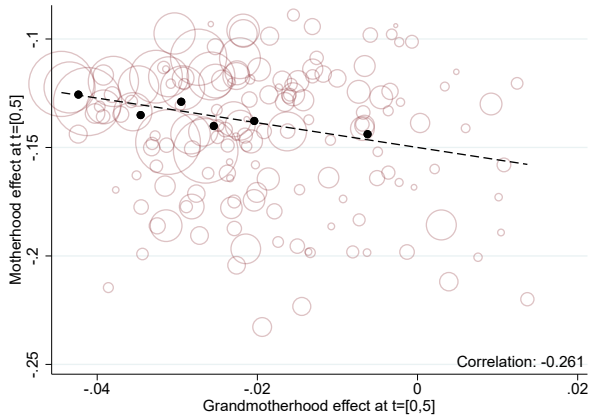


Employment

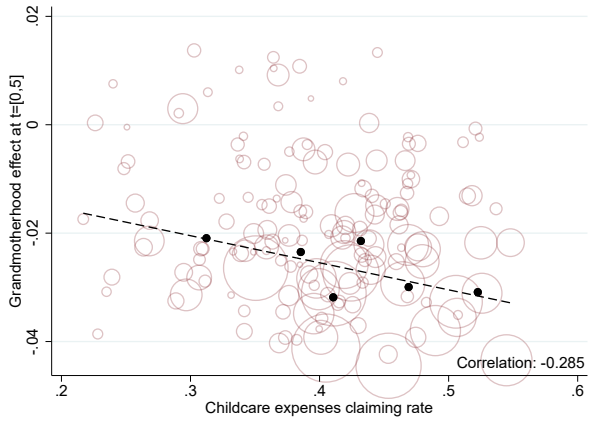
(b) Motherhood effect vs. Childcare Take-up



(d) Motherhood vs Grandmotherhood effect



(f) Grandmotherhood effect vs. Childcare Take-up



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 expense 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.

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 potential outcome framework to analyze how childcare subsidies impact labour supply in a setting with multiple modes of care. In this environment, the effects of childcare subsidies are heterogeneous and depend on the counterfactual mode of care in the absence of the subsidy. To connect this interpretative framework with our empirical setting, we then document patterns of substitution between modes of care in Canada using survey data.

4.1 A Conceptual Framework for Interpreting 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 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. Always-takers use formal childcare whether they are eligible for the subsidy or not. In contrast, families who use formal childcare *only* when eligible for the subsidy consist of 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. Below, we discuss some potential channels through which childcare subsidies may affect groups differently.

At the extensive margin of labor supply, subsidies can draw counterfactual caretakers who otherwise stay home into the labour force if daycare costs constitute a fixed cost of working. This crowding-in would increase employment and earnings of mothers in p-complier families and grandmothers in g-complier families but leave employment unchanged in always-taker and never-taker families.

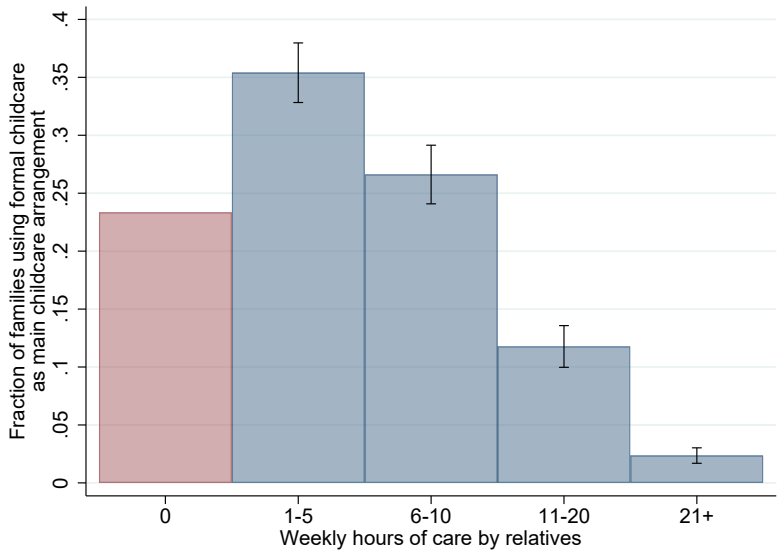
At the intensive margin, subsidies reduce the fixed costs of working, which has a negative income effect for always takers, possibly reducing mothers' hours of work and earnings. However, childcare subsidies may also affect long-term career decisions in ways that could offset such negative effects on earnings for always takers. For instance, childcare subsidies can facilitate mothers' return to work—particularly if they improve the stability and predictability of formal childcare—and thereby increase incentives to pursue occupations characterized by long work hours.

Importantly, subsidies can also affect *secondary* caretakers in complier families. For instance, if daycare is not perfectly reliable (e.g., sick children cannot attend daycare and must remain in parental or grandparental care), subsidies may reduce the earnings of secondary caretakers 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. That is, while formal care and informal care by grandparents are substitutes for g-compliers (by definition), they could complement each other in p-complier families. We turn to survey data to explore the validity of this intuition.

4.2 Patterns of Substitution Across Modes of Care

We use data from the National Longitudinal Survey of Children and Youth (NLSCY) to evaluate substitution patterns across modes of care (Statistics Canada, 2020e).[23] 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.

Figure 5: Joint Use of Formal Care and Informal Care by Relatives

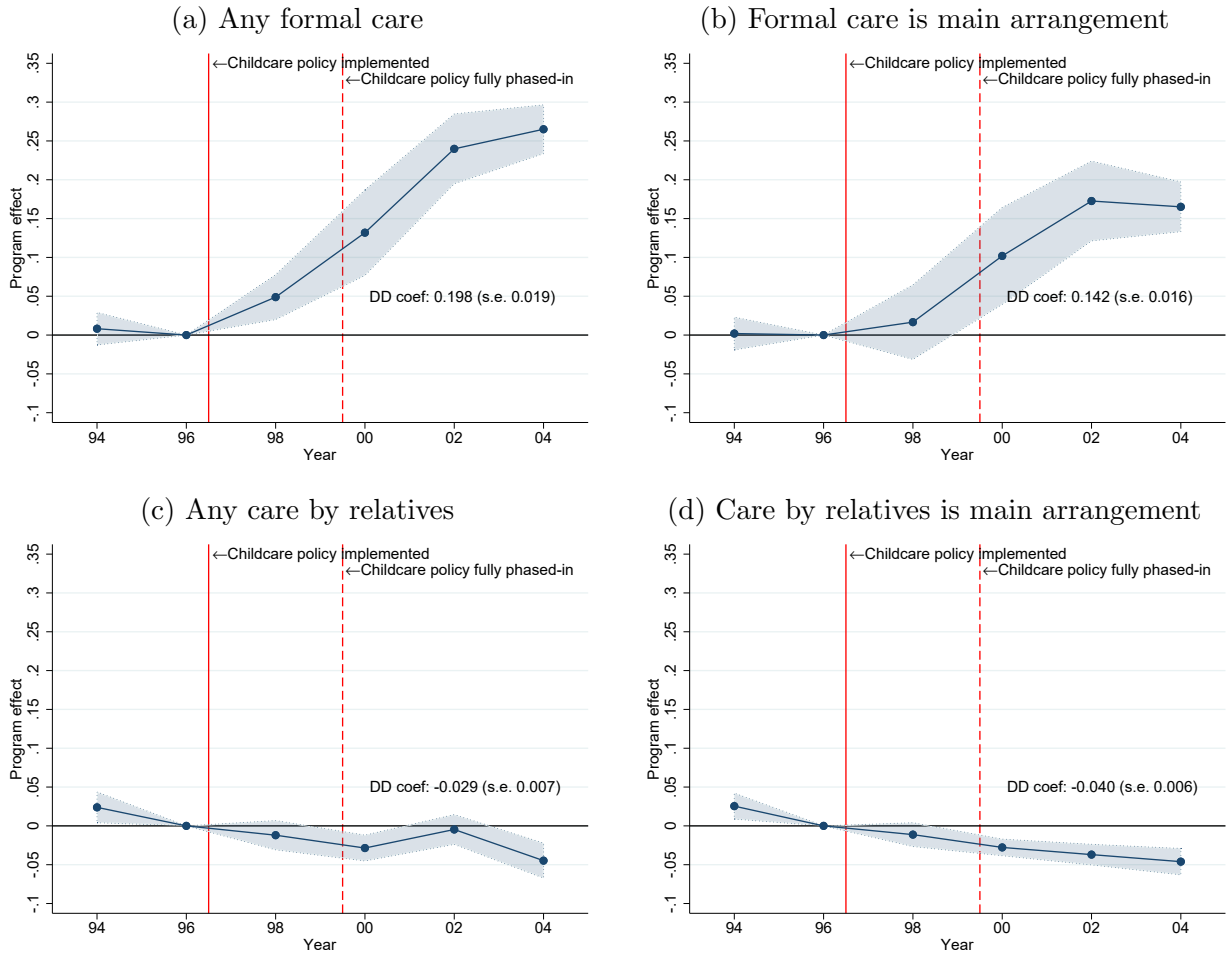


Notes: The figure plots the fraction of families of children aged 0-5 whose main childcare arrangement is either a formal childcare centre or a dayhome, as a function of the weekly number of care provided by relatives. The statistics are based on data pooling all first 6 waves of the NLSCY (covering 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.

We study differences in the weekly number of hours of care provided by relatives (most likely grandparents) to examine the complementarity and substitutability between formal and informal care. Figure 5 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.

[23]These 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. These data and the empirical specifications implemented below are described in Appendix D.4.

Figure 6: 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 D.4.

We next examine how modes of care for children aged 0-5 change around the time of the reform in Quebec relative to the Rest of Canada. Figure 6 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 effect of the policy on these measures of daycare use pins down the share of families who use formal childcare only when subsidies are available, which corresponds to the combined share of p and g-compliers. The corresponding difference-in-differences coefficients indicate that 14 to 20% of the sample are complier families. Panels (c) and (d) reproduce these analyses for care provided by relatives. The results show that in response to the policy, the share of families whose main caregiver is a relative decreased by 3 to 4 percentage points. That is, 3 to 4% of families in the sample opt for care by relative in the absence of the subsidy but switch to formal care when eligible for the subsidy. If grandparents provide most care by relatives, then the difference-in-differences estimates imply that 3 to 4% of the sample are g-complier families, with the caveat that there is a slight negative pre-trend. Overall, these analyses suggest that there are about four times more p-complier families than g-compliers.

Moreover, Appendix Figure A17 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 are consistent with the idea that childcare policies change both parents' and grandparents' supply of informal care and that formal care can substitute or complement informal care.

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

	Mothers'		Grandmothers'	
	Employment	Earnings	Employment	Earnings
p-compliers	+	+	0	-
g-compliers	0	-	+	+
always takers	0	-	0	0
p-never takers	0	0	0	0
g-never takers	0	0	0	0
Average effect	+	? (+)	+	? (-)

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

Finally, we analyze this survey evidence through the lens of our conceptual framework to make best-informed predictions on the *sign* of the employment and earnings effects of

childcare subsidies in the Canadian context. Table 1 summarizes these predictions. The average employment effect is unambiguously positive for mothers and grandmothers.[24] In contrast, the sign of the overall impact on mothers' and grandmother's earnings is ambiguous. For grandmothers, our survey evidence suggests earnings may decline. The reason is that (1) the earnings effect is likely negative in p-complier families since formal and infrequent informal care are complements, and (2) there are more p-complier than g-complier families.[25] For mothers, the sign of the earnings response additionally depends on the share of always takers. We expect a positive net effect if the income effect for always-takers is small and if there are more p-compliers than g-compliers families.

Table 1 also provides a guide for interpreting differences in the *magnitude* of the effects of childcare subsidies on mothers' outcomes as a function of 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 evaluate whether this is the case in section 6 by estimating mothers' employment and earnings responses separately for each Census Division within Quebec. We then correlate these responses with estimated group shares in each Census Division.

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 the motherhood effect and validate that our results are consistent with prior studies. We then use the IID to document whether the policy affects the grandmotherhood effect.

5.1 Average Impact of the Childcare Policy on Motherhood Effects

Empirical Specification. Our main object of interest is the motherhood effect on employment and earnings, which refers to the impact of children on these outcomes relative

[24] Some grandparents may have to withdraw entirely from the labour force to provide infrequent care in p-compliers families. We believe that such negative employment effects are likely negligible in our setting given that the supply of childcare hours by relatives only goes up at 5 hours per week or less (see Figure A17).

[25] Moreover, 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.

to pre-motherhood years. To estimate the effect of Quebec's childcare policy on the motherhood effect, we implement a triple-difference design, where we compare across provinces, time, and parenthood status. We estimate the following equation

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

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). $\boldsymbol{I}_t^{Year}$ is a vector of calendar year dummies and $\boldsymbol{X}_{is}$ 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.[26]

The coefficients of interest, α_s^{Qs}, measure how the motherhood effect—the differences in outcomes between mothers and yet-to-be mothers—for women living in Quebec in year s differs from women 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.[27]

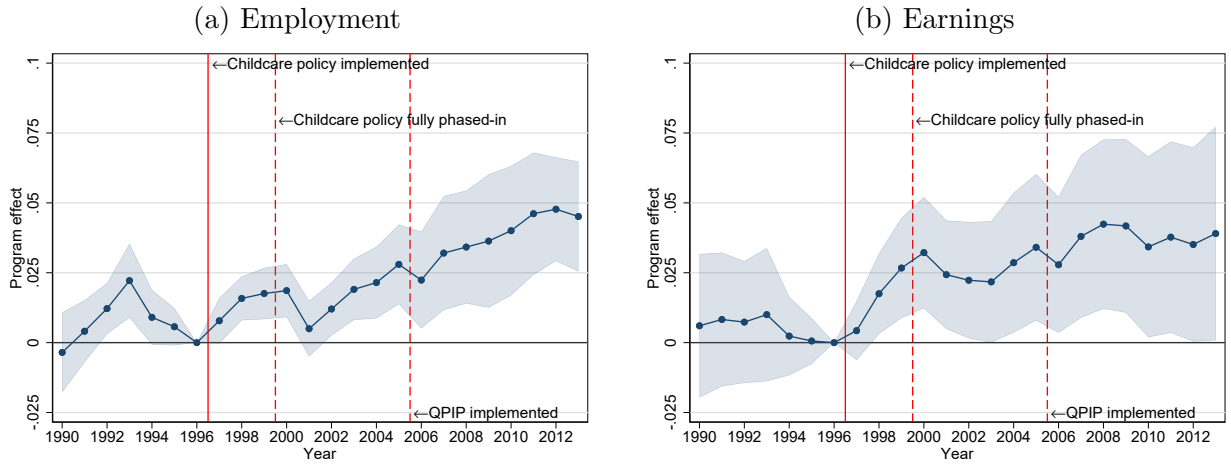
Our specification differs from difference-in-differences designs in prior work on Quebec's childcare policy (e.g., Haeck et al., 2015; Baker et al., 2008) in that we normalize mothers' outcomes relative to those of yet-to-be mothers. The identification relies on differential trends between women who 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 express our estimates directly in terms of the motherhood effect. It also implicitly accounts for differential trends in women's employment between Quebec and the rest of Canada (i.e., changes common to women with and without children). If the childcare policy positively affected the outcomes of yet-to-be mothers through anticipatory effects, then our triple-difference estimates will understate the policy's effect on mothers' labour market outcomes. We examine this possibility below using a stacked difference-in-differences design.

[26]Tax 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 D.3 discusses endogenous tax filing in more detail.

[27]Analytical 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.

Main Results and Robustness. Figure 7 presents our main estimates of the post-childbirth employment and earnings impacts of Quebec's childcare policy.[28] Mothers see weak increases in earnings and employment during the phase-in years, consistent with many not yet 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 D.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.

Figure 7: Impact of Quebec's Childcare Policy on the Motherhood Effect



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.

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 A19 shows that results are robust to this alternative coding.

[28] Appendix Figure A18 shows that the policy increased formal childcare take-up after the first child's birth 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.

Our main outcome variables are based on T4 earnings, capturing earnings from employment. Jeon and Ostrovsky (2019) and Lloyd (2020) show that parenthood is associated with an increase in the likelihood of self-employment. To allow for this additional margin of adjustment, Appendix Figure A20 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 yet-to-be mothers respond to the policy change before childbirth.[29] 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

$$\begin{aligned} Y_{ist} = &\boldsymbol{\alpha^{Qs}} \left(\boldsymbol{I}_t^{Event} \times \boldsymbol{I}_s^{Year} \times QC_{is} \right) + \boldsymbol{\alpha^{s}} \left(\boldsymbol{I}_t^{Event} \times \boldsymbol{I}_s^{Year} \right) \\ &+ \boldsymbol{\alpha^{Q}} \left(\boldsymbol{I}_t^{Event} \times QC_{is} \right) + \boldsymbol{\beta X}_{is} + v_{ist}. \end{aligned} \tag{4}$$

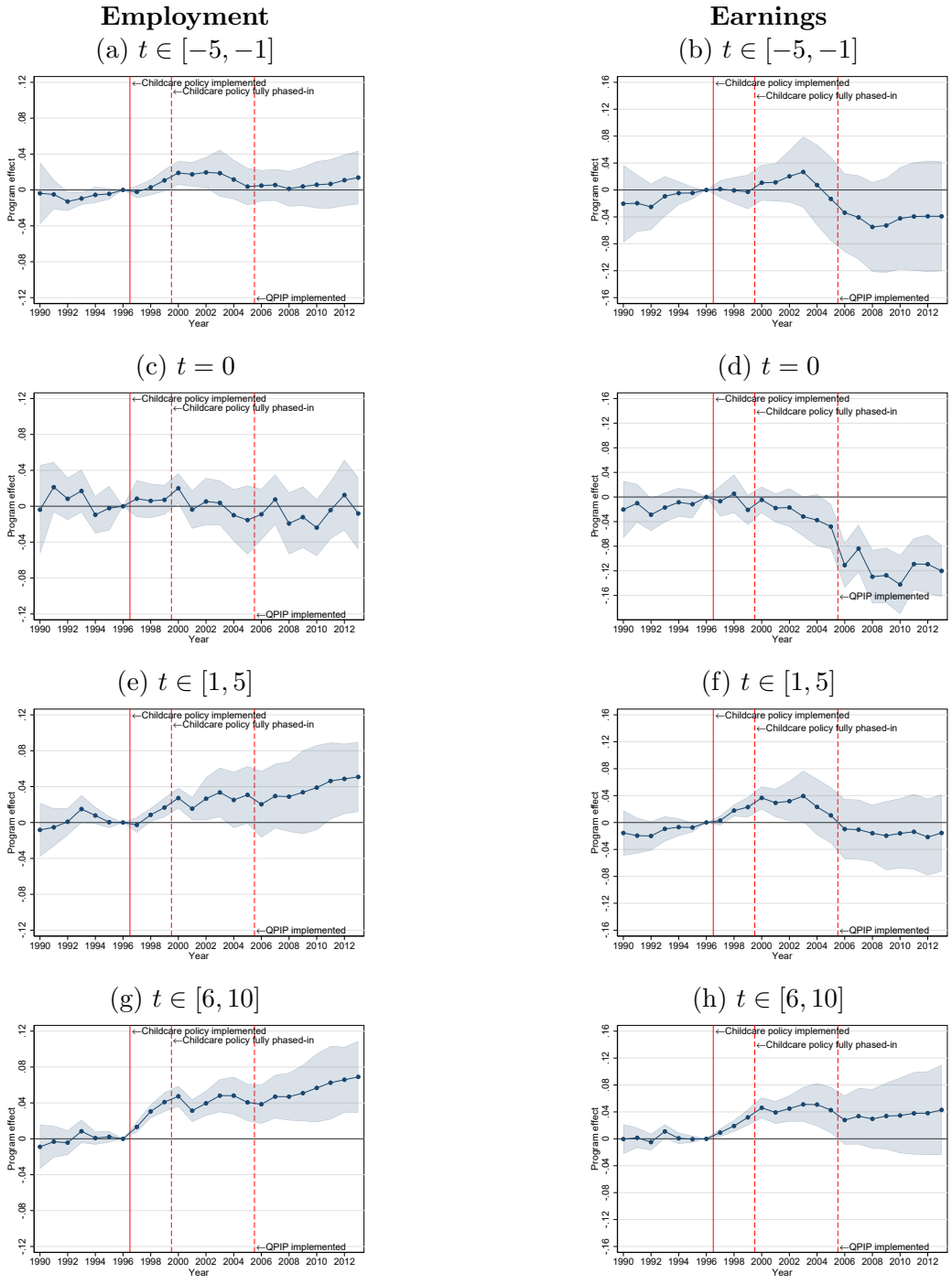
The coefficients of interest, α_{ts}^{Qs}, indicate how outcomes in Quebec at event time t and year s differ from outcomes in the rest of Canada at that same event time and year. This approach effectively corresponds to a stacked difference-in-differences specification (Kleven et al., 2022). It is similar to estimating separate difference-in-differences models for each event time t, but the stacked model allows us to express coefficients in percentages by re-scaling them by predicted outcomes $E[\tilde{Y}_{ist}^{g}|t, QC_{is} = 1]$.

The causal interpretation relies on a different set of assumptions than the triple-difference approach considered before. The stacked difference-in-differences estimates are only valid under the usual parallel trend assumption that outcomes for women 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 E, 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.

Figure 8 presents the results, pooling event times $t \in [-5, -1]$, $t \in [1, 5]$, and $t \in [6, 10]$

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

Figure 8: 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.

for visual clarity.[30] We also show estimates for event-time $t = 0$ separately 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, although this effect is short-lived. This finding provides suggestive evidence that some women may change career plans in response to the childcare policy even before they become mothers. It 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 first childbirth ($t = 0$). Employment then rises by 4 percent at $t \in [1, 5]$ and $t \in [6, 10]$ over the 2000-2005 period and increases further in later years.

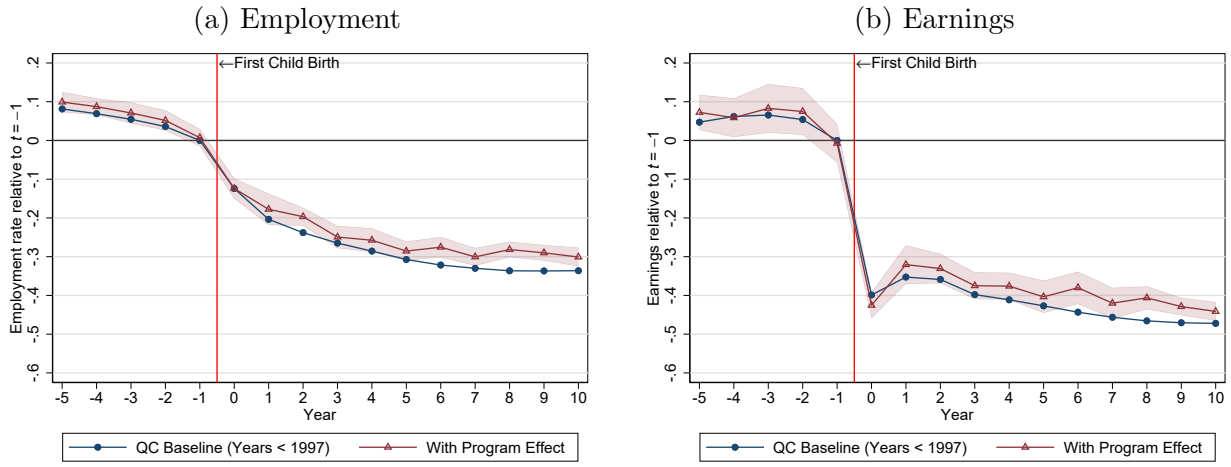
We find no clear anticipatory effects on earnings, but the impact of QPIP on earnings at childbirth is visually striking. Earnings at $t = 0$, when new mothers are eligible for parental leave, decline significantly after 2005. The introduction of QPIP likely also contaminates post-childbirth earnings 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 the years 2000-2005 as the relevant post-treatment period when summarizing the impact of the childcare policy moving forward.

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.[31] What do our estimates imply for the overall motherhood effect? Figure 9 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

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

[31] Our 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 have much lower base rates.

Figure 9: Impact of Quebec's Childcare Policy on Women's Earnings and Employment



Notes: This figure reports the estimated policy effect of Quebec's childcare program on women's outcomes at different event times. 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 of the impact of children 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.

baseline employment drop of 33 percentage points.[32]

5.2 Average Impact of the Childcare Policy on Grandmotherhood Effects

Empirical Specification. To estimate the impact of childcare subsidies on the grandmotherhood effect, 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:

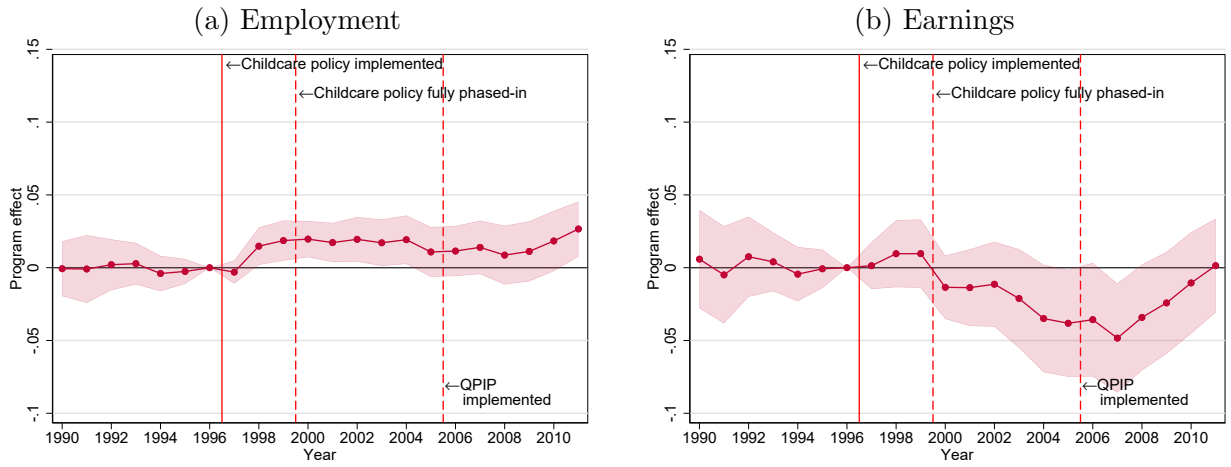
$$\begin{aligned} Y_{ist} = &\boldsymbol{\alpha^{Qs}}\left(GP_{ist} \times \boldsymbol{I}_s^{Year} \times QC_{is}\right) + \boldsymbol{\alpha^{s}}\left(GP_{ist} \times \boldsymbol{I}_s^{Year}\right) \\ &+ \boldsymbol{\gamma^{p}}\left(\boldsymbol{I}_s^{Year} \times \boldsymbol{I}_i^{Prov}\right) + \boldsymbol{\alpha^{p}}\left(GP_{ist} \times \boldsymbol{I}_i^{Prov}\right) + \boldsymbol{\beta X}_{is} + v_{ist}. \end{aligned} \tag{5}$$

where GP_{ist} is a dummy that takes a value of 1 if $t \in [1, 10]$ and the grandparent lives

[32]It 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)).

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. $\boldsymbol{I}_t^{Prov}$ is a vector of province dummies, which we interact with a vector of calendar year dummies $\boldsymbol{I}_t^{Year}$ to account for province-specific changes in retirement policies. $\boldsymbol{X}_{is}$ 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).[33] 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}, 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 10: Impact of Quebec's Childcare Policy on the Grandmotherhood Effect



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 10 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

[33]Conditional 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.

line with our predictions, employment increases by about 2 percent following the policy's implementation and remains at that level in the long run.[34] 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 A21, 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 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 alway-taker shares in conformity with the predictions we presented in Table 1.

We estimate the effect of the program on mothers' outcomes separately by Census Division using the following parsimonious difference-in-differences model:

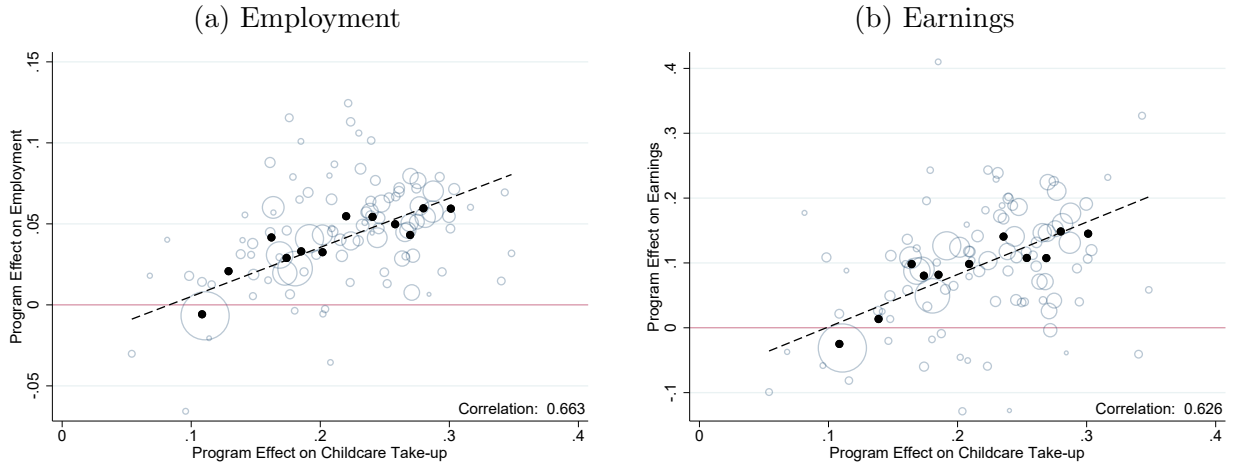
$$Y_{icst} = \boldsymbol{\alpha^{Qc}} \left(\boldsymbol{I}_c^{CD} \times QC_i \times Post_s \right) + \boldsymbol{\gamma^s} \boldsymbol{I}_s^{Year} + \boldsymbol{\gamma^t} \boldsymbol{I}_t^{Event} + \boldsymbol{\gamma^c} \boldsymbol{I}_c^{CD} + \boldsymbol{\beta} \boldsymbol{I}_{is}^{Age} + v_{icst}.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

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 $t \in [0, 10]$. $\boldsymbol{I}_c^{CD}$ is a vector of indicator variables for each Census Division in Canada. The coefficients of interest are α_c^{Qc}, which are CD-specific policy effects. The

[34] The 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 level increase.

outcomes are employment, earnings, and childcare take-up. Standard errors are clustered at the Census Division level

Figure 11: 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).

Figure 11 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). The estimated program effect on childcare take-up pins down the combined share of complier families $\pi_C^{CD} = \pi_{PC}^{CD} + \pi_{GC}^{CD}$. Since only compliers and always-takers use formal childcare when subsidies are available, we use the measured post-policy childcare take-up rate to approximate $\pi_C^{CD} + \pi_A^{CD}$, 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

$$\pi_{GC}^{CD} = P\left[D_i(1) = c, D_i(0) = g \middle| CD\right] = E[G_i|S_i = 0, CD] - E[G_i|S_i = 1, CD]$$
$$\pi_{GN}^{CD} = P\left[D_i(1) = g, D_i(0) = g \middle| CD\right] = E[G_i|S_i = 1, CD],$$

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. Then, the pre-policy fraction of families where grandparents are the main caretakers, $E[G_i|S_i = 0, CD]$, is equal to $\pi_{GC}^{CD} + \pi_{GN}^{CD}$.

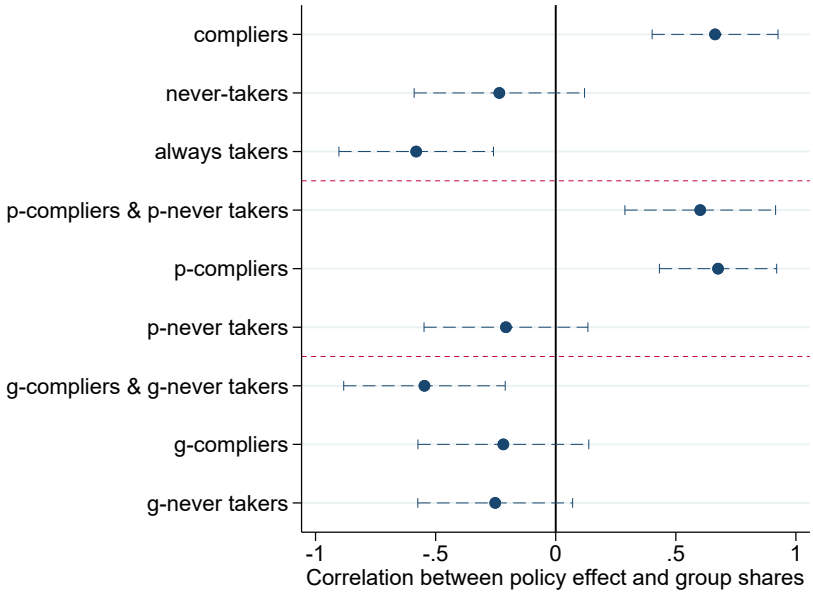
Unfortunately, the NLSCY does not record the Census Division of residence, and the sample size is relatively small, preventing us from estimating CD-specific shares of g-compliers and g-never takers with these data. Instead, we use a proxy for frequent grandparent-provided care from the Census. The long-form 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 (Statistics Canada, 2019a, 2020b). We extract these data for 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.[35]

In Figure 12, 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—measured as the pre-policy index of intensity of grandparent-provided unpaid care—strongly predicts the effect size with a correlation coefficient of -0.55.

Discussion. What do our results imply for other contexts? Overall, we find that subsidized childcare in Quebec attenuated the motherhood effect on earnings and employment, as well as the grandmotherhood effect on employment to a smaller extent. In comparison,

[35] Appendix F provides further details on the calculation of group shares using this proxy.

Figure 12: 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.

Andresen and Nix (2022) estimate that a childcare reform implemented in 2002 across Norwegian municipalities, which expanded availability for 1- and 2-year-olds, reduced the child penalty by 23% for each additional full year of early childcare use. To make our estimates more comparable to theirs, one would have to re-scale the reported reduced-form effect of the Quebec reform by the first-stage impact on childcare use, which is roughly equal to 20 percentage points (see Figure 6). For example, the 2.5% reduction in the motherhood effect on earnings we report in Figure 7 translates into a (2.5/0.2=)12.5% reduction in the motherhood effect per childcare spot use. In contrast, Kleven et al. (2022) find no impact on the motherhood effect from expansions of childcare provision for 1- to 5-year-olds across Austrian municipalities. They show that, on average, the take-up rate of nursery care increased by 20 percentage points in Austria (from 8% to 28%), a first-stage impact similar in size to that of the Quebec reform. Differences in the magnitude of the policy shocks can thus not explain the differences in findings.[36]

To reconcile these findings, Appendix Figure A3 shows that Austria has one of the highest

[36]Brewer et al. (2022) find small but positive effects of childcare provision on maternal employment in the UK. These results, however, may not be directly comparable to ours since we focus on the motherhood effect as our main outcome. We note that both Brewer et al. (2022) and Kleven et al. (2022) show that formal childcare crowds out informal childcare in their setting.

rates of informal childcare arrangements, while Quebec and Norway have some of the lowest rates. Conversely, Austrian women between 55 and 64 are significantly less likely to work than same-aged men compared to Quebec and Norway. These patterns are consistent with grandparent-provided care being an important substitute for formal childcare in countries where childcare has a limited impact on mothers. In fact, Kleven et al. (2022) show that in their context, a 1 percentage point increase in a childcare supply index is associated with a 0.36 percentage point increase in childcare take-up and an almost perfectly symmetric decrease of 0.35 percentage point in the share of children receiving care by relatives. In our view, this suggests the share of g-compliers is possibly higher in Austria than in Canada or Norway.

However, these conclusions come with some caveats. First, informal childcare includes care by friends, neighbours, and relatives other than grandparents. Observed differences in informal childcare could thus partly reflect differences in care provided by other relatives, friends, and neighbours. Second, gender norms differ across countries. 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.[37] Austrians are also more likely to believe children restrict employment and career chances than Norwegians and Quebecois. Still, the fraction of people who agrees that working mothers can have a warm relationship with their child is roughly the same everywhere.

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.

7 Conclusion

In this paper, we examine how children affect parents' and grandparents' earnings and employment trajectories and whether childcare subsidies can attenuate the impact of children

[37] Individuals in the rest of Canada are more likely to think that women with pre-school children should stay home, which was also true in 1994 before the childcare reform. Differences in norms across provinces are thus unlikely the result of the policy itself.

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 labour supply impacts.

The first innovation of our analysis is to estimate the impact of childcare subsidies on both mothers and grandmothers. Our findings indicate that a universal childcare policy helped attenuate the impact of children on mothers in Quebec, and that it also affected the impact of grandchildren on grandmothers in subtle ways. We find that Quebec's childcare program increased grandmothers' employment rate but negatively affected 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 second innovation is to use spatial variation in program effect sizes to analyze whether grandparent-provided care can modulate the impact of childcare subsidies on mothers. Existing evidence on the effect of childcare subsidies on the impact of children on women is mixed, ranging from no effect in Austria (Kleven et al., 2022) to a 23% reduction in earnings drop from motherhood in Norway (Andresen and Nix, 2022). Our results indicate that policy effects on mothers are lower when grandparents provide more informal childcare. That is, childcare policies can affect child penalties, but to what extent they do depends on the availability and substitutability of alternative modes of care.

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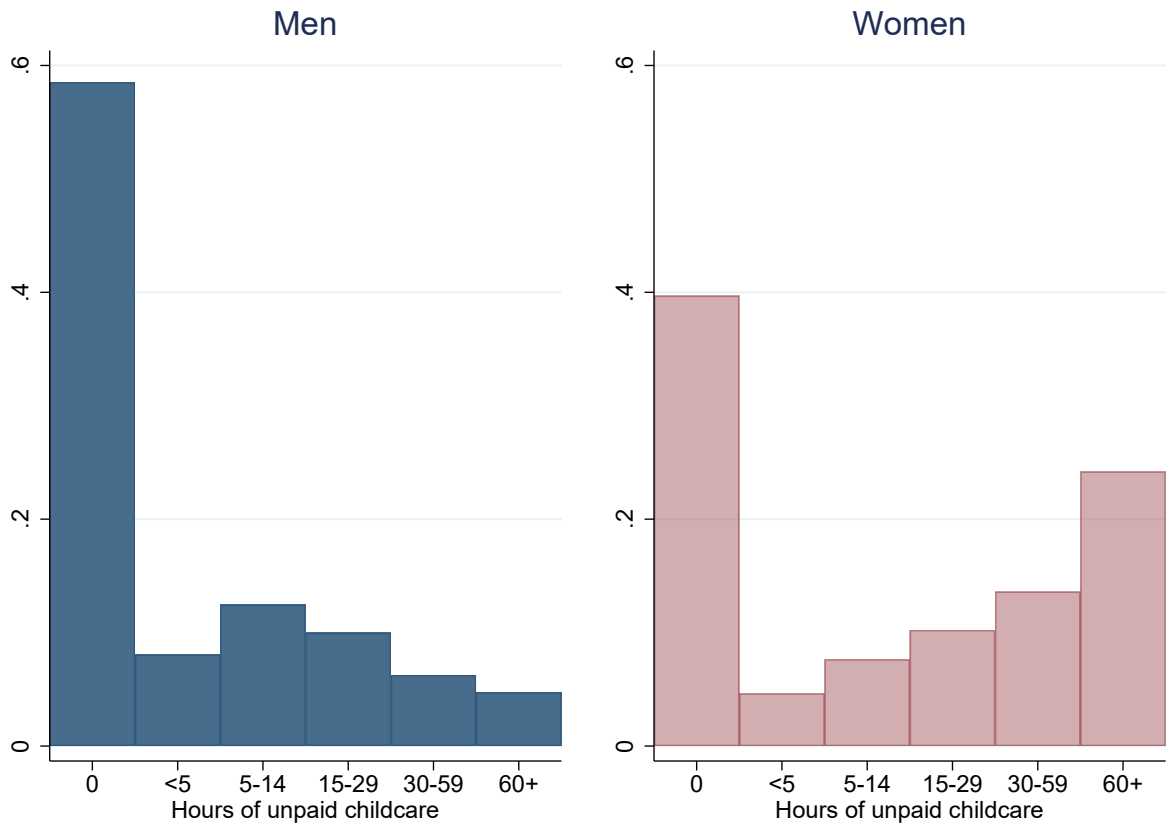
__ , "Table 14-10-0060-01 Retirement age by class of worker, annual [data table]," 2022.
__ , "Table 14-10-0327-01 Labour force characteristics by sex and detailed age group, annual [data table]," 2022.
Staubli, Stefan and Qiongda Zhao, "The long-run impact of early retirement programs: Evidence from Canadian pension reforms," 2021.
Zamarro, Gema, "Family labor participation and child care decisions: The role of grannies," *SERIEs*, 2020, pp. 1–26.
Zhao, Qiongda, "Design Matters: Causal Evidence on Cash Benefits and Fertility," 2021.

Appendix for Online Publication

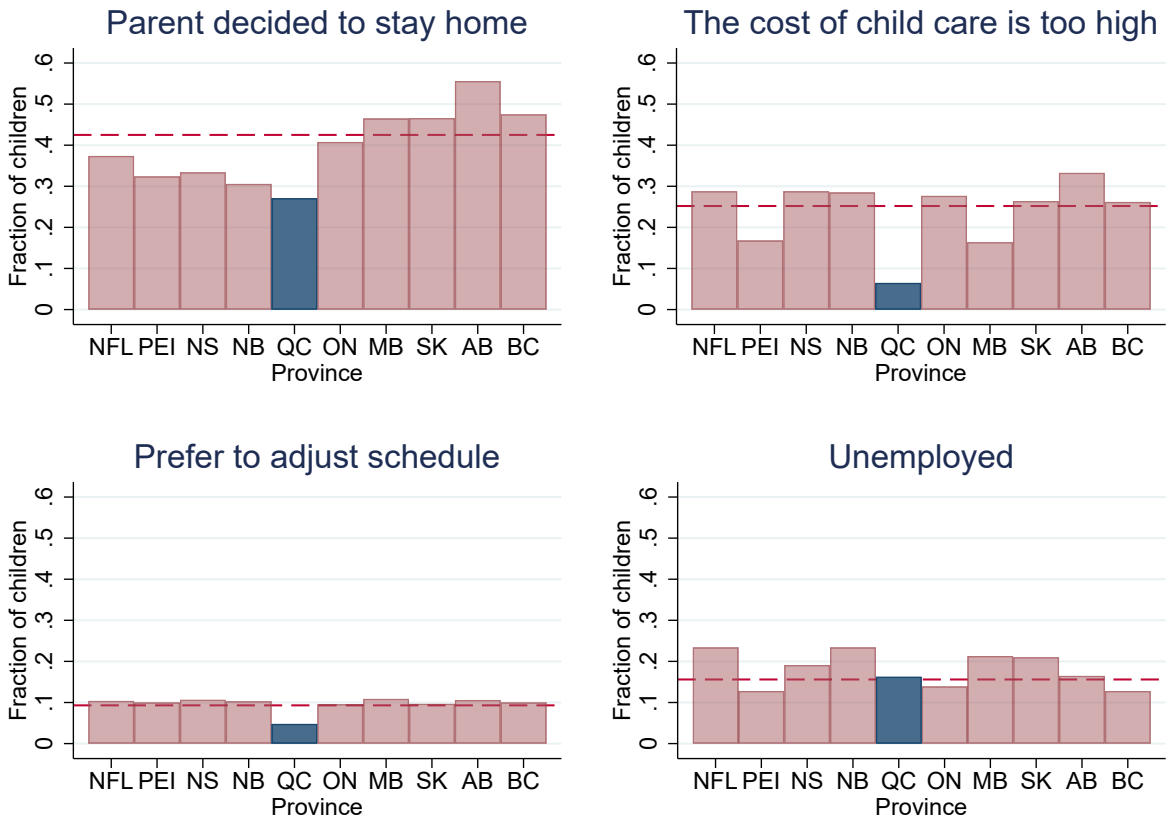
A Institutional Background: Additional Evidence

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



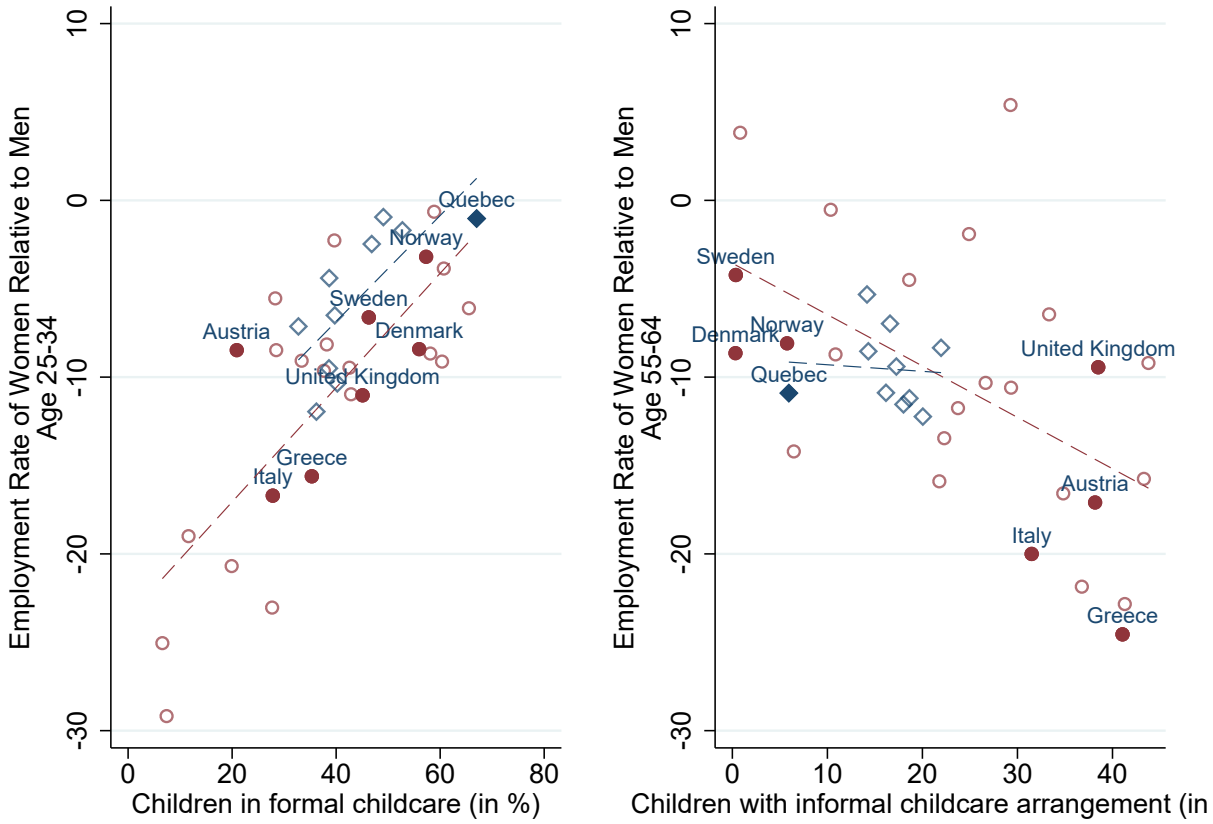
Notes: Data are for the year 1996, and are drawn from Statistics Canada (2019a).

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



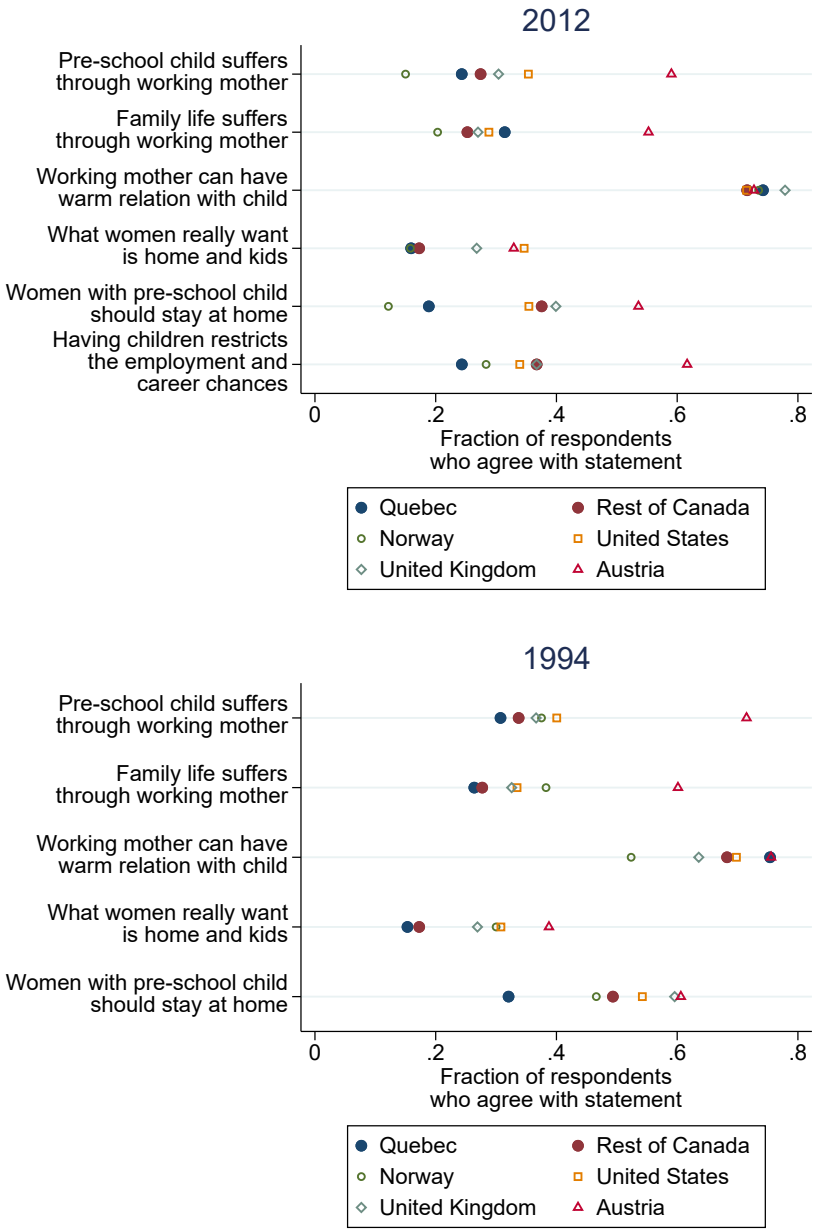
Notes: All statistics are based on data from the Survey on Early Learning and Child Care Arrangements (Statistics Canada, 2022a) for the year 2019. The horizontal dashed red lines indicate the corresponding fraction of children for all of Canada. Survey respondents could select multiple options. The 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 are 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 the year 2019 and taken from the OECD Family Database (2023c). Formal and informal care use is based on Tables PF3.2 and PF3.3. 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. Employment rates are obtained from the OECD's Labour Force Statistics (2023b). For Canadian provinces, statistics are for the year 2019 and extracted from Statistics Canada (2022a,c). Formal care combines daycare centres and family child care homes, whereas informal care is measured by the fraction receiving care from 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 shows 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 (2022). 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.gesis.org/en/issp/home.

B Additional Results on the Impact of Children

B.1 Robustness

Robustness of Estimates of the Impact of Children on Parents. Figure 2 uses the IID to document the impact of the arrival of the first child on several outcomes. Since the IID is not a representative sample of the population, Appendix Figure A5 replicates the same results using the LAD. The patterns and magnitudes are very similar to those in the IID.

Robustness of Estimates of the Impact of Grandchildren on Grandparents. As Kleven et al. (2019a) discuss, the event study estimates on the earnings and employment impacts of (grand)children will be biased if we do not fully control for labour supply determinants that do not depend on (grand)children (e.g., age, ability, and preferences). Capturing the non-grandchild-related labour supply determinants is particularly challenging as the impacts of grandchildren on grandparents are more gradual and grow over time. Including age and year dummies, as in our main specification, goes a long way to alleviate this concern, but we cannot be sure to have purged all bias. We consider two robustness checks to assess whether bias remains.

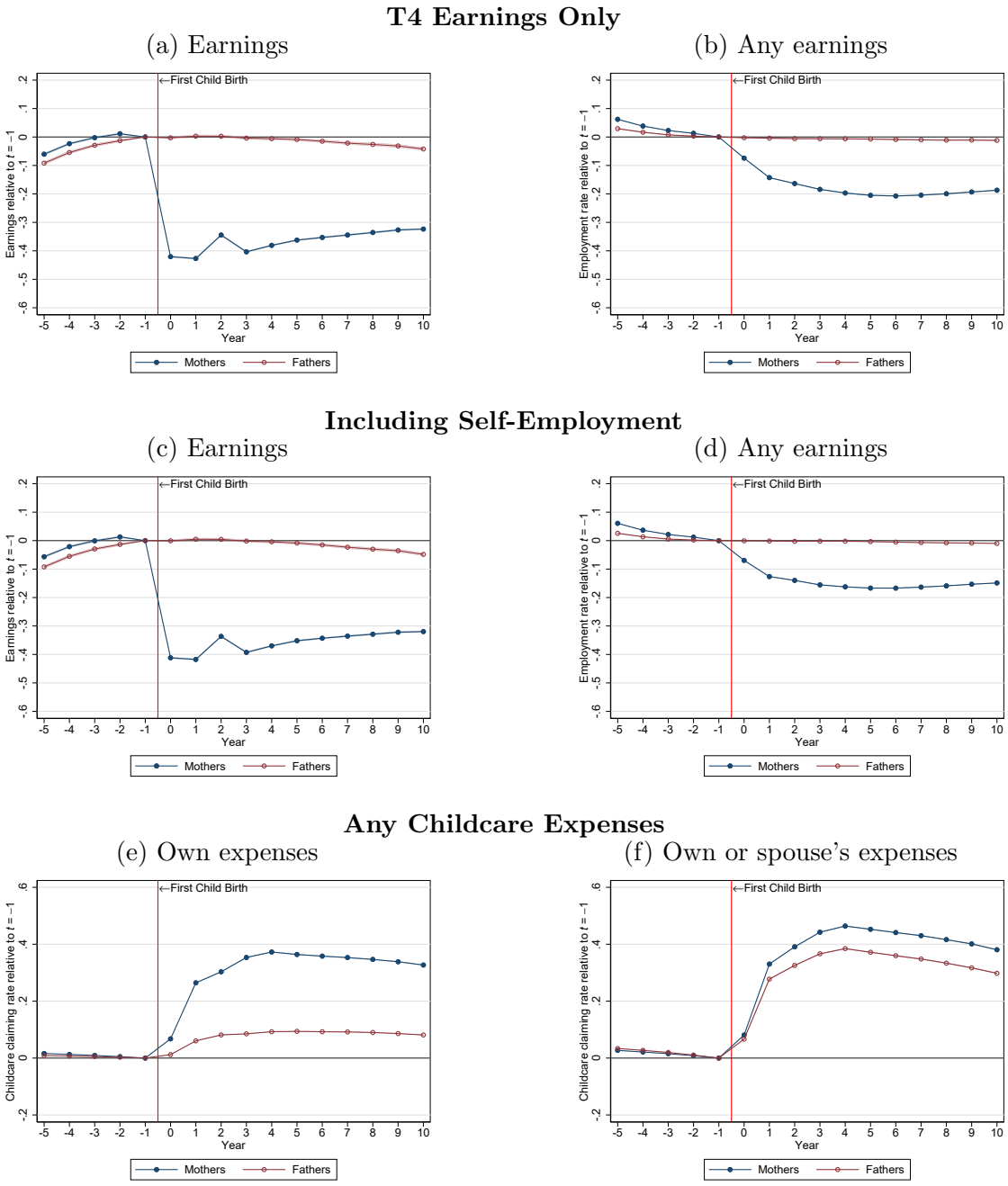
The first check is to control more flexibly for life-cycle and time trends by interacting the age and year dummies with province dummies. These interaction terms capture differences in life-cycle or time trends across provinces, such as differences in economic conditions or life-cycle earnings profiles. A second check, proposed by Kleven et al. (2019a) as a potential solution to remove any remaining bias, is to use individuals who never have grandchildren as a control group in a difference-in-differences design. The idea is to assign placebo births to individuals who never have grandchildren, drawing from the observed distribution of age at first grandchild among grandparents. Following Kleven et al. (2019a), we approximate the distribution of age at first grandchild 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

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

where T_i is an indicator for grandparents.

Figure A6 shows the event studies for both robustness checks. For comparability, we also

Figure A5: Estimates using LAD



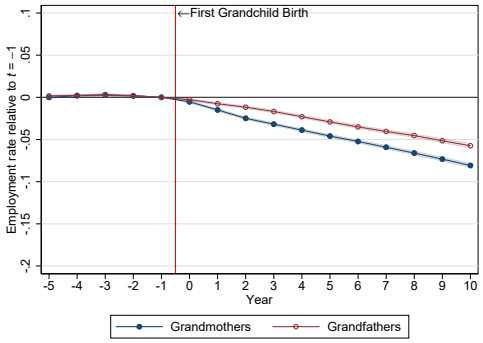
Notes: This figure reports estimates of child penalties based on equation (1), estimated on a sample of new parents in the Longitudinal Administrative Database (LAD). In panels (a) and (b), earnings and employment is based on T4 earnings alone. In panels (c) and (d), earnings also include self-employment 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 (they are barely visible because point estimates are extremely precise).

reproduce the main estimates in panels (a) and (b). Both the event study estimates with more flexible age and year trends (panels c and d) and the difference-in-differences event study estimates (panels e and f) resemble our baseline results very closely.

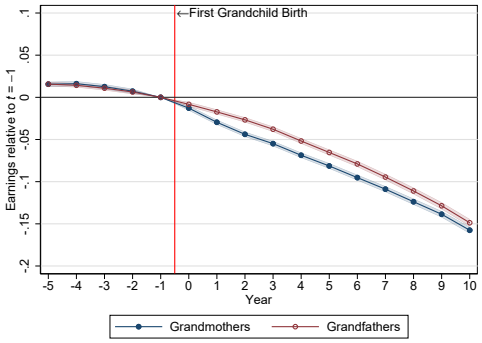
Figure A6: Robustness of Grandchild Penalties

Main Estimates

(a) Employment

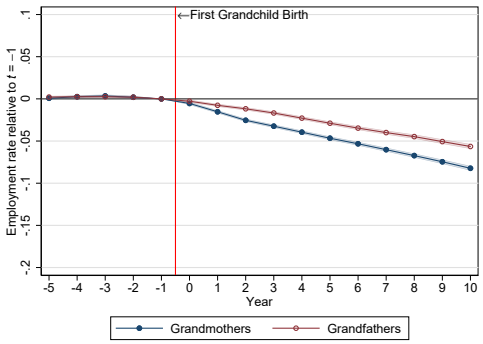


(b) Earnings

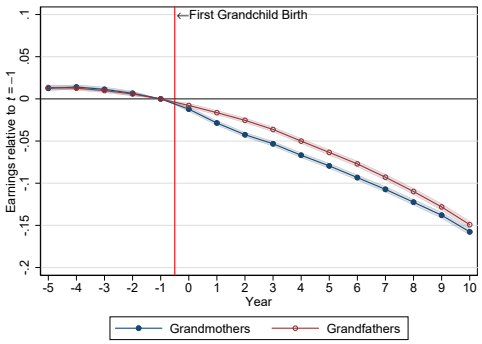


Province-Age-Year Interaction Terms

(c) Employment

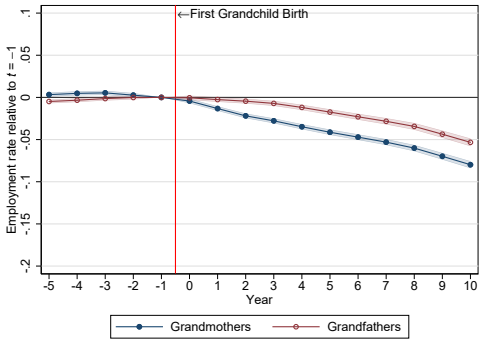


(d) Earnings

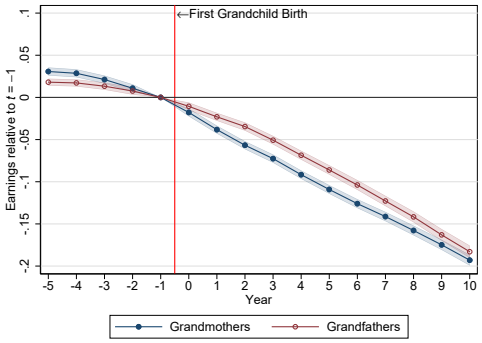


Placebo Difference-in-Differences

(e) Employment



(f) Earnings



Notes: This figure reports estimates of grandchild penalties based on equation (1) (panels a and b), estimates of grandchild penalties when interacting age and year dummies with province dummies (panels c and d), and estimates of grandchild penalties from a difference-in-differences event study design that compares individuals who have grandchildren to those who never have grandchildren (panels e and f). Models are estimated separately by gender. Shaded areas show 95% confidence intervals (they are barely visible because point estimates are extremely precise).

B.2 Heterogeneity

Heterogeneous Effects of Children on Parents. Figure A7 presents the impact of the arrival of the first child separately for mothers who do and do not live in the same CD as their parents at event time 0.[38] These two groups 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 control for these differences, we follow Kleven et al. (2022) and apply an Inverse Probability Weighting correction, but unweighted results are qualitatively similar. 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 figure shows that motherhood effects are 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. The patterns are similar when we do not scale the event time coefficients by the baseline earnings absent children. But since mothers not in the same CD have higher baseline earnings than those in the same CD, the unscaled earnings gaps are larger.[39] 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.

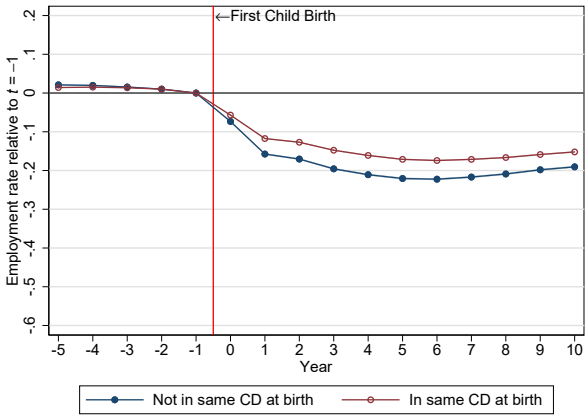
Appendix Figure A8 examines heterogeneity by whether a mother lives in an urban or rural area and by the number of children a mother ends up having. To account for differences in pre-birth characteristics across subsamples, we perform the same inverse-probability-weighting approach as in Figure A7. We 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

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

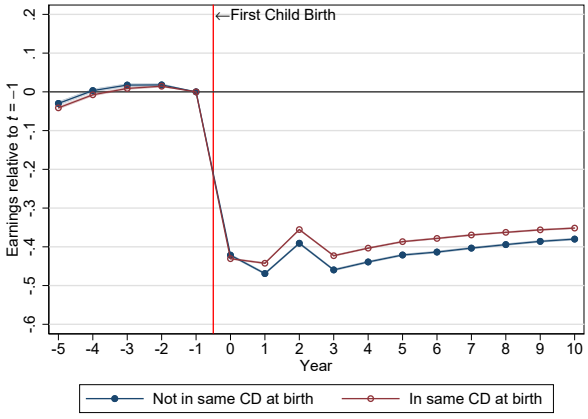
[39] Without scaling, mothers not in the same CD earn \$21,107 less at event time $t = 10$, while mothers not in the same CD earn only \$17,882 less. This \$3,300 gap in earnings is equivalent to a percent drop of 18.5% when measured relative to the earnings drop for mothers in the same CD.

Figure A7: Heterogeneous Impacts of Children on Mothers by Proximity to Grandparents at Childbirth

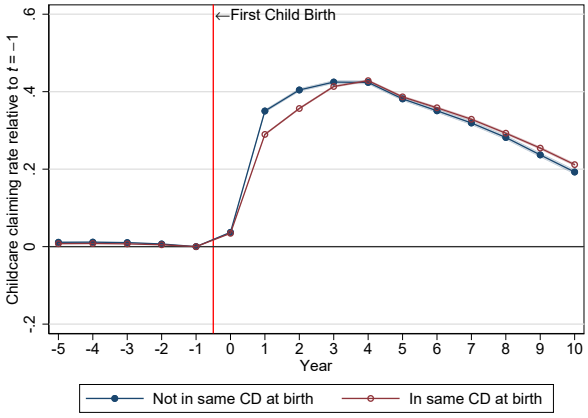
(a) Employment



(b) Earnings



(c) Any childcare expenses



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 (they are barely visible because point estimates are extremely precise).

the first child enters preschool (event time 4) or kindergarten (event time 5).

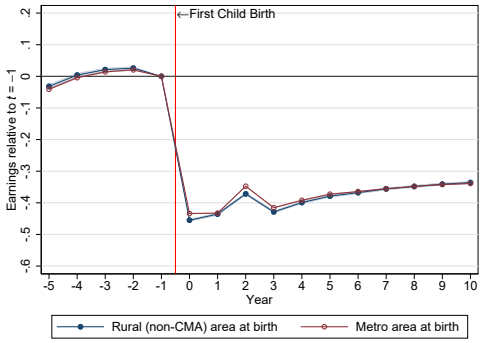
The analysis across mothers with 1, 3, or 3 or more children reveals a significant motherhood effect in all three family types. Earnings and employment rates drop by 41-45% and 11-14% at event time 0. Earnings start to diverge sharply after event time 1 as more children arrive in larger families. The 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 having 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.

Heterogeneous Effects of Grandchildren on Grandparents. As shown in Appendix Figure A11, 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. Moreover, grandmothers not residing in the same CD experience slightly larger employment and earnings penalties, consistent with the impact of longer commuting times or grandmothers moving closer to their children.

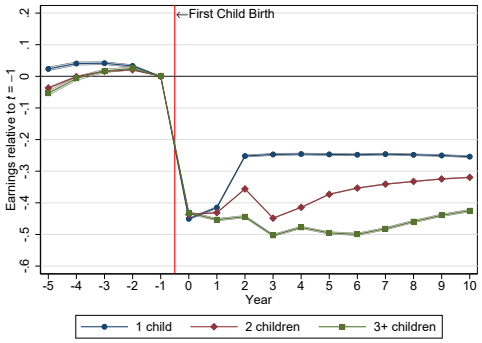
For most grandchildren in our sample, we can observe maternal and paternal grandparents as both parents belong to the IID child generation. Kleven et al. (2019a) find that female child penalties strongly correlate with the labor supply history of maternal grandparents but not paternal grandparents. Given this asymmetry, we might expect that grandchild penalties differ for maternal and paternal grandparents. Indeed, Appendix Figure A9 shows 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. In Appendix Figure A10, 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.

Figure A8: Heterogeneity in Child Penalties for Mothers

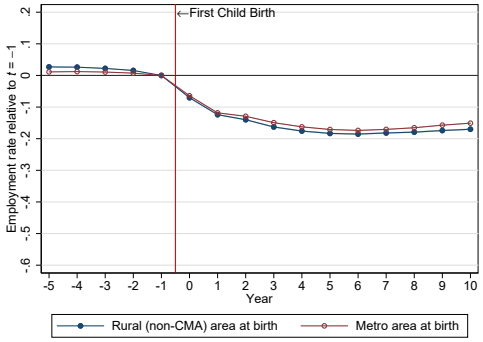
(a) Earnings, by Urban Status



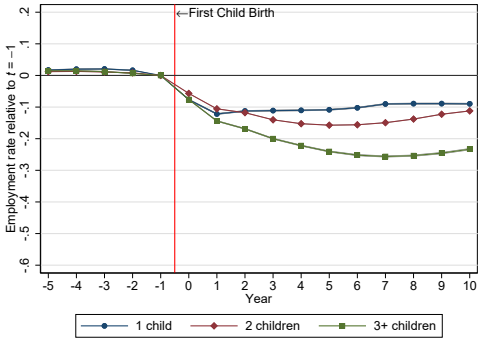
(b) Earnings, by # of kids



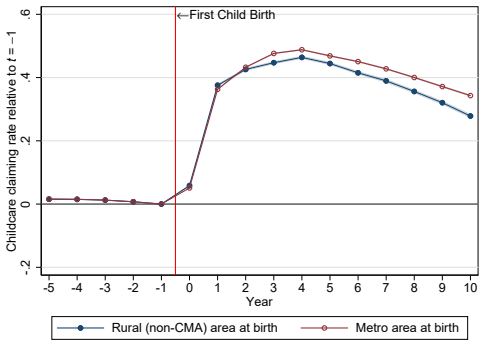
(c) Employment, by Urban Status



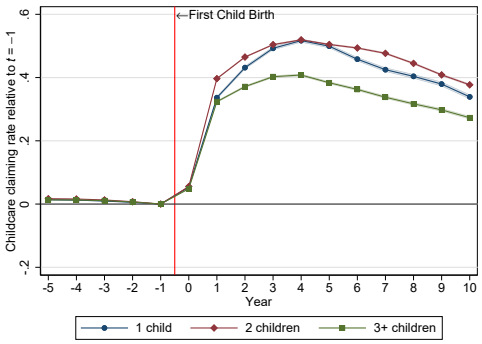
(d) Employment, by # of kids



(e) Any childcare expenses, by Urban Status



(f) Any childcare expenses, by # of kids

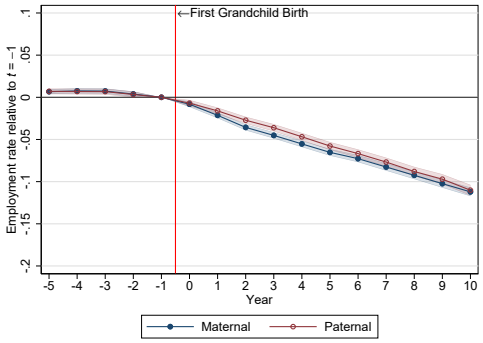


Notes: This figure reports estimates of the child penalty for mothers, separately for those who live in an urban (CMA) or a rural area (panels (a), (c) and (e)), and for those who have either 1, 2 or 3+ children (panels (b), (d) and (f)). The sample consists of new mothers in the Intergenerational Income 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 (they are barely visible because point estimates are extremely precise).

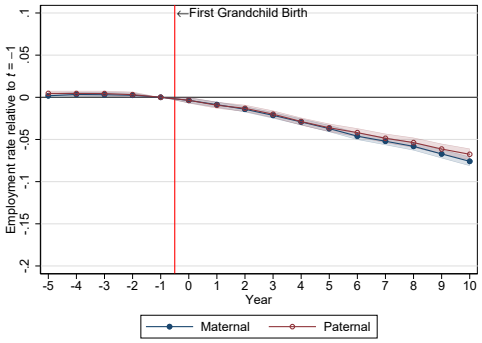
Figure A9: Comparing Maternal and Paternal Grandchild Penalties

Employment

(a) Grandmothers

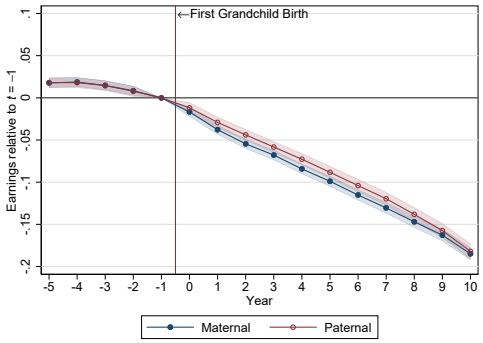


(b) Grandfathers

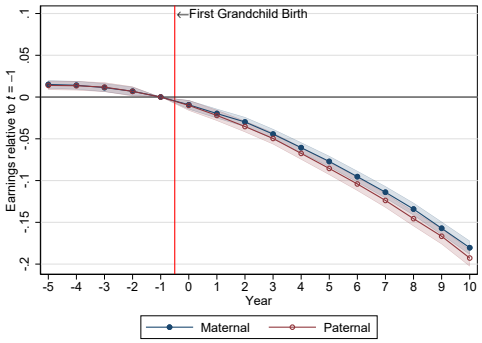


Earnings

(c) Grandmothers



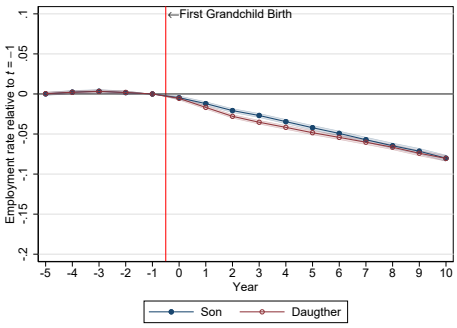
(d) Grandfathers



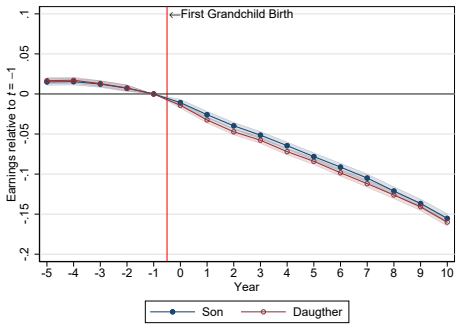
Notes: This figure reports estimates of the grandchild employment and earnings penalty separately for maternal and paternal grandmothers (panels (a) and (c)) and maternal and paternal grandfathers (panels (b) and (d)). Shaded areas show 95% confidence intervals.

Figure A10: Heterogeneity in Grandchild Penalties for Grandmothers

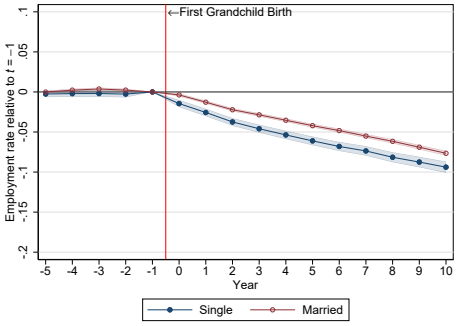
(a) Employment, by gender of child



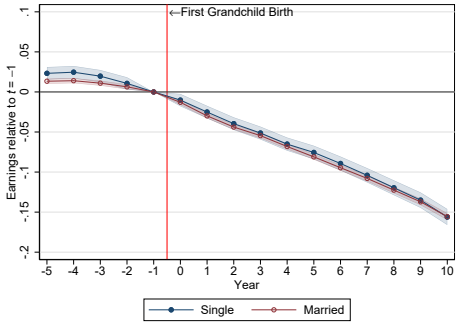
(b) Earnings, by gender of child



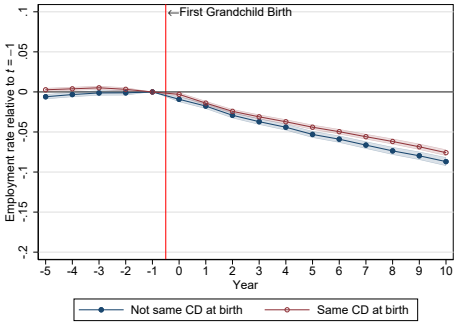
(c) Employment, by marital status



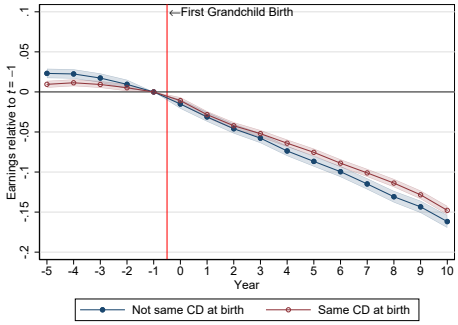
(d) Earnings, by marital status



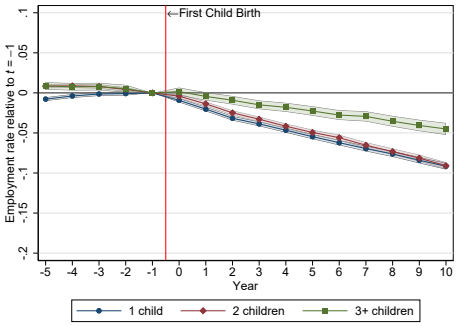
(e) Employment, by proximity to parents



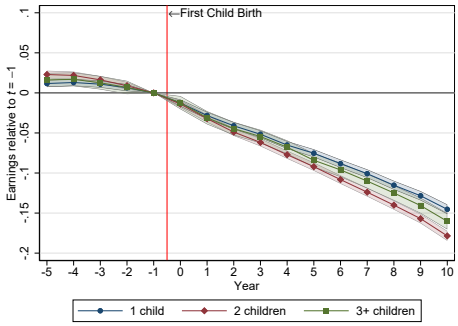
(e) Earnings, by proximity to parents



(g) Employment, by # of kids



(h) Earnings, by # of kids

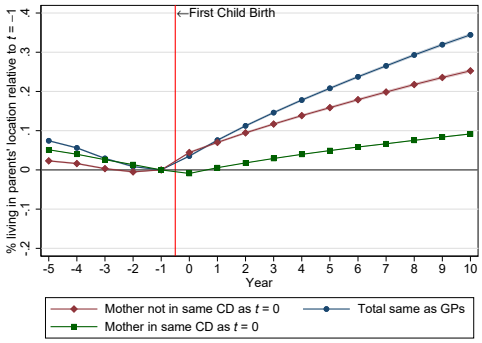


Notes: This figure reports estimates of the grandchild penalty for grandmothers, separately for those who have a daughter or a son (panels (a) and (b)), for singles and married (panels (c) and (d)), for those who do and do not live in the same CD as their child (panels (c) and (d)), and for those who 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.

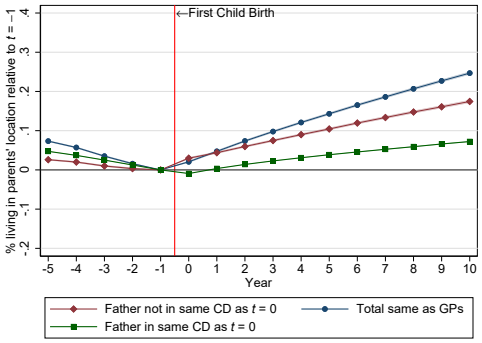
B.3 Additional Outcomes

Figure A11: Who Is Moving?

(a) Mother (not) in same CD



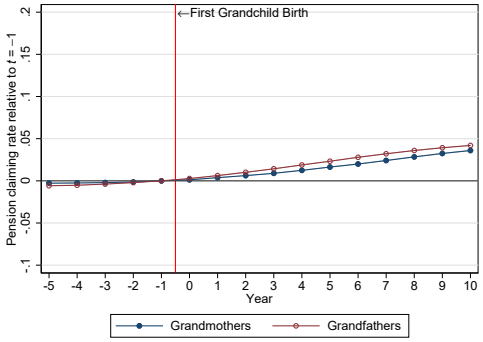
(b) Father (not) in same CD



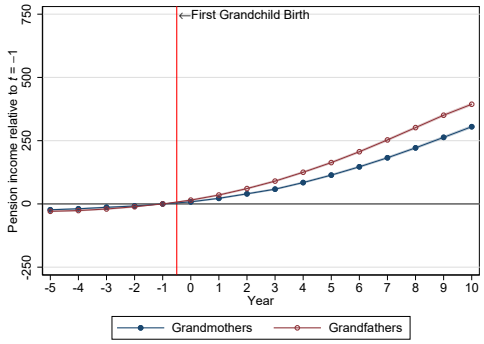
Notes: This figure reports estimates of whether an individual resides in (i) the same Census Division (CD) as one's own parent (blue circles), (ii) the same CD as one's own parent and the same CD as at $t = 0$ (maroon diamonds), and (iii) the same CD as one's own parent and not the same CD as at $t =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

Figure A12: Grandparents' Public Pension Take-up

(a) Claiming of Public Pension



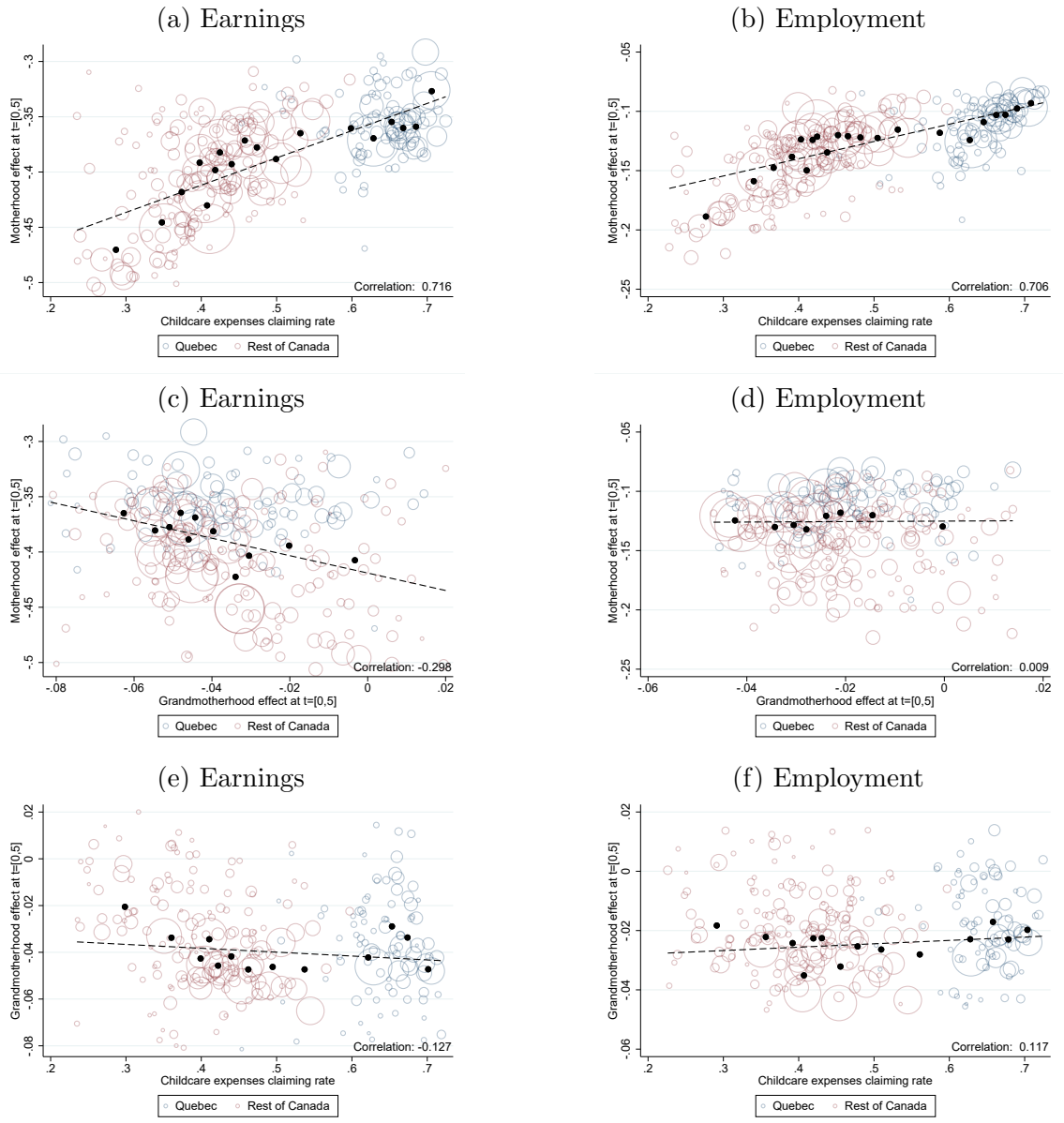
(b) Public Pension Income



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 (they are barely visible because point estimates are extremely precise).

B.4 Spatial Heterogeneity

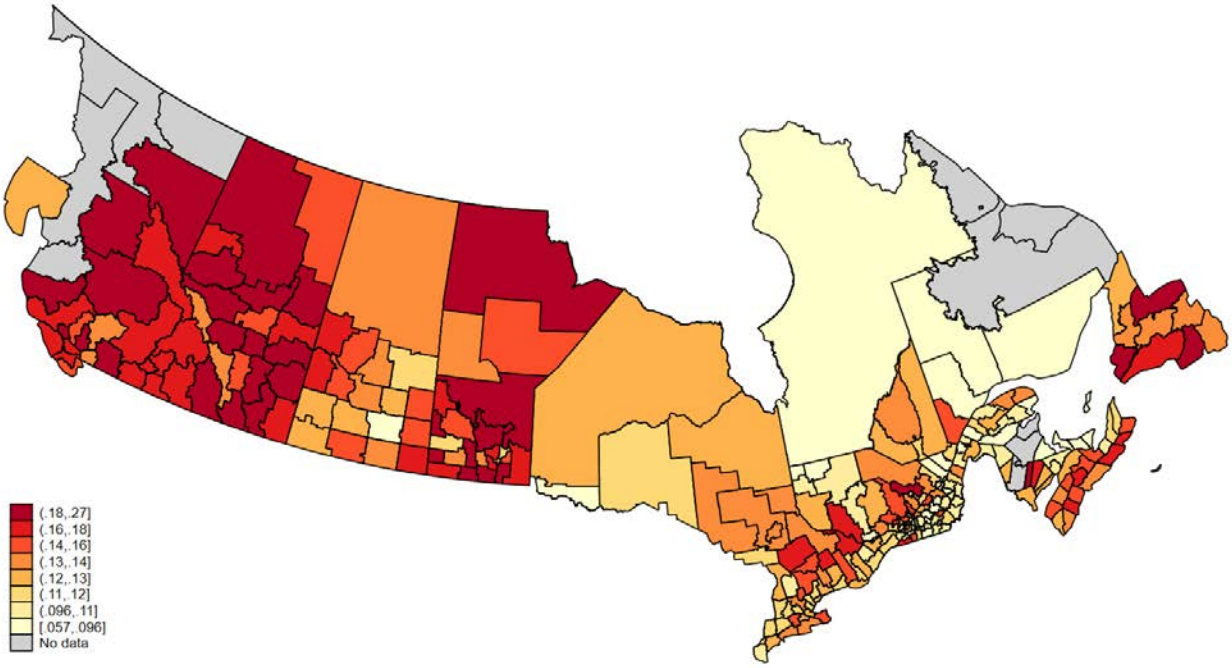
Figure A13: Child Penalties Across Census Divisions, including Quebec



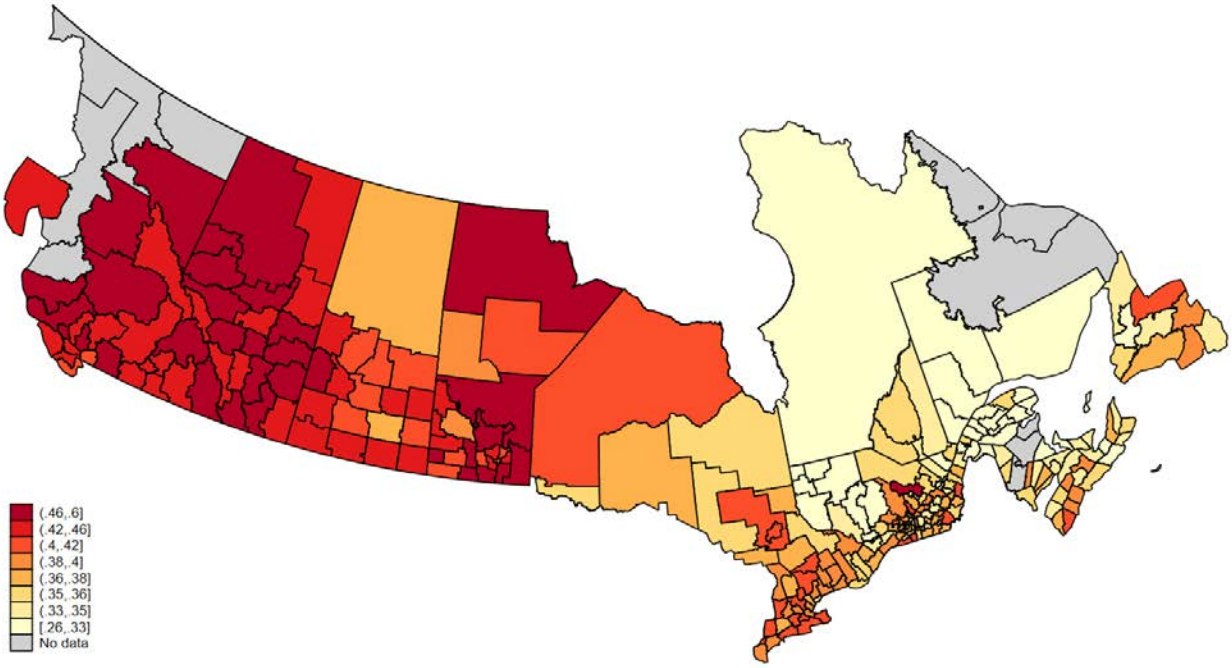
Notes: This figure presents estimates of (grand)motherhood effects on employment and earnings for all Canadian Census Divisions, including those in Quebec. Each red/blue circle represents one Census Division. The size of the circle is proportional to the number of observations. Black dots represent a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

Figure A14: The Geography of Motherhood effects

(a) Employment



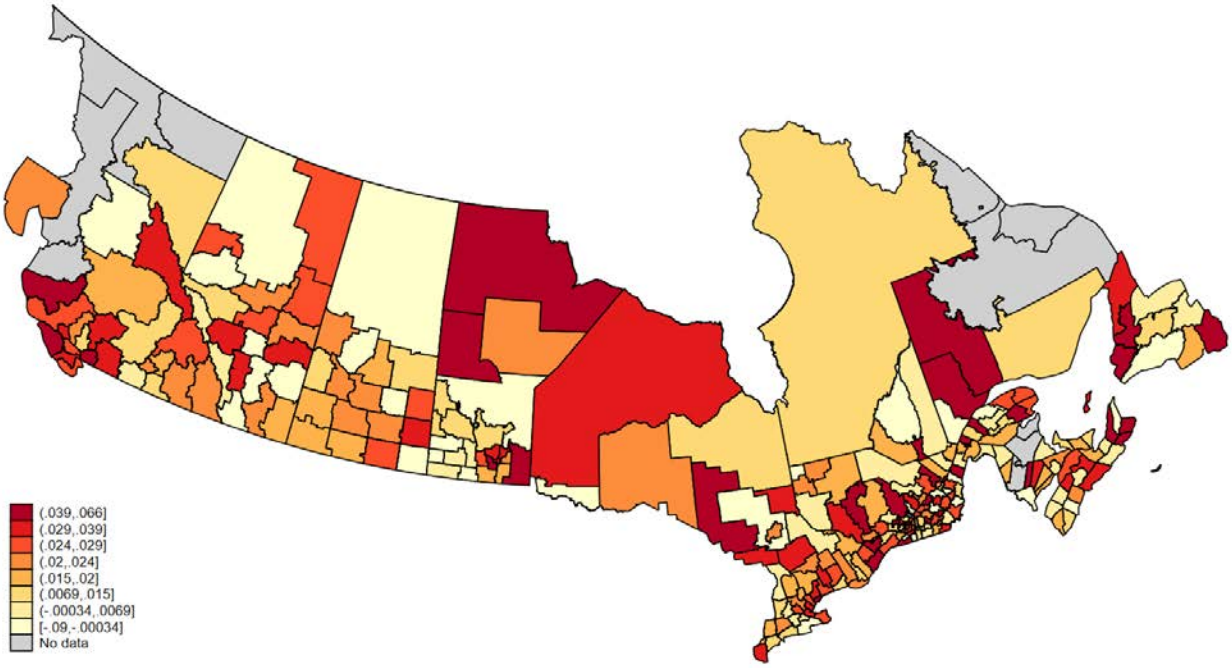
(b) Earnings



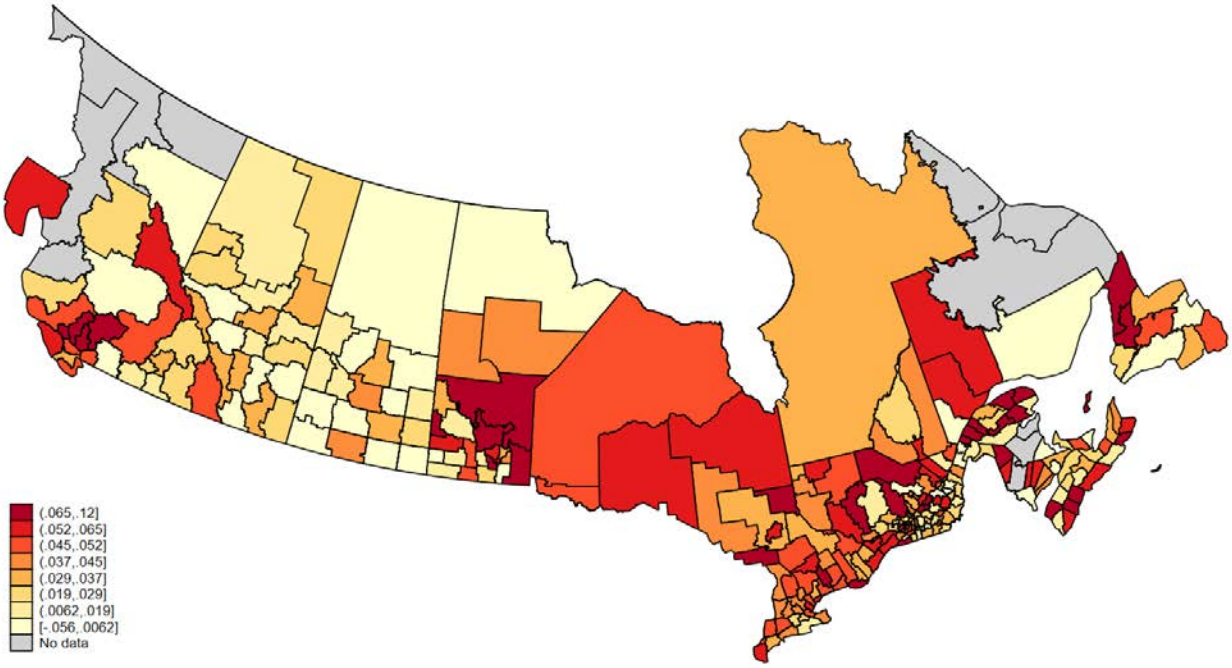
Notes: Census Divisions with too few observations are labelled as "No data".

Figure A15: The Geography of Grandmotherhood effects

(a) Employment

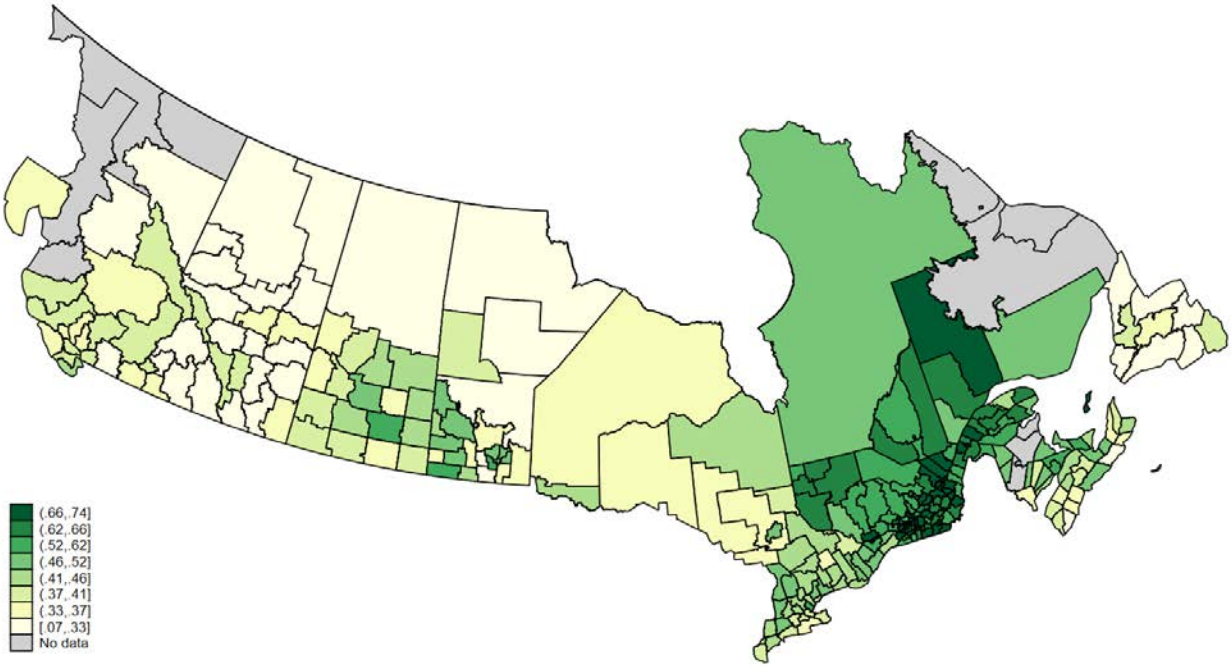


(b) Earnings



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

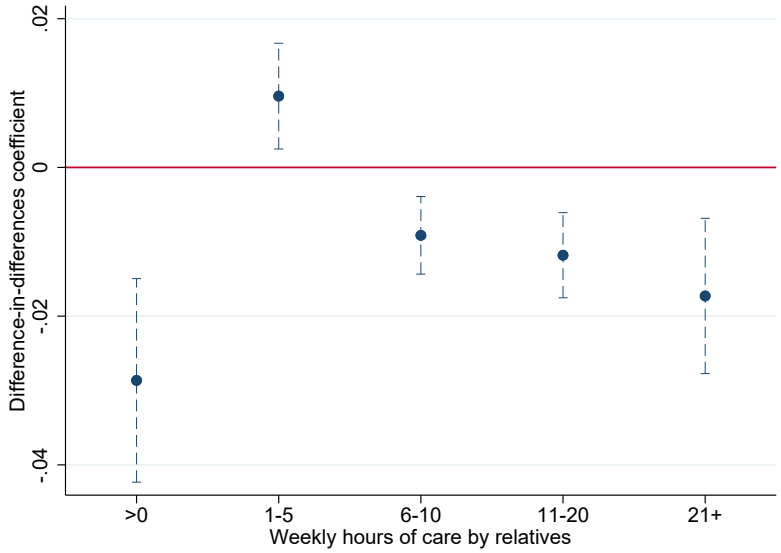
Figure A16: The Geography of Childcare Take-up



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

C Additional Results on the Impact of Childcare Subsidies

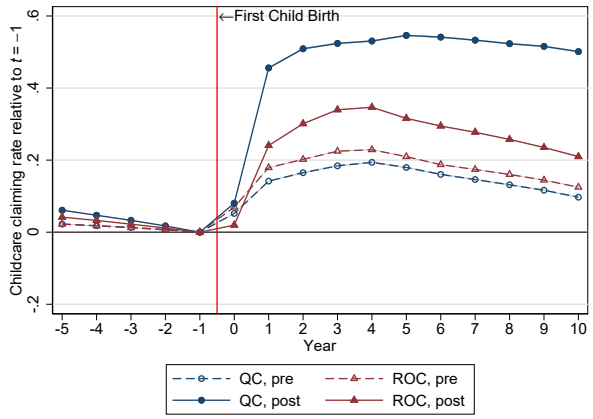
Figure A17: Program Effect on Hours of Care by Relatives



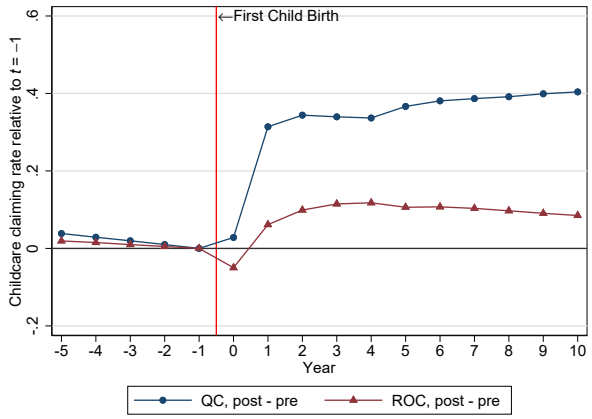
Notes: This figure reports difference-in-differences coefficients of the impact of childcare subsidies on care provided by relatives. There are 5 separate regressions for 5 different outcomes. The outcomes, respectively, are dummies for receiving either more than 0, 1-5, 6-10, 11-20, or 21+ weekly hours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

Figure A18: Impact of Children on Mothers, Quebec vs Rest of Canada, Pre- vs Post-1997

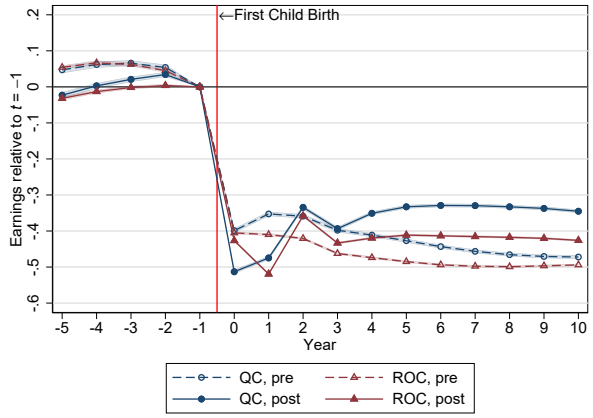
(a) Any childcare expenses



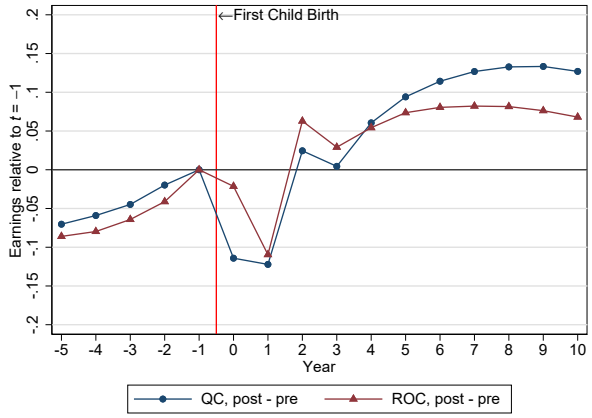
(b) Any childcare expenses, difference



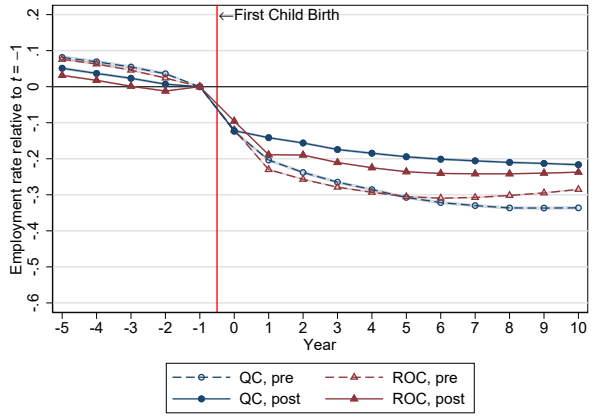
(c) Earnings



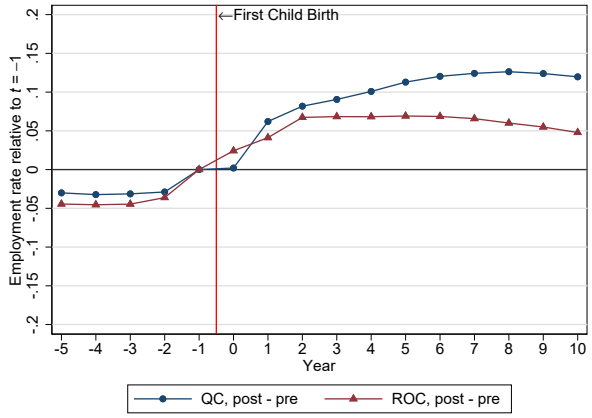
(d) Earnings, difference



(e) Employment

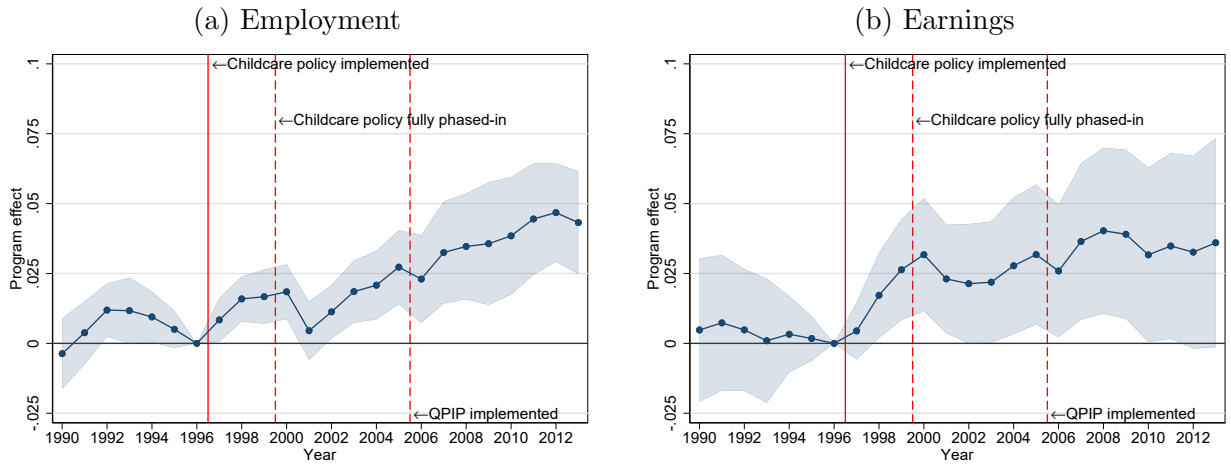


(f) Employment, difference



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 (they are barely visible because point estimates are extremely precise).

Figure A19: Effect of Quebec's Childcare Policy on the Montherhood Effect, Time-Invariant Place of Residence

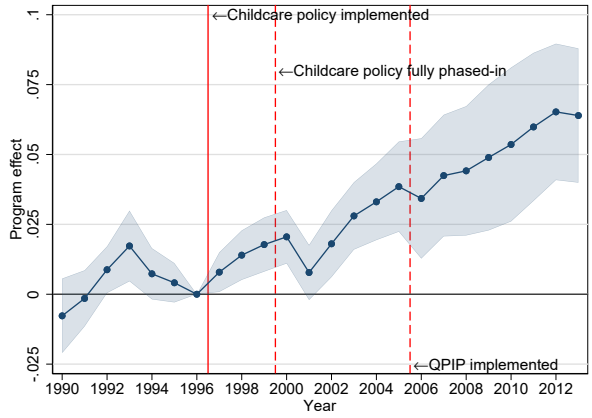


Notes: This figure reports the estimated policy effect of Quebec's childcare program on mothers' earnings and employment. All estimates are based on samples of mothers (including yet-to-be mothers) in the Longitudinal Administrative Database (LAD). All regression coefficients are scaled by predicted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

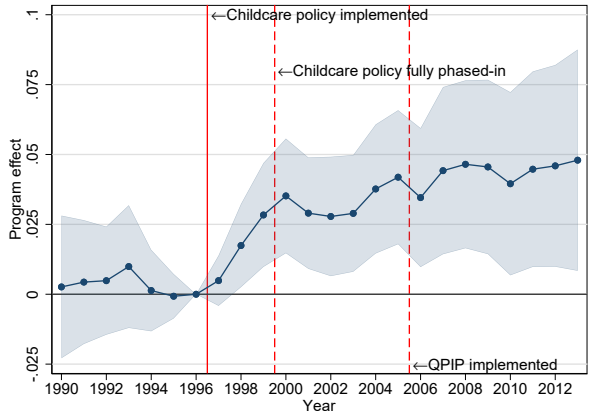
Figure A20: Effect of Quebec's Childcare Policy on the Motherhood Effect, Including Self-Employment

Time-Varying Place of Residence

(a) Employment

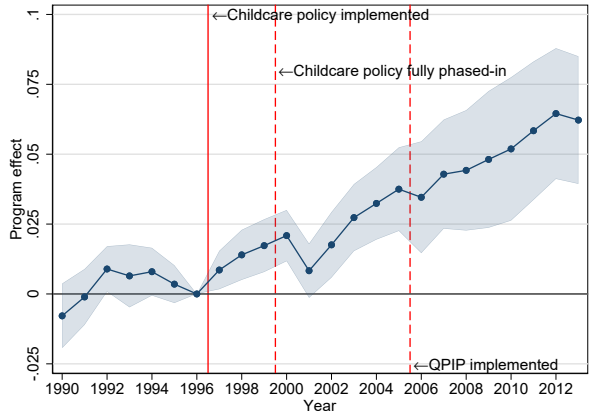


(b) Earnings

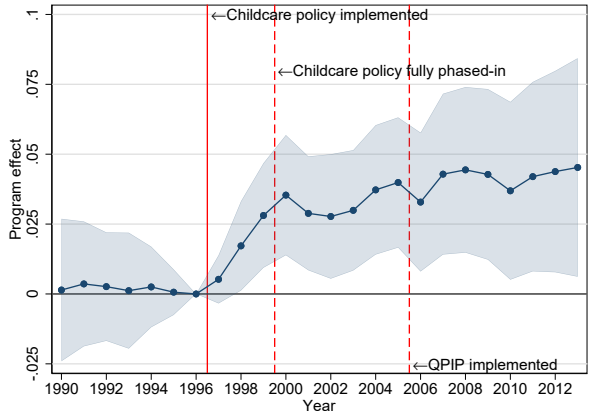


Time-Invariant Place of Residence

(c) Employment



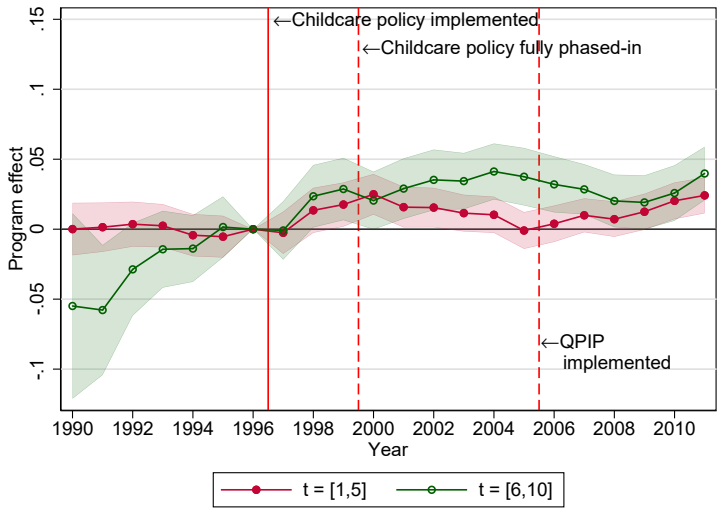
(d) Earnings



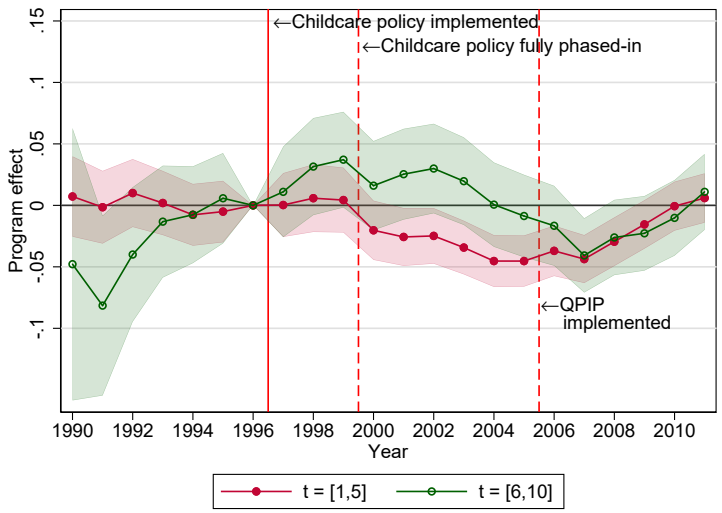
Notes: This figure reports the estimated policy effect of Quebec's childcare program on mothers' earnings and employment. All estimates are based on samples of mothers (including yet-to-be mothers) in the Longitudinal Administrative Database (LAD). All regression coefficients are scaled by predicted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

Figure A21: Effect of Quebec's Childcare Policy on the Grandmotherhood Effect

(a) Employment



(b) 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.

D Data Appendix

The first two parts of this appendix section provide 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.

D.1 Intergenerational Income Database (IID)

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 cover years 1978 to 2016 for panel A, and years 1981 to 2016 for panel B.[40] 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 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.

Individual-level Variables. *Parenthood and grandparenthood:* The tax data contain 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 individual from the IID's child generation to their spouse, and combine the dependent data of both spouses. In addition, we assign dependents registered

[40] Statistics 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.

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 misassignment 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 with 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 ensure 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 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 adjusting 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 contain 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. The reason is that 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.

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 we use childcare expenses as an outcome variable in the IID. (The LAD is not subject to this problem)

D.2 Longitudinal Administrative Databank (LAD)

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 the years 1982 to 2018. Even though LAD's sample size is substantially smaller compared to 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 analysis 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 before the non-filing year for demographics. We fill in zeros for earnings and income measures if the individual did not file taxes.

Individual-level Variables. *Parenthood:* LAD contains the age of a tax filer's children up to 7, 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 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 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, including 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.

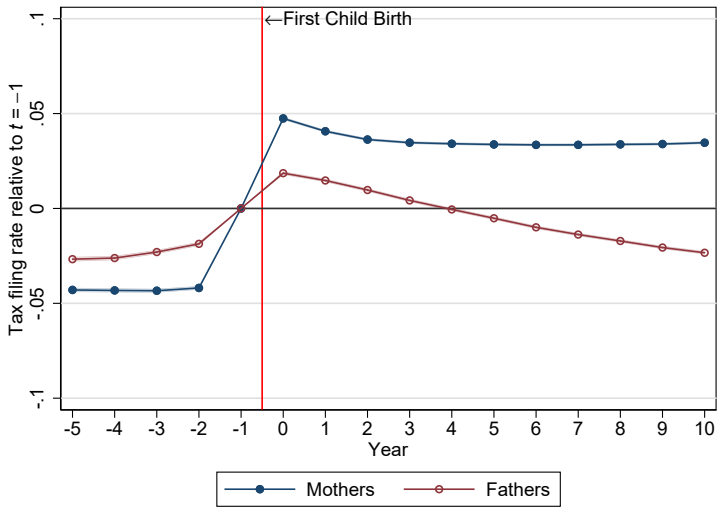
Household-level Variables. *Childcare expenses:* we use data on household deductions for childcare expenses from LAD. We code an individual as using childcare if their household has positive childcare expense deductions in a given year.

D.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 concerns.

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 A22 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. The figure indicates that sample selection is 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 A22: Impact of Children on Tax Filing



Notes: This figure reports estimates of tax filing based on equation (1), estimated on all individuals aged 20-54 who ever file taxes in the LAD. The outcome is an indicator for filing taxes in a given year. Shaded areas show 95% confidence intervals (they are barely visible because point estimates are extremely precise).

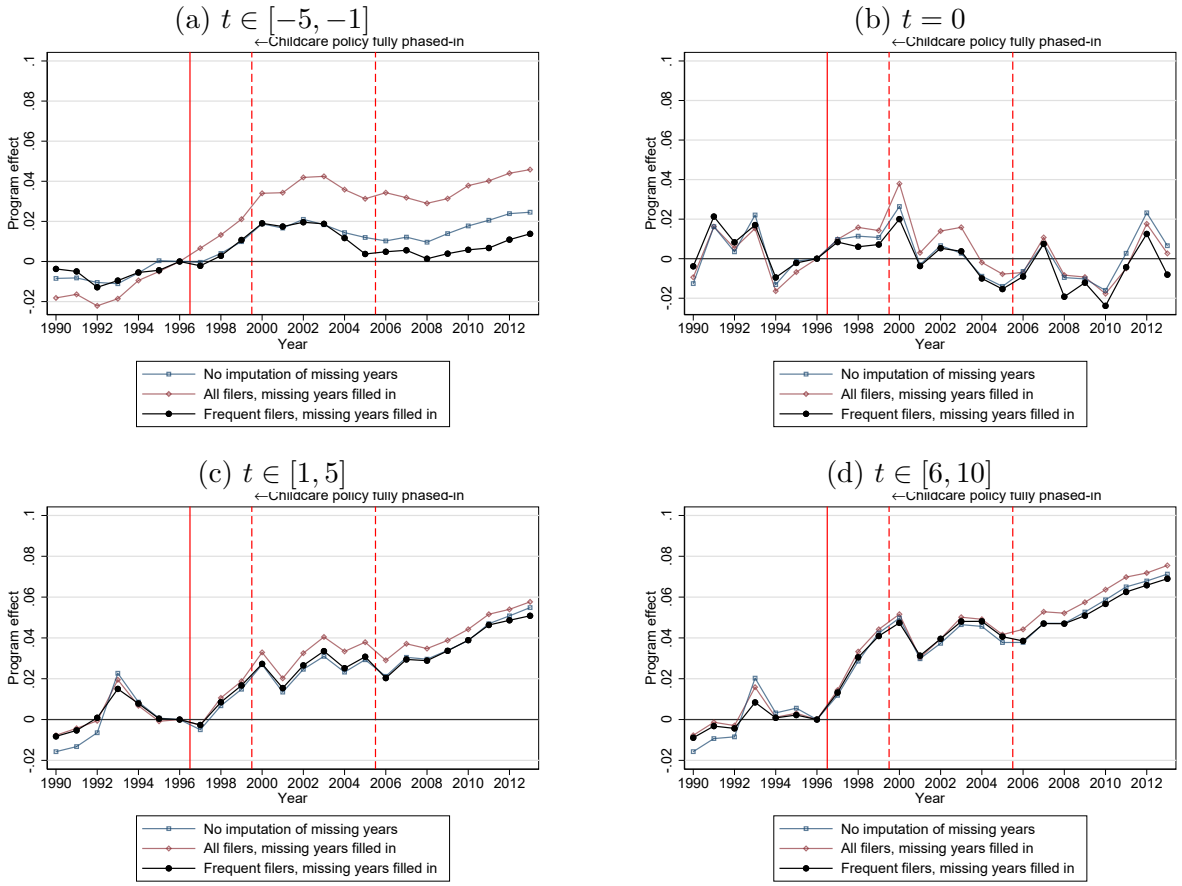
Second, the federal government introduced a new Canada Child Tax Benefit in 1993, which replaced three other child tax credits. Quebec, however, maintained the administration of its family allowance program. This policy change reinforced incentives for parents to file tax returns, but more so in the 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 A23 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 keeps 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 before 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 A23: 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).

D.4 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 for 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 6 are based on the following estimating equation:

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

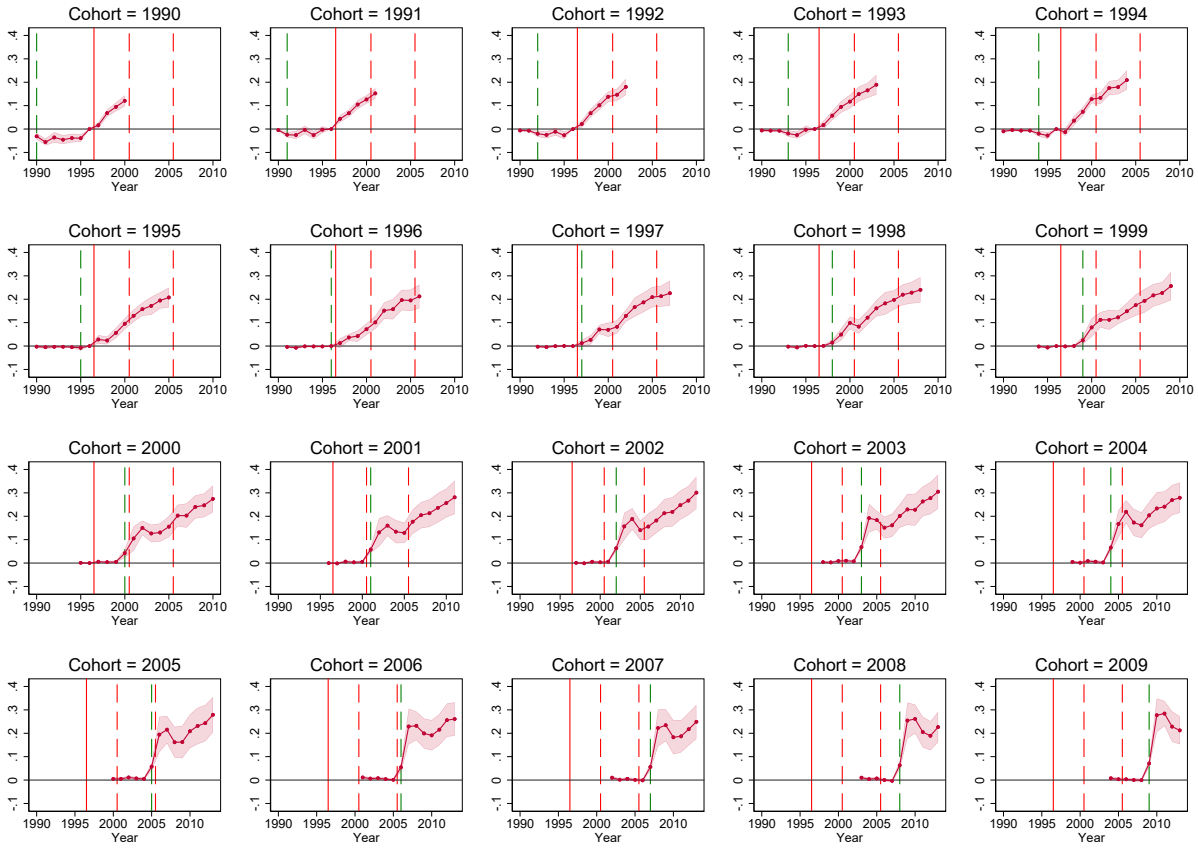
where Y_{ips} is an outcome variable indicating the use of different modes of care for child i residing in province p in calendar year s. The model includes year fixed effects and Census Metropolitan Areas (CMA) fixed effects, as well as a vector of control variables, which includes gender and age. Standard errors are clustered at the CMA-level. The difference-in-differences coefficients reported in the figure are obtained by replacing the interaction terms $\boldsymbol{I}_s^{Year} \times QC_i$ with $Post_s \times QC_i$, where $Post_s = 1\{Year \geq 2000\}$. Figure A17 is based on that same estimating equation.

E 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 A24, A25, and A26 for childcare expenses, earnings, and employment. The first-stage impact on the childcare expenses claiming rate is particularly salient. For instance, early 1990s birth cohorts, representing women who became mothers before the policy and are observed with children in both pre- and post-policy periods, evolve in parallel in Quebec and the Rest of Canada until 1997, when a sharp divergence emerges.

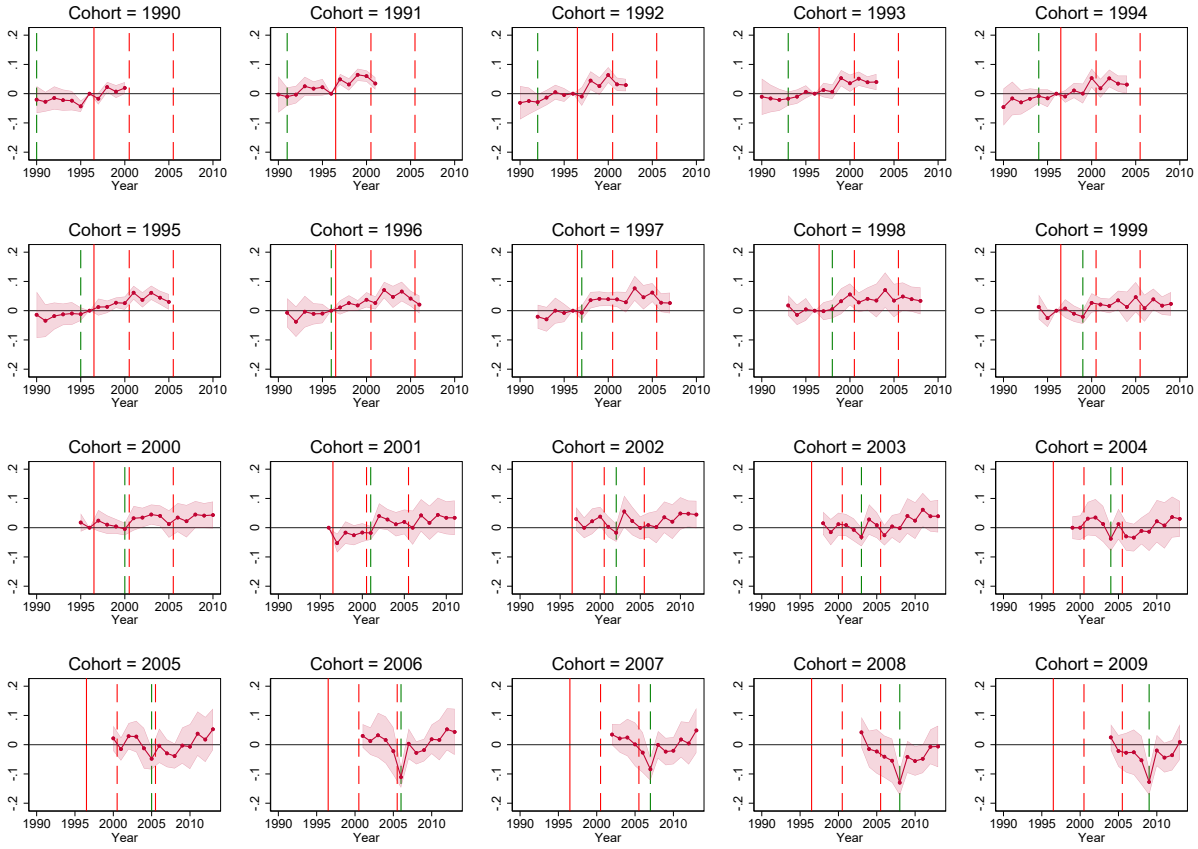
Cohort-specific trends for employment and earnings are considerably noisier, but some interesting patterns emerge. Birth cohorts of the early 1990s see substantial earnings increases after 1997. Among cohorts of women who became mothers between 1997 and 2002, a significant positive earnings differential 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, mothers' employment rates 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 A24: Effect of Quebec's Childcare Policy on Childcare Expenses Claiming Rate, by Cohort



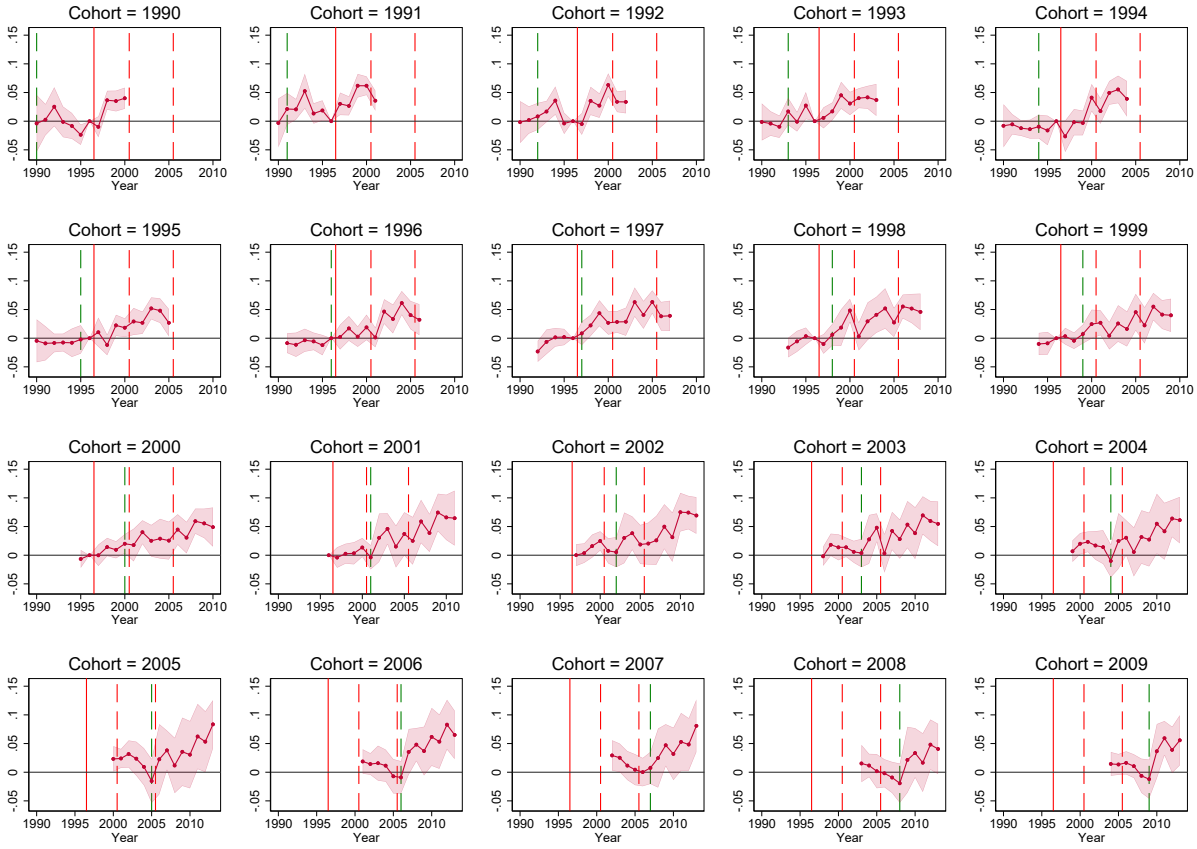
Notes: This figure presents estimates from equation (4) separately by cohort (year when women became mothers). All estimates are based on a sample of new mothers in the Longitudinal Administrative Databank (LAD). The solid red line indicates when Quebec's childcare policy was implemented. The left-most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

Figure A25: 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 indicates when the policy was fully phased-in, and the right-most dashed red line indicates when QPIP was put in place. Green dashed lines indicate the cohort year. Shaded areas show 95% confidence intervals.

Figure A26: 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 indicates when the policy was fully phased-in, and the right-most dashed red line indicates when QPIP was put in place. Green dashed lines indicate the cohort year. Shaded areas show 95% confidence intervals.

F Group Shares

Using childcare take-up 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 $\tilde{G}_i = \left(\frac{1}{\alpha}\right) G_i + \nu_i$, 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.[41] 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 6. The resulting scaling factor is equal to 0.87, but the results are largely insensitive to the value of the scaling factor. 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 $\pi_{PN}^{CD} = \pi_N^{CD} - \pi_{GN}^{CD}$.

[41] These data are compiled in Statistics Canada (2019a,b, 2020a,b).