The Effect of Childcare Access on Women's Careers and Firm Performance*

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Abstract

We study the effect of government-subsidized childcare on women's career outcomes and firm performance using linked tax filing data. Exploiting a universal childcare reform in Quebec in 1997 and the variation in its timing relative to childbirth across cohorts of parents, we show that earlier access to childcare increases not just new mothers' employment, but their active reallocation of careers across firms. New mothers are more likely to switch to firms they find traditionally unattractive (e.g., offering demanding, inflexible jobs), leading to higher earnings and productivity. Such firms benefited from the reform, drawing more young female workers and experiencing better performance. Our results suggest that childcare frictions hamper women's career progression and their allocation of human capital in the labor market.

JEL Classification Codes: G28, G32, K22

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1 Introduction

Gender disparities in pay and career progression characterize labor markets in most economies. An extensive literature has sought to understand the drivers of these disparities, with an increasing consensus pointing to women's child-rearing responsibilities as an explanation (Kimmel, 1998; Goldin, 2014; Goldin and Katz, 2016; Kleven et al., 2019). At the same time, there is increasing demand by firms, investors, and policymakers for a gender-balanced workforce and greater equality in pay (Fluchtmann and Patrini, 2023). One potential policy solution is to subsidize early-age childcare. Childcare subsidies have been at the forefront of policy debates in several major developed countries in recent years, albeit with mixed legislative success.[1]

Existing evidence on the effect of childcare subsidies on maternal labor supply and family outcomes has been mixed. While some studies have found positive impact of childcare subsidies on female labor supply (Baker et al., 2008; Lefebvre and Merrigan, 2008), others have found limited to no impact (Havnes and Mogstad, 2011a; Fitzpatrick, 2010; Kleven et al., 2020), with mixed evidence on children or parents' well-being (Baker et al., 2019; Brodeur and Connolly, 2013; Kottelenberg and Lehrer, 2017). Moreover, we know little about how such policies impact female employees' career progression, productivity and, ultimately, how they impact firm outcomes. This paper fills this void by studying a universal childcare reform in Quebec, Canada. Our objective is to understand how subsidizing childcare may impact individual and firm outcomes by relaxing constraints in labor allocation and reducing frictions that potentially generate gender-segmented labor markets.

Beginning in 1997, Quebec introduced subsidized, universally accessible childcare to parents, regardless of their income and employment. The program provided childcare for

[1] Canada and Australia both passed major subsidies in recent years, with the Canadian federal government reaching an agreement with all provinces to provide $10-a-day childcare in 2022 and the Australian government passing a $5.4 billion childcare subsidy package in 2023 that subsidizes up to 90% of their childcare costs for families earning up to $80,000. In contrast, major childcare subsidy provisions were cut from both the Build Back Better Act in 2021 and the Inflation Reduction Act of 2022 in the United States.

young children at a subsidized rate of \$5 CAD per day (about \$3.6 USD), at a time when the median childcare cost was \$11 CAD per day. The program also substantially increased the number of available regulated childcare spaces. Our empirical strategy exploits the timing of childbirth relative to the reform across different cohorts of parents to generate variation in the length of childcare-related career interruptions. Using the birth year of the first child as the reference point, we estimate a difference-in-differences (DID) model comparing parental outcomes before and after childbirth across different cohorts.

We employ linked tax filing data from Statistics Canada for our analysis. The data contain the universe of Canadian workers and their linked employers, with information on individuals' earnings, family structure, reasons for job separations, as well as firms' financial outcomes. Such family-employee-employer administrative data is important in tracing out the career impact of access to childcare, as well as its effect on worker-firm sorting in the labor market.

We find that earlier access to childcare significantly increases employment among new mothers, consistent with prior studies (Baker et al., 2008; Goux and Maurin, 2010; Bauernschuster and Schlotter, 2015; Geyer et al., 2015). We further differentiate the employment response based on employment status prior to childbirth. We show that such employment effect is stronger among mothers who were unemployed before childbirth, and is more limited among those employed before childbirth. This result suggests that childcare subsidies have weaker employment effects on women who were already attached to the labor force, and are effective in drawing those who were previously unemployed into the labor force.

The insignificant employment response by new mothers previously employed, however, could mask important intensive margin employment effects in terms of worker-firm sorting and earnings, which is the focus of our paper. Becker (1985) argues that childcare causes women to expend less effort at work and to seek less demanding jobs. Therefore, shortening the length of career interruptions brought on by childbirth could lead new mothers to pursue more ambitious career paths. Focusing on new mothers who were employed pre-childbirth, we find that timely access to childcare increases the likelihood they voluntarily switch employers; it also increases their earnings growth, suggesting potential career upgrades into more demanding, higher-paying jobs. Such an earnings increase happens both within an individual's current employer, as well as across employers when they switch firms. Using *large* earnings changes to proxy for promotions and demotions, we also find that childcare access increases new mothers' promotions and decreases their demotions both within and across firms. These findings are consistent with prior research showing large and persistent wage gains accompanying voluntary job changes (Antel, 1986).

Examining heterogeneity, we find that the above individual-level results are stronger among single, younger, and lower-income mothers, consistent with these individuals facing greater time or financial constraints in providing/securing childcare. We also find that the reform reduced female workers' absenteeism proxied by sick leave (Bennedsen et al., 2019), suggesting that subsidized childcare increases women's productivity at work. The reform also lead new mothers to delay the births of their subsequent children after the first child, consistent with childcare access setting them on a more intense career path.

Our baseline individual-level results on mothers' employment, turnover, and earnings are robust to several alternative specifications. First, in a dynamic DID, we find no evidence of pre-birth trends in these outcomes, suggesting that the parallel trends assumption is likely to hold across different cohorts of mothers. Second, we find similar results controlling for heterogeneous trends across individuals' ex-ante characteristics. Third, using fathers as a benchmark group in a triple-difference analysis, we show that our main results are concentrated in new mothers and largely absent among new fathers. This finding is consistent with mothers bearing most of the childcare responsibilities, and it is the relief from such responsibilities that drives our results. The triple-difference analysis also allows us to further include family-year fixed effects to compare mother and father within the same household, as well as cohort-year fixed effects to absorb cohort-level shocks. Last, to rule out the concern of pregnancy or birth timing, we show that our results remain similar when focusing on the cohorts whose fertility decisions were made before the reform.

We then examine the effect of the reform on female workers' sorting into firm types. We show that access to subsidized childcare increases the likelihood that new mothers sort into firms that tend to be less attractive to the needs of new mothers. We proxy for firms where new mothers might find it costlier to work in three ways. Our main measure is the fraction of new mothers employed at a given firm pre-reform. Arguably, firms employing fewer new mothers are less appealing to this employee demographic. Additionally, we identify "greedier" jobs, i.e., jobs with higher earnings-hours elasticity (Goldin, 2014), which are less attractive to new mothers who value flexible hours or time with their young children. Lastly, we exploit heterogeneity based on firm size. Smaller firms are known to offer fewer non-wage benefits such as maternity benefits (Liu et al., 2021) and should thus be less appealing to new mothers. Using these measures, we show that childcare subsidies increase new mothers' sorting into "mom-unfriendly" firms, firms with high pay convexity, as well as smaller firms. Such a sorting effect is not driven by higher retention of female workers by "mom-unfriendly" firms; rather, these firms draw job switchers from "mom-friendly" firms. These findings are consistent with the career upgrade channel revealed by our individual-level results: lowering childcare frictions allows new mothers to pursue more demanding careers that are traditionally unappealing to new mothers.

Last, we examine the impact of the childcare reform on firm outcomes. We document heterogeneous impacts of the reform on Quebec firms: consistent with the individual-level sorting result, firms that had lower shares of new mothers before the reform gained employment post reform relative to other firms, an increase driven by more female employees. "Mom-unfriendly" firms also experienced better performance, measured by higher sales growth and higher labor productivity. These results suggest that firms traditionally unattractive to new mothers benefit particularly from universal childcare.

Our paper contributes to the literature studying the effect of providing non-wage amenities to female employees (e.g. maternity leave, childcare) on individual career, and firm outcomes. Studies have shown that maternity leave increases job continuity (Baker and Milligan, 2008), encourages female entrepreneurship (Gottlieb et al., 2021), and can be used by firms to attract and retain female talent (Liu et al., 2021). Bennett et al. (2020) show that paid family leave improves firm productivity by reducing employee turnover. We differ by showing that childcare subsidy *increases* female workers' voluntary turnover to pursue more ambitious careers, leading to heterogeneous impacts on firms. Unlike family leave, which ties a worker to her firm, childcare subsidy is unrelated to a worker's employer or employment status. Prior literature has documented either a positive or insignificant effect of childcare subsidies on mothers' labor supply (Baker et al., 2008; Lefebvre and Merrigan, 2008; Fitzpatrick, 2010; Havnes and Mogstad, 2011a; Kleven et al., 2020), with mixed evidence on children and parents' well-being (Baker et al., 2019; Cascio, 2009; Havnes and Mogstad, 2011b).[2] Several papers document childcare as a driver of gender disparity in labor market during the COVID-19 pandemic (Barber et al., 2021; Furman et al., 2021; Couch et al., 2021). Related to our paper, Chhaochharia et al. (2021) show that women in German counties with more childcare provision have higher earnings and are more likely to be promoted after childbirth. We differ from these papers in two dimensions. First, our cohort-based identification allows us to exploit finer variation in the length and severity of childcare-induced career interruptions for tighter inference. Second, we document important intensive margin effects of childcare frictions conditional on employment: childcare frictions hamper working women's career progression and impact worker-firm matching. We also document the heterogeneous impact of childcare frictions on firm performance. Government policies that support childcare can therefore reduce gender gaps in the labor market by allowing talent to flow more freely between gender-segmented sectors.

Our paper also relates to the literature on women's career progression and firms. Several papers highlight gender disparities in certain sectors or occupations. Lagaras et al. (2022)

[2]Other papers include Lefebvre and Merrigan (2008), Lefebvre et al. (2009), Bettendorf et al. (2015), Nollenberger and Rodríguez-Planas (2015), Bauernschuster and Schlotter (2015), Kottelenberg and Lehrer (2017), and Cornelissen et al. (2018). See Olivetti and Petrongolo (2017) for a review.

show that female talent sort away from the financial sector due to inflexible working schedules. Ellul et al. (2020) show that female employees are underrepresented in more demanding careers in finance, such as asset management. Reuben et al. (2014) and Hebert (2020) document gender stereotypes in science careers and the financing of entrepreneurs, respectively. He and Whited (2023) show that shortage of suitable candidates explain underepresentation of women CEOs rather than labor demand. Several papers also provide evidence of potential solutions to gender gaps in the labor market. Bennedsen et al. (2020) show that transparency regulation that asks firms to disclose gender-disaggregated wage statistics narrows the gender pay gap by lowering the wage growth of male employees. Tate and Yang (2015) show that female leadership promotes a more female-friendly culture in firms. We add to this literature by showing that policies that improve access to childcare improve women's employment and pay outcomes and can be beneficial to firms.

2 The Quebec 1997 Reform

Our empirical strategy exploits the introduction of universal childcare in Quebec in 1997, which is the center piece of the 1997 Quebec Family Policy ("Politique Familiale").[3] The policy provided childcare for children aged zero to four at a subsidized rate of $5 CAD per day, at at time when the median childcare cost was $11 per day (Lefebvre and Merrigan, 2008).[4] The program was rolled out gradually between 1997 and 2000, with four-year-olds first qualifying in September 1997, followed by three-year-olds qualifying in September 1998, two-year-olds qualifying in September 1999, and children aged zero to one qualifying in September 2000. During the same period, childcare services remained unchanged in the rest of Canada, and universal childcare was never offered in other provinces.

[3]Quebec is the second largest province in Canada, representing 25% of Canada's population and 22% of GDP in 1997. Unlike other large provinces in Canada that rely heavily on a particular sector (e.g., Alberta on energy and Ontario on auto), Quebec's economy is well diversified across a large number of sectors.

[4]This $11 reflects the childcare cost paid by a middle-income family after tax credits and federal deductions.

The subsidized childcare program was universally offered to all families in Quebec, without any employment or income restrictions. However, low-income and single-parent households were eligible for some childcare subsidies prior to 1997. The universal childcare plan for children aged zero to four was accompanied by additional measures that targeted school-age children, which included voluntary full-day kindergarten for five-year-olds and subsidized after-school childcare programs for children aged 5 through 12. This additional school support, however, was not phased in based on children's age.

The implementation of the program involved the conversion of existing non-profit childcare centers into *Centres de la petite enfance* (centers for young children, known by the acronym CPE). Each CPE offered childcare services but also served a network of regulated home-based childcare providers that emerged as part of the policy implementation. The home-based providers were favored by children of younger children while parents of children above the age of two preferred CPEs (Baker et al., 2008). From 1996 to 2005, the number of CPEs doubled and the number of home-based care centers quadrupled. Together, the total number of regulated spaces increased from about 74,000 in 1996 to 200,000 in 2006 (Figure 1 Panel A), while program funding increased from $288 million to $1.6 billion over that period. As a result of this reform, the percentage of Quebec children of 0-5 years old in childcare centers increased from 11% in 1996 to 31% in 2002 (Figure 1 Panel B) (Baker et al., 2008; Lefebvre and Merrigan, 2008). Moreover, there was no evidence that this increase came at the expense of unregulated spaces (Kohen et al., 2008).

The 1997 Quebec family policy was announced in January of 1997 (Tougas, 2002). The details about the policy were revealed in a white paper titled *Les enfants au Coeur de nos choix* (*Children at the heart of our choices*) released in 1997. The policy was introduced into the provincial legislature in the spring of 1997, and was met by opposition from unlicensed childcare providers who were left out of the subsidy program (Globe and Mail, June 1997). There were doubts at the time that the government could afford the program, as well as critiques that the government was "leaping before it looks". Because of these frictions, the policy was not implemented until September 1, 1997. Given the suddenness and uncertainty of the policy implementation, it is unlikely that its timing was anticipated well in advance. Additionally, the policy was introduced to achieve the objectives of fighting family poverty and enhancing child development and equality of opportunity (Lefebvre and Merrigan, 2008); parental employment and firm productivity were not part of the policy objectives. Important for our identification, Quebec did not implement a welfare-to-work program around 1997 like other major Canadian provinces. Such a program, which requires welfare recipients to work as a condition of receiving benefits, could increase the labor supply of single mothers (Agostinelli et al., 2020).[5] As such, our empirical strategy will not use non-Quebec provinces as a control group; instead, we will exploit finer variations within Quebec.

Despite staggered eligibility by age and gradual increases in the number of regulated spaces, demand exceeded supply in the first few years and many parents were placed on waiting lists (Baker et al., 2008). As documented by Ding et al. (2020), this rationing led to a disproportionate increase in childcare enrollment among younger children who did not yet have access to the subsidy (they would simply enroll at the unsubsidized price). This strategic response from parents to "claim a spot" in the system could also reflect intertemporal smoothing of expected future subsidies.[6] Although parents had to pay unsubsidized prices for early enrollment, they still benefited from the increased availability of childcare spaces. These institutional features inform our empirical design, in which we exploit variation in the number of years that parents *knew about the subsidy program* rather than years of eligibility across birth cohorts.

[5]In 1995, the federal government of Canada replaced the Canada Assistance Plan (CAP) with the Canada Health and Social Transfer (CHST). The CAP had ensured welfare benefits could not be denied to eligible individuals in need, but those protections were lifted under the CHST, giving individual provinces the ability to pass legislation requiring welfare recipients to work as a condition of receiving benefits. This resulted in several provinces (including Ontario, British Columbia, Alberta, Manitoba, and Nova Scotia) instituting welfare-to-work programs in the following years that significantly increased the incentives of single mothers to enter the workforce (Evans, 2009).

[6]Anticipatory response to social programs before becoming eligible is prevalent. See Attanasio and Rohwedder (2003), Card and Hyslop (2005), and Card et al. (2007) for examples.

3 Data and Sample

We use an administrative dataset custom developed by Statistics Canada for our analysis. The dataset links the Longitudinal Worker File (LWF) and T2-Longitudinal Employment Analysis Program data (T2LEAP), and covers the full universe of Canadian employees and their matched employers from 1989 to 2013. The dataset is built from four tax files: T1 personal income tax file, T2 corporate income tax file, T4 employee remuneration file, and Record of Employment (ROE). The ROE file contains information on workers' employment history and reasons for job interruptions and separations.[7] We further add family relationship information and children's birth years from the T1 Family File (T1FF) to identify when an individual has a child. Because of the comprehensive and linked nature of our data, we are able to observe, longitudinally, worker-level employment, absenteeism, turnover, earnings, family structure, as well as firm-level financial performance. This allows us to trace out the career and productivity impact of the policy from the individual level to the firm level. As such, our data represents one of the most comprehensive data sets assembled to study childcare and employment. Detailed variable definitions are discussed in the next section.

We assign each individual in the LWF dataset to a childbirth cohort based on the earliest childbirth year associated with that individual in the T1FF dataset. This means that we examine outcomes around the birth of each parent's *first child*. Classifying cohorts based on the birth of one particular child allows us to clearly define pre-birth and post-birth outcomes for each individual, and using the first child as the reference point is motivated by the higher degree of career disruption associated with first-time parents.[8]

We further restrict our sample to individuals who gave birth between 1993 and 1999.

[7]A firm needs to file an ROE whenever its employees experience earnings interruptions, even if the employee does not intend to file a claim for Employment Insurance benefits.

[8]The T1FF data does not indicate if an individual linked with a child was part of the family (as the biological or adoptive parent) at the time of the child's birth. To limit the number of cases in which an individual marries into a family years after the birth of the child (in which case the birth should not have affected that individual's career trajectory), we exclude all instances where the gap between parent age and child age is less than 18.

We start from the 1993 cohort because this is the earliest cohort for which we have 4 years of pre-birth data. Another reason is that the 1993 cohort is the earliest cohort that's still in childcare in 1997, so this ensures that we are not picking up any effect from kindergarten subsidies. We end with the 1999 cohort to make sure our results are not affected by the federal paid parental leave extension at the end of 2000.[9] Our calendar years run from 1989 to 2004.

Our firm-level sample covers the period of 1993 to 2001. We exclude firms in the financial, utilities, and government sectors (NAICS 52, 22, 91, respectively). We also require the firm to have at least five employees and \$25,000 in total assets in 1996.

4 Empirical Strategy

4.1 Individual-level Specification

Our empirical strategy at the individual-level exploits the differential effects of the program on parents of children born in different years. Specifically, we estimate the following generalized difference-in-differences (DID) regression:

$$Y_{i,t,y} = \alpha_i + \beta_t + \gamma_y + \theta \times CCYears_i \times PostBirth_{i,t} + \epsilon_{i,t,y}, \tag{1}$$

where $Y_{i,t,y}$ is an employment outcome for individual i in calendar year t and event year y (relative to childbirth year), α_i, β_t, and γ_y are individual fixed effects, calendar-year fixed effects, and event-year fixed effects, respectively. Individual fixed effects remove fixed personal characteristics, such as education, demographics, or innate tendency to participate in the labor market due to ability or ambition. We use calendar-year fixed effects to control for macro trends. Event-year fixed effects absorb kid's age fixed effect and control for changing

[9] On December 31, 2000, the Canadian federal government extended the income replacement period associated with parental leave (i.e. paid leave) for all provinces.

need for childcare that varies with kid's age, as well as changing life patterns and preferences around pregnancy and childbirth. In our more stringent specifications, we also include industry (or industry-year) fixed effects or firm fixed effects to identify within-industry(firm) variation.[10] We cluster standard errors at the individual level.

Our key treatment variable is $CCYears_i \times PostBirth_{i,t}$, where $PostBirth_{i,t}$ indicates years after an individual gives birth. $CCYears_i$ is a cohort-level treatment intensity variable that indicates years of post-reform childcare access before the first child turns five.[11] Specifically, it is equal to $min(5, T - 1992)$, where T is the year an individual gives birth. As such, $CCYears_i$ takes a value of 5 for individuals giving birth in or after 1997, a value of 4 for those giving birth in 1996, a value of 3 for those giving birth in 1995, and so on. This specification forms a generalized difference-in-differences estimation in which all individuals are "treated" by the birth of their first child, but the intensity of the treatment varies with how old the child was when the 1997 program was introduced. Our treatment variable $CCYears_i \times PostBirth_{i,t}$ thus identifies the effect of each additional year of *earlier* access to subsidized childcare on an individual's labor outcomes post-childbirth (relative to pre-childbirth).

Our variation in treatment intensity comes from two sources: 1) the number of years one's child is age-eligible for subsidized childcare rate (eligible years), and 2) the number of years parents are aware of but not yet qualify for subsidized rates (anticipatory years). During the anticipatory years, parents pay the unsubsidized price for childcare but benefit from access to an increased number of childcare spaces (see Figure 1); they may also enroll their children to strategically secure a spot or intertemporally smooth the expected future subsidy (Ding et al., 2020). Later cohorts enjoy longer eligible years and/or longer anticipatory years, hence receiving higher treatment intensity.

[10]We define industry as 4-digit NAICS. In a robustness test, we also include age bin fixed effects. See Panel A of Table A.3.

[11]Note that the level term of $CCYears_i$ is absorbed by individual fixed effects, and the level term of $PostBirth_{i,t}$ is absorbed by event-year fixed effects.

Table 1 provides a breakdown of the eligible years (dark blue cells) and anticipatory years (light blue cells) for each cohort. The total number of treated years corresponds to the value of $CCYears_i$. The grey cells indicate years pre-treatment, i.e., years parents had to wait before the reform. We see, for example, that the 1995-cohort and the 1996-cohort parents both qualified for childcare subsidy when their child turned three, but 1996-cohort parents *learned* about the subsidy program one year earlier in the life of their child (at age one versus age two). If parents responded to the subsidy program by enrolling their child early in order to secure a spot, then we should expect childcare-induced career interruption to be shorter and less severe for 1996-cohort parents relative to 1995-cohort parents.

To balance our panel around the birth of the child, we restrict our sample to a fixed event window from 4 years before childbirth to 5 years after. This allows us to treat our specification as a continuous difference-in-differences in which all individuals are treated at the same event time (i.e., birth year of first child) and the continuous variation in treatment intensity comes from $CCYears$. Framing our analysis in event time and including event-time fixed effects also allows us to mitigate concerns over treatment effect weighting issues found in *staggered* continuous treatment difference-in-differences (Callaway et al., 2021). Our calendar-year fixed effects allow us to control for differences in calendar-time windows across cohorts.

Most of our analysis based on Equation 1 is limited to the sample of female individuals because women bear the vast majority of childcare responsibilities. Nevertheless, in subsequent analysis, we use males as a benchmark comparison group in a triple-difference specification. We expect to find limited effects among males if childcare responsibilities are the main driving force behind our results. We describe the triple-difference specification in detail in Section 5.2.

We examine three sets of individual-level outcomes: employment, turnover, and earnings. We define employment as an indicator variable for whether an individual filed a T4 tax slip in a given year. We define turnover as an indicator for whether an individual separated from their employer in a given year with their record of employment (ROE) indicating "quit" being the reason for separation, which captures voluntary rather than forced turnover (e.g., layoff). We also use an alternative turnover measure from T4 filings that identifies a switch in employer based on whether an individual is employed by a different firm than they were the previous year (though such a switch may not necessarily be voluntary). Lastly, we define earnings as an individual's T4 earnings scaled by their pre-birth T4 earnings.

We also use Equation 1 to examine the sorting of individuals into firm types. We construct several firm type measures. Our primary measure is whether a firm had an above-industry-median fraction of mothers of pre-kindergarten children (age$<$5) in 1996, the year before the reform, which captures how "mom-friendly" a firm is. Additionally, we use industry-level pay convexity (i.e., earnings-hours elasticity) from Goldin (2014) to capture sectors with jobs that reward long and continuous hours (i.e., "greedy" jobs). Lastly, we use a firm's relative size within the industry to capture the quality of maternity benefits, as smaller firms tend to provide fewer maternity benefits (Liu et al., 2021). We include these firm type measures as the dependent variable in our regression to examine sorting into firm types.

4.1.1 Validating Sources of Identification

To illustrate and validate our cross-cohort comparison, we plot cohort-level mean employment rate for Quebec women in Figure 2(a).[12] We see a clear difference across cohorts in post-birth employment rate, with later cohorts experiencing higher levels of employment earlier in the life of their children relative to earlier cohorts. Another pattern is that each pair of adjacent cohorts follows parallel paths until an inflection point where the later cohort diverges upward. We interpret these inflection points as reflecting the different ages at which adjacent cohorts respond to the subsidy program. Importantly, we observe this pattern when examining the 1993/1994 cohort pair and the 1995/1996 cohort pair. Since both

[12] The means are adjusted to align with the 1993 cohort's value in the year before childbirth.

cohorts within each of these pairs became eligible for the subsidy at the same age (Table 1), the patterns indicate that the difference comes from the anticipatory earlier response by the later cohort. We also find similar patterns when examining the 1994/1995 and 1996/1997 cohorts pairs. In each pair, both cohorts had the same number of anticipatory years, while the later cohort enjoyed one additional eligible year (Table 1). In contrast, Figure 2(b) shows that we do not find similar patterns of divergence among the 2000 to 2004 placebo cohorts, whose children were always eligible for the subsidy (i.e., no variation in treatment). This placebo result suggests that the employment effect in Panel A is not driven by any secular trend in female employment. Additionally, any secular trend should shift employment in parallel across cohorts instead of inducing divergence at a specific child age.

We use the National Longitudinal Survey of Children and Youth (NLSCY) to validate the anticipatory and the eligibility effect of the reform on childcare take-up.[13] We focus on Quebec children aged 0-4 and create two treatment indicators indicating anticipatory years and eligible years as in Table 1, with pre-reform years as the control years. Table A.1 presents the results. Column 1 shows that childcare take-up increased significantly following the 1997 reform. In particular, the take-up rate increased by 57% relative to the pre-reform mean. Column 2 shows that this increase can be attributed to both an anticipatory effect (a 3.1 pp increase in take-up) and an eligibility effect (a 9.5 pp increase in take-up), which represents a 36% and a 110% increase relative to pre-reform mean, respectively.

5 Individual-Level Results

5.1 Baseline Results

We first examine the effect of access to childcare on women's career outcomes. Table 3 shows the effect on employment status. We find that earlier access to subsidized childcare

[13]We use the 94-95, 96-97, and 98-99 cycles. The sample is repeated cross-sections since children are de-identified in the public version of the data accessible to us.

significantly increases the likelihood of women being employed post-childbirth relative to pre-childbirth (column 1). In particular, a five-year earlier access to childcare—i.e., full access for all years before kindergarten—increases the female employment rate by 1.75 percentage points, which is a 2.7% increase relative to post-childbirth mean before the reform. This effect accounts for 12% of the child penalty in employment estimated in Kleven et al. (2022) for Canada. This effect is smaller than that estimated in prior papers (e.g., Baker et al. (2008), Lefebvre and Merrigan (2008), and Bauernschuster and Schlotter (2015)), likely because we exploit finer variation in exposure to the reform across cohorts, as well as within-person changes surrounding childbirth. Additionally, we estimate an intent-to-treat, i.e., the effect of childcare access, instead of the effect of childcare take-up, which would be larger.

Next, we partition the sample based on individuals' employment status the year before childbirth. We find that the increase in employment rate is mainly driven by mothers who were unemployed before childbirth, whereas mothers employed pre-childbirth do not react significantly to the availability of childcare. Such a heterogeneous response is new to the literature.[14] The latter result could be explained by the fact that those working before childbirth were attached to labor force enough that they do not drop out for childcare-related reasons. Nevertheless, childcare access could still impact other margins of employment conditional on being employed. The insignificant result for previously-employed mothers also alleviates selection concerns in our subsequent analyses that examine other labor outcomes conditional on employment.[15]

Next, we investigate other margins of employment conditional on being employed the year before childbirth. We first examine voluntary turnover in Table 4. In columns 1-3, we define voluntary turnover based on "quits" in records of employment (ROE), and in columns 4-6, we examine employer changes based on T4 filings. We find that earlier access to

[14]Prior literature has documented heterogeneous employment response to childcare by marital status (Kimmel, 1998; Cascio, 2009; Goux and Maurin, 2010) and education (Lefebvre et al., 2009).

[15]Note that unemployed in the year before childbirth does not include individuals on maternity leave. We define unemployment as missing a T4 wage slip. Individuals on leave from their employer would still be issued a T4 and be classified as employed (but on leave).

childcare significantly increases the likelihood that new mothers voluntarily switch employers from one year to the next.[16] Based on the coefficient in column 1, a five-year earlier access to childcare increases a new mother's voluntary job switching rate by 1.2pp, which is a 34% increase relative to the post-childbirth mean before the reform. These effects are robust to including industry fixed effects or firm fixed effects, which control for the average job turnover rates in an industry or firm. We find similar effects on turnovers identified from T4 employer changes (columns 4-6), with similar magnitudes. In Panel A of Table A.2, we also show that new mothers are more likely to leave their pre-childbirth employer and join a new firm (columns 1-2). These findings suggest that access to childcare increases labor market mobility for female workers. These results are consistent with childcare support freeing up time and resources for new mothers to look for a new job, or to take up more demanding careers.

We then examine the income effect of childcare access, again conditional on employment. In columns 1-2 of Table 5, the dependent variable is an individual's current earnings scaled by her earnings in the year before childbirth. We find that earlier access to childcare significantly increases a woman's earnings relative to her pre-childbirth earnings. In particular, the coefficient in column 1 implies that accessing childcare five years earlier increases a new mother's post-birth earnings by 21.6% relative to pre-birth earnings. In column 2, we further include individual-firm fixed effects to identify the earnings' effect within the same employer and find a five-year effect of 7.5%. This suggests that a large part of the earnings increase is realized through switching employers.[17] Of course, increases in work hours may also explain some of the earnings increase, though we do not observe hours in our data.

Columns 3-8 of Table 5 further examine the likelihood of promotions and demotions, which we proxy using large earnings changes. Following McCue (1996), we define promotions

[16] Quits from ROE identifies voluntary rather than forced separations. Our results cannot be driven by quitting to become unemployed as we showed that the employment rate did not change significantly for those employed before childbirth.

[17] We find similar results when examining year-to-year earnings growth (see Panel A of Table A.2).

as earnings increases higher than 10% and demotions as earning decreases higher than 10% relative to pre-childbirth earnings.[18] We find that accessing childcare earlier by five years increases the likelihood of promotions by 8.2pp (41% relative to the mean) (column 3), increases the likelihood of within-firm promotions by 6.9pp (column 4), and increases the likelihood of between-firm promotions by 7.9pp (column 5). Columns 6 and 7 show an opposite effect on demotions: a five-year earlier access leads to a 5.3pp decrease (19% relative to the mean) in the likelihood of demotions, a 3.8pp decrease in the likelihood of within-firm demotions, and a 1.4pp decrease in the likelihood of between-firm demotions, respectively. In summary, access to childcare increases new mothers' earnings and their career advancement through both a within-firm and between-firm effect.

Our baseline results cannot be explained by a wealth effect from childcare subsidies. First, the subsidy is not a cash transfer, but is tied to the use of government childcare. Hence, it would only represent a positive wealth shock for those who would have paid for the more expensive private childcare. Further, a positive wealth shock would likely make new mothers *less willing* to upgrade their careers, as a large literature documents that positive liquidity shocks reduce individuals' labor supply or job search efforts.[19]

One may also wonder why some new mothers did not take up unsubsidized childcare before the reform given the large earnings gains we document. It is also worth emphasizing that the effect of the reform comes not just from childcare cost subsidy, but also from the expansion in the number of childcare spaces available (see Figure 1). Anecdotal evidence abounds that many parents, including wealthy ones, could not put their kids into childcare and had to be on long waiting lists. Hence, our results do not suggest that parents were not optimizing or were "leaving money on the table" before the reform.

[18] McCue (1996) documents that wage growth associated with promotions centers around 10% across different demographic groups. Our results are robustness to using 20% or 30% as the cutoff point (see Panel B of Table A.2).

[19] See, for example, Lentz and Tranaes (2005), Card et al. (2007), Chetty (2008), Cesarini et al. (2017), and Li et al. (2020).

5.2 Robustness

We conduct a variety of robustness tests on our baseline individual-level results.

First, we verify parallel trends in our main outcomes using a dynamic DID specification:

$$Y_{i,t,y} = \alpha_i + \beta_t + \gamma_y + \sum_{n=-4}^{5} \theta_n \times CCYears_i \times YearsToBirth_{i,n} + \epsilon_{i,t,y}, \tag{2}$$

where $YearToBirth_{i,n}$ is a dummy indicating event year relative to childbirth. The childbirth year ($n = 0$) is omitted as the base year. Figure 3 shows the results. We find that the cohorts do not exhibit significantly different trends in all three outcomes in the years before childbirth, but diverge significantly in the years after childbirth. This indicates that childcare access does not affect individuals' career trajectories prior to the birth of their first child.

An interesting story emerges when we examine the post-childbirth period. We see that the effect of childcare access on employment begins to decline by year 3, but the effects on turnover and earnings persist and strengthen over time. These results suggest that new mothers are likely to return to the workforce eventually, especially when their child becomes eligible for kindergarten at age 5, but facing longer and more severe childcare-induced work interruption has long-term effects on their career trajectories as measured by job-switching and earnings growth. These results highlight the importance of studying the intensive margins of employment when examining the effect of childcare on women's careers.

Second, one may be concerned that our results are driven by secular trends across different cohorts of mothers. The placebo graph in Figure 2b alleviates this concern. To further address this concern, we saturate the model with heterogeneous trends across individual characteristics. Specifically, we include the interactions of individuals' pre-birth characteristics with event-year dummies. These characteristics include age, marital status, and earnings. The results are in Panel B of Table A.3 and are similar to our baseline results.

Third, to further sharpen identification, we conduct a triple-difference using men as a benchmark group. This helps address any remaining concerns about unobserved differences across cohorts of mothers unexplained by individual, event-year, and calendar-year fixed effect. Specifically, we estimate the following two equations:

$$\begin{aligned} Y_{i,t,y} = \alpha_i + \beta_{s,t} + \gamma_{s,y} + \theta_1 \times CCYears_i \times PostBirth_{i,t} \\ + \theta_2 \times CCYears_i \times PostBirth_{i,t} \times Female_i + \epsilon_{i,t,y}, \end{aligned} \tag{3}$$

$$Y_{i,t,y} = \alpha_i + \beta_{s,t} + \gamma_{s,y} + \delta_{c,y} + \sigma_{f,t} + \theta_1 \times CCYears_i \times PostBirth_{i,t} \times Female_i + \epsilon_{i,t,y}, \tag{4}$$

where $Y_{i,t,y}$, α_i, $CCYears_i$, and $PostBirth_{i,t}$ are the same as in equation 1. $\beta_{t,s}$ and $\gamma_{y,s}$ indicate calendar year-gender fixed effects and event year-gender fixed effects, respectively. These fixed effects absorb gender-specific trends across calendar years and event years around childbirth. In our tightest specification in Equation 4, we further include cohort-event year fixed effects ($\delta_{c,y}$) to absorb secular trends in labor market across cohorts of parents, and family-year fixed effects ($\sigma_{f,t}$) to absorb unobserved household-level shocks.[20] Table 6 presents the triple-difference results. We find that our baseline results indeed concentrate among mothers, and are largely absent among fathers (columns 1, 3, and 5), except for some weak earnings results likely linked to within-household earnings spillovers. The lack of response among fathers is consistent with the idea that mothers bear most of the childcare responsibilities, and that our main results are driven by differences in the easing of these responsibilities across cohorts of mothers rather than other confounding differences that would also affect cohorts of fathers. We find similar results in columns 2, 4, and 6, except that the employment result is weakened, as family-year fixed effects restrict to married mothers who have a much weaker employment response than single mothers (see Table 7).

Last, to address concerns about potential pregnancy/birth timing in response to the reform, we further restrict our sample to the 1993-1997 cohorts, whose fertility decisions were made before the announcement of the reform. Table A.4 shows that the results remain

[20]Note that in both equations, $PostBirth_{i,t} \times Female_i$ is absorbed by gender-event year fixed effects and $CCYears_i \times Female_i$ is absorbed by individual fixed effects. In Equation 4, $CCYears_i \times PostBirth_{i,t}$ is absorbed by cohort-event year fixed effects.

similar. We also show that our results are robust to excluding employees in the public sectors or part-time employees (Table A.5).

5.3 Heterogeneity and Additional Outcomes

Next, we examine heterogeneity in our main results. In particular, we explore the role of marital status, age, and earnings (all measured in the year before childbirth) in a triple-difference specification.[21] A priori, the interaction effects are ambiguous. Single, young, low-income mothers could react more strongly to childcare subsidies due to their greater time and financial constraints in providing/securing childcare themselves. On the other hand, these individuals received a smaller subsidy shock from the reform, as some of them already qualified for other childcare support from the government before the reform. Furthermore, the increased supply of childcare spaces should benefit all parents regardless of their social economic status. Table 7 presents the heterogeneity results. We find that the responses of employment, turnover, and earnings to childcare access are all stronger among single, younger, and lower-income women, consistent with these individuals facing greater constraints in private childcare provision. Consistent with our finding, Kimmel (1998), Cascio (2009), Goux and Maurin (2010) also document stronger employment response to childcare by single mothers.

We further find that earlier childcare access reduces the likelihood of working mothers taking sick leaves (column 1 of Table 8, Panel A). Bennedsen et al. (2019) show that sick leaves are often discretionary rather than health-induced and can thus capture employee effort and productivity; our result thus suggests childcare access increases productivity. Such an effect persists after we control for firm fixed effects (column 2), suggesting it is not driven by mothers switching to firms that have lower work intensity or stricter sick leave policies.[22]

[21] Our definition of "married" includes common-law partnership, which is common in Quebec.

[22] If anything, our earnings and sorting results suggest that the new job is likely to be more demanding, hence more likely to induce sickness.

We also examine whether better access to childcare leads new mothers to invest more in education. Columns 3-4 of Table 8, Panel A show that this is not the case. We fail to find a significant increase in the likelihood of taking a leave for schooling or further education. These results suggest that childcare-induced earnings growth is likely due to higher on-the-job productivity rather than further investment in human capital.

Finally, we examine how childcare subsidies impact mothers' subsequent fertility decisions after the first child. A lower childcare cost could encourage fertility. However, childcare access may also set new mothers on a more demanding career path, discouraging further fertility. We examine this in Panel B of Table 8, where the dependent variable is the total number of subsequent kids a mother had 5 years (or 10 years) after the birth of the first child. We estimate this regression using a Poisson pseudo maximum likelihood (PPML) estimator. We find that childcare subsidies discourage subsequent fertility in the short-run, but has no effect in the long-run. This suggests that new mothers delayed subsequent childbirths, likely due to their more demanding career path.

6 Worker-Firm Sorting and Firm-Level Impact

6.1 Sorting into Firms

In this section, we examine how access to childcare affects the sorting of new mothers into firms. Our goal is to understand what types of firms were affected by the reallocation of female labor supply induced by childcare subsidies, as indicated by the higher turnover and job-switching results discussed above.

Our results on turnover, earnings, and productivity suggest potential career upgrades by new mothers due to better childcare access. Becker (1985) argues that childcare responsibilities cause women to seek less demanding careers. Therefore, we should expect childcare access to lead new mothers to take on more ambitious or demanding careers that they previously lacked the time or flexibility to pursue. Motivated by this, we construct several measures of how "mom-friendly" a firm was before the reform, and check whether mothers with greater access to childcare were more likely to sort into "mom-unfriendly" firms.[23]

Our primary measure of "mom-friendliness", *High%Mom96*, is a dummy equal to one if a firm had above-industry-median percentage of mothers of young children (age<5) among all employees in 1996. We define this measure as of 1996 (immediately prior to the reform) to capture changes coming only from individuals switching employers, rather than changing characteristics of employers that may result from the reform itself. As a placebo, we similarly define *High%Dad96* to capture firms with above-industry-median percentage of fathers of young children in 1996. The *High%Momt96* measure intends to capture the barriers preventing mothers from pursuing employment at a particular firm due to childcare responsibilities. These barriers may arise due to, among other things, workplace culture, lack of family-friendly HR policies, time inflexibility, or the demanding nature of work at the firm.[24] Since we expect later cohorts of mothers to face less stringent childcare responsibilities due to longer access to government childcare, they should be able to overcome these barriers more easily. Therefore, we expect later cohorts to sort into low-%mom firms post-birth relative to earlier cohorts. Further, if these barriers are gender-specific, we should see limited sorting on *High%Dad96*.

Panel A of Table 9 tests sorting along this dimension. We find that earlier access to childcare increases new mothers' sorting into firms that are traditionally less appealing to new mothers (columns 1-2). Specifically, a five-year earlier access to childcare shifts new mothers towards a "mom-unfriendly" career by 2% relative to the mean. This effect is similar when we include industry-year fixed effects, suggesting that such sorting also happens within an industry-year across firms. In contrast, we do not see a similar sorting into "dad-unfriendly"

[23]By "mom-unfriendly", we do not mean that such firms have gender bias or are averse to hiring women. Rather, we use it to refer to an equilibrium outcome in which new mothers are less likely to be employed at such firms, due to either supply or demand side factors.

[24]Appendix Table A.6 shows that *Low%Mom96* firms tend to be smaller, more productive, more capital intensive, and similarly profitable compared with *High%Mom96* firms.

firms. This suggests that the childcare barriers mitigated by the reform are gender-specific. Figure 4a shows the dynamics for sorting away from mom-friendly firms and we find largely parallel trends across cohorts before child birth.

We also examine the convexity of pay relative to work hours, a job characteristic that previous research has shown to be unfriendly to new mothers (Goldin, 2014; Goldin and Katz, 2016). Jobs with high earnings-hours elasticity reward long, continuous hours, and tend to be "greedier" jobs with less time flexibility, such as lawyers and bankers. To construct this measure, we use earnings-hours elasticities from Goldin (2014), which are originally defined at the occupation level. Because we do not observe occupation in our data, we aggregate this measure to industry-level (NAICS 2-digit) using occupation-industry crosswalks and occupation weights within each industry. We then define *HighPayConvexity96*, a dummy indicating firms in 2-digit-NAICS with above-median pay convexity. Industries with the highest pay convexity include retail and finance/insurance, while industries with the lowest pay convexity include healthcare and agriculture. Column 1 of Table A.7 presents the sorting result along this dimension. We find that earlier access to childcare induces mothers to sort into industries with higher pay convexity. Specifically, a five-year-earlier access increases the chance new mothers work at high-pay-convexity firms post-birth by 1.4%.

Finally, we examine the sorting of new mothers into firms of different size categories. Prior literature shows that firm size is the strongest predictor of the provision of maternity benefits by firms (Liu et al., 2021), with smaller firms offering fewer non-wage benefits in general. Larger firms may also be able to offer more flexible work arrangements to its employees by having employees share job responsibilities, given prior research showing that job-sharing is more valuable for larger firms (Kotey and Koomson, 2021). We therefore examine *HighSales96*, a dummy indicating firms with above-industry-median sales in 1996, as a dependent variable in our benchmark specification. Columns 2-3 of Table A.7 show that, consistent with the sorting results above, earlier access to childcare motivates mothers to switch to smaller firms that tend to provide fewer maternity benefits and less flexible work arrangements. The effect is about 2% based on a five-year earlier access. This result also points towards potential substitution between public and private childcare provision.

To further understand the sorting results above, we decompose employer switches into switches into mom-friendly vs mom-unfriendly firms. We then examine how the effects differ by the mom-friendliness of the pre-birth employer. Panel B of Table 9 shows the results. We first find that childcare access increases job switching mainly among mothers employed at mom-friendly firms pre-birth, while those working at mom-unfriendly firms pre-birth have limited response (column 1). Further, neither group of mothers switched to mom-friendly firms in response to childcare access (column 2). Instead, switching largely happens from mom-friendly firms to mom-unfriendly firms (column 3). We obtain similar findings in Table A.8 when we split by prior-year employer type rather than pre-birth employer type. These results suggest that our sorting effect mainly reflects greater career upgrade (i.e., switching to less mom-friendly firms) rather than less career downgrade or greater retention of female workers at "mom-unfriendly" firms.

Overall, childcare subsidies appear to reallocate female labor supply from mom-friendly firms to mom-unfriendly firms, firms offering "greedier" jobs, or firms with fewer maternity benefits and less flexible work arrangements. Note that such sorting effects are equilibrium outcomes, and are not driven solely by mothers' labor supply choice. For example, childcare subsidies could increase labor demand for new mothers from firms with inflexible jobs. Regardless, these reallocations are likely to reduce gender employment gaps across firms and sectors. We next examine how such reallocation impacts mom-unfriendly firms by providing them access to a larger supply of female workers.

6.2 Firm-Level Results

Motivated by the individual-level sorting results, we examine the differential impact of the 1997 childcare reform on firms with different levels of mom-friendliness. We present evidence consistent with the notion that firms that were less attractive to mothers before the reform benefit more from the reduction of workplace gender barriers brought on by the reform.

Specifically, we employ a difference-in-differences model comparing Quebec firms with different levels of mom-friendliness before the reform, measured by the fraction of new mothers in the workforce in 1996 (the year before the reform). We estimate the following specification:

$$Y_{j,t} = \alpha_j + \beta_{k,t} + \theta \times Low\%Mom96_j \times Post97_t + \epsilon_{j,t}, \tag{5}$$

where $Y_{j,t}$ is an outcome for firm j in year t, α_j is firm fixed effect, $\beta_{k,t}$ is industry-year fixed effect, $Low\%Mom96_j$ is an indicator equal to one if firm j had below-industry-median fraction of moms of pre-kindergarten children in 1996, $Post97_t$ indicates years since 1997. Our main variable of interest is the interaction term $Low\%Mom96_j \times Post97_t$, which measures the differential impact of the reform on "mom-unfriendly" firms relative to "mom-friendly" firms. Note that $Low\%Mom96_j$ is absorbed by firm fixed effect and $Post97_t$ is absorbed by year fixed effect. Our sample period is 1994 to 2000. We cluster standard errors at the firm level.

We first use this specification to verify the sorting effect at the firm-level. If childcare access motivates mothers to switch from mom-friendly firms to mom-unfriendly firms, we should see the latter group experience an increase in female employment after the reform relative to the former group. Table A.9, Panel A confirms this result. We find that, post-reform, mom-unfriendly firms (with $Low\%Mom96_j = 1$) experienced a 1.3 pp increase in the fraction of female workers (column 1), which is a 3.3% increase relative to the mean. This increase is even bigger when we examine the fraction of young female workers with age<35 (column 2), which increased by 1.8pp and 7.2% relative to the mean. Columns 3 and 4 show that the increase in female share is driven by more female workers joining rather than fewer female workers leaving. Consistent with this, we find that the overall employment growth increased by 3.7% in mom-unfriendly firms relative to mom-friendly firms (column

5). Columns 6 and 7 confirm that this employment growth is driven mainly by growth in the number of female employees rather than the number of male employees.

Panel B of Table A.9 examine the impact on wages. we do not find a significant effect of the reform on either the average wage or female wage in mom-unfriendly firms relative to mom-friendly firms. If anything, average female wage decreased slightly by 0.36%. At the same time, there is a weak increase in average male wage by 0.7%, leading to a small 1.7pp increase in gender pay gap in mom-unfriendly firms. These results could reflect the downward pressure on female wages from the additional female labor supply to mom-unfriendly firms. However, such an interpretation can be confounded by compositional shifts in jobs: if the new female workers seek more ambitious positions, this could offset the downward pressure on wages from labor supply shock. Unfortunately, we could not disentangle these interpretations as we do not observe titles or positions in the data.

We then turn to the impact of the reform on firm performance. We employ three key performance measures: sales growth, ROA (pre-tax income/total assets), and labor productivity (i.e., sales per employee). Tabel 10 shows the results. We find that, simultaneous to the female labor inflow, mom-unfriendly firms experienced improved performance relative to mom-friendly firms after the refrom. Specifically, the reform led to a 4.2% increase in the sales growth of mom-unfriendly firms relative to mom-friendly firms (column 1). Mom-friendly firms also experienced a 3.3 pp increase in ROA (though statistically insignificant) relative to other firms, as well as a 2.3% increase in labor productivity (columns 2 and 3). Figure A.1 shows the dynamic effects of the reform on firms performance, confirming largely parallel trends between the two groups of firms before the reform. The outperformance of mom-unfriendly firms could suggest either that the newly hired female employees are more productive than existing employees, that mom-unfriendly firms were labor-constrained, or that workplace gender diversity itself increases the productivity of all workers. Regardless, our firm-level results together demonstrate that childcare subsidies reduced labor market segmentation across genders and benefited firms that were traditionally unattractive to mothers.

One may wonder why mom-unfriendly firms did not voluntary provide childcare before the reform if their performance could benefit. This is because firms have to pay for private childcare, whereas the universal childcare program is "free", i.e., paid by the government and not financed through payroll taxes. As such, firm-provided childcare may not necessarily benefit firms if taking into account the costs. Our results only suggest that government-provided childcare could benefit firms if paid by the government.

We caveat that, although our firm-level results are consistent with mothers' sorting into male-dominated firms, other channels may be at play too. For example, our results could be driven by changes in women's career choices before they even become a mother, due to changes in expectations. This is a margin we do not study in this paper. Hence, our firm-level results should not be interpreted as all being driven by new mothers' sorting.

7 Further Discussions

Did the Quebec universal childcare program generate net benefits? Unfortunately, we cannot fully answer this question within the scope of our paper. This is because the program likely generated other general equilibrium or welfare effects that are hard to quantify. For example, universal childcare may change the signaling value of employment or taking short leaves. The program may also change social norms. In terms of welfare, some studies have documented negative effect of childcare subsidies on parental well-being and child development outcomes (Baker et al., 2019; Brodeur and Connolly, 2013; Kottelenberg and Lehrer, 2017). Finally, all the above effects need to be weighed against the opportunity cost of the subsidies.

That said, we attempt one quantification of the Quebec childcare program from a pure fiscal perspective. Specifically, we conduct a back-of-the-envelope calculation of the net fiscal value of the program. We obtain fiscal costs from realized annual program spending in Lefebvre and Merrigan (2008). The fiscal benefits come from the additional tax revenue from higher personal income and higher corporate income. Table A.10 provides details on the calculations. From 1997 to 2002, the average program annual cost is \$593m/year. The Quebec government gained additional \$353.5m tax revenue per year, while the Quebec and federal governments together gained additional \$854.4m tax revenue per year. As such, assuming Quebec did not use federal funding to pay for the program, the program lost \$239m/year from Quebec's perspective. However, at the national level, the program gained \$261m/year on net. Nevertheless, we emphasize that the program had many non-fiscal or non-pecuniary costs or benefits that we cannot quantify.

8 Conclusion

Much of the attempt to reduce gender gaps in the labor market has been focused on childcare and family policies. However, governments around the world differ greatly in the amount of childcare support they provide, partly driven by hesitancy on the merits of these subsidies (Kleven et al., 2019). This paper advances this debate by studying the effect of childcare subsidies on women's career progression and firm outcomes, using linked Canadian tax filing data. Exploiting a universal childcare reform in Quebec in 1997 and variation in its timing relative to childbirth across cohorts of parents, we show that earlier access to childcare increases employment among new mothers. Departing from the previous literature, our paper further shows that childcare subsidies lead to greater reallocation of their human capital towards more demanding and male-dominated careers. This results in higher earnings and productivity for new mothers. Such a reallocation also reduces gender-based segmentation across firms, benefiting firms that are traditionally unattractive to mothers with young children. These results suggest that childcare frictions not only reduce women's labor supply, but also constrain the types of firms women are willing to work at. Removing frictions in childcare can therefore advance women's careers and help narrow gender gaps across firms and sectors.

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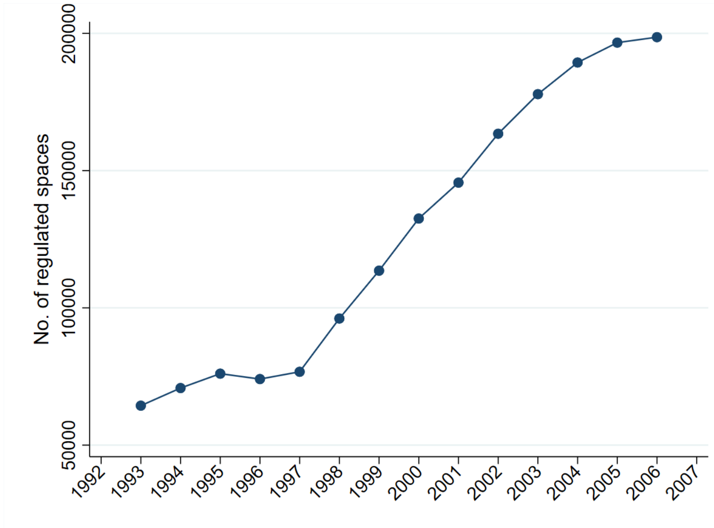
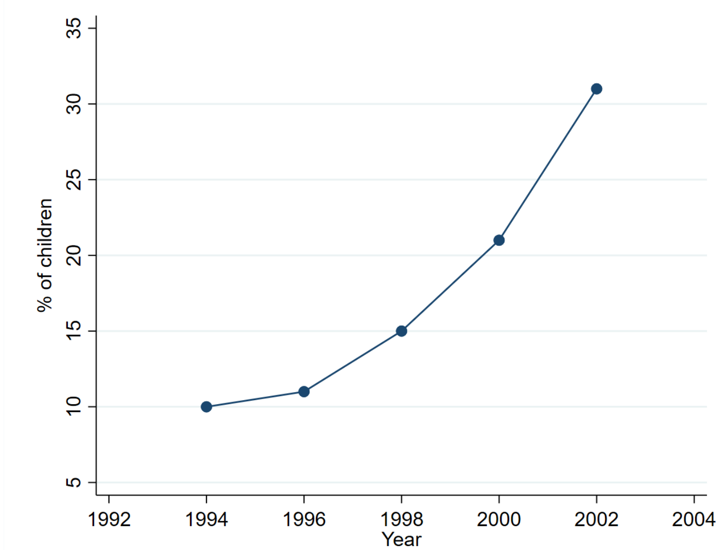
Figure 1: Childcare Provision and Usage in Quebec Around 1997



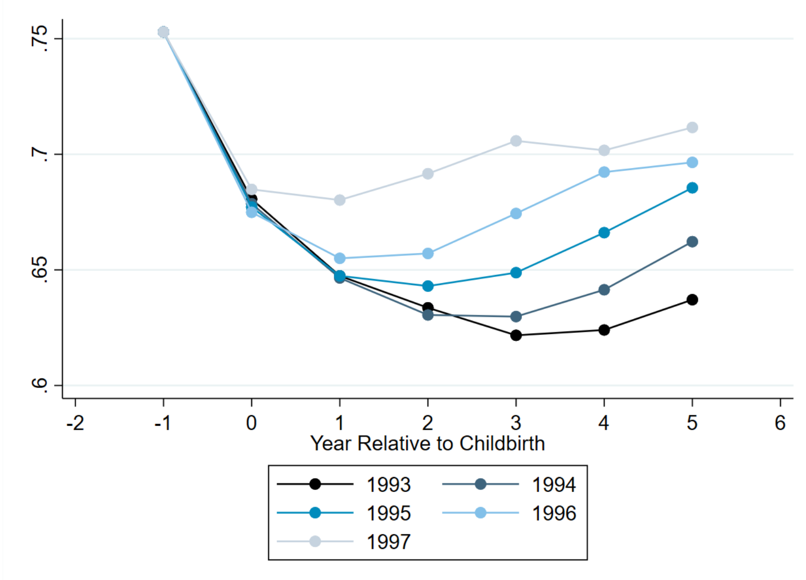
(a) Number of regulated childcare spaces



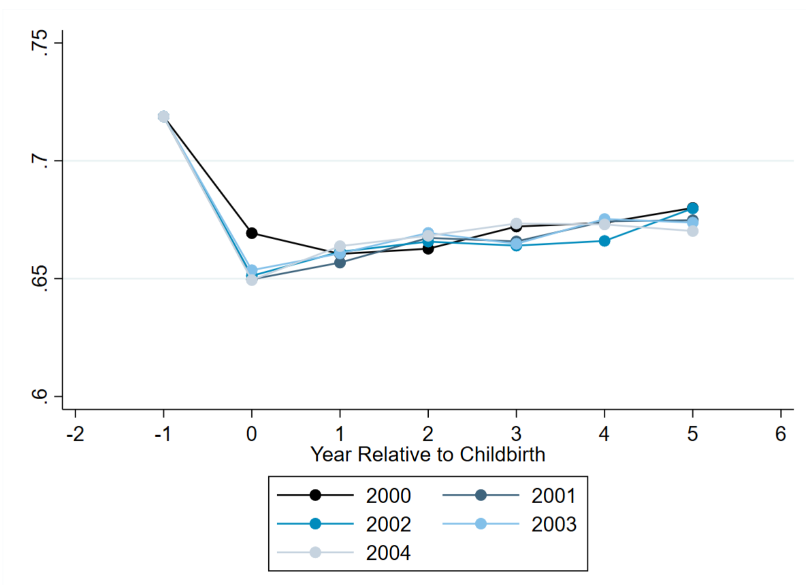
(b) Percentage of 1-5-year-olds in childcare centers

Figure (a) shows the number of regulated childcare spaces in Quebec from 1993 to 2006 based on data in Table 2 of Lefebvre and Merrigan (2008). Figure (b), based on Table 3 of Lefebvre and Merrigan (2008), shows the percentage of children of age 1-5 whose primary care arrangement is childcare center in Quebec. The data come from biennial National Longitudinal Survey of Children and Youth (NLSCY).

Figure 2: Mean Employment Rate by Cohort Around Childbirth



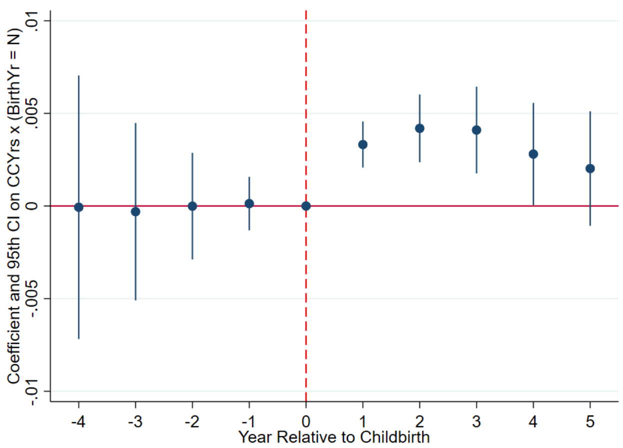
(a) Cohorts in our sample



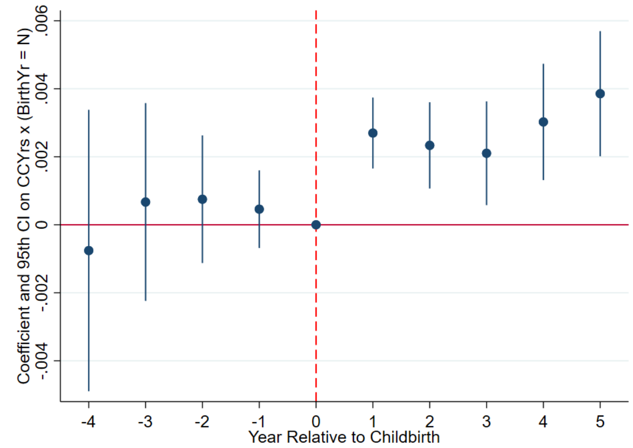
(b) Placebo cohorts

This figure shows the adjusted mean employment rate for different cohorts of mothers over a window of -1 to 5 years relative to childbirth. Panel A shows the 1993-1997 cohorts who had different exposures to the reform. Panel B shows the 2000-2004 placebo cohorts whose children were always eligible for the subsidy. Darker colors represent earlier cohorts. In each graph, the cohorts are shifted to align with the pre-childbirth employment rate of the earliest cohort.

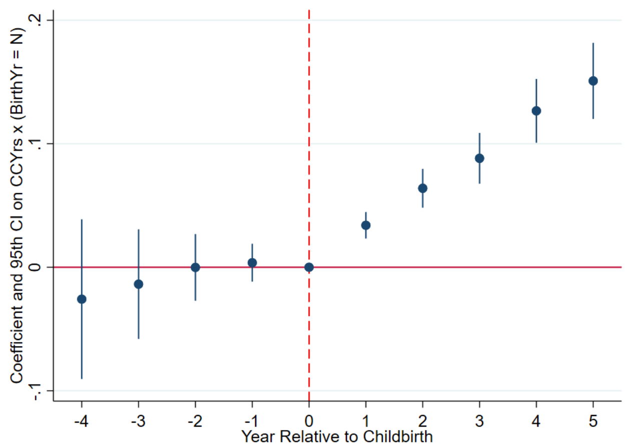
Figure 3: Dynamics Treatment Effects: Employment, Turnover, and Earnings



(a) Employment



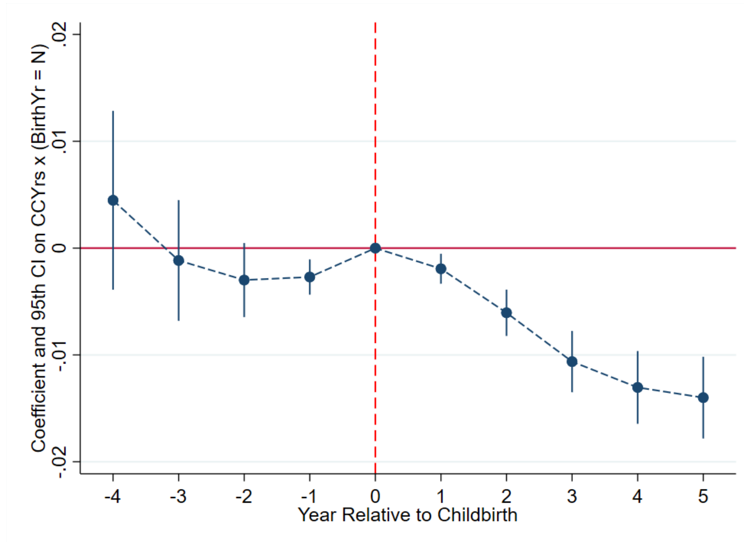
(b) Quit



(c) Earnings

These figures show the dynamic DID effects estimated from equation 2, where the childbirth year is omitted as the base year. Each dot (bar) represents the point estimate (95th confidence interval) of the coefficient on $CCYears_i \times YearToBirth_{i,n}$ in equation 2. *Employment* is an indicator equal to one if the individual is employed. *Quit* is an indicator equal to one if the individual voluntarily leaves the employer from the previous year. *Earnings* is total T4 earnings scaled by earnings in the year prior to childbirth.

Figure 4: Dynamics Treatment Effects: Sorting



(a) High%Mom96

This figure shows the dynamic DID effects estimated from Equation 2, where the childbirth year is omitted as the base year. Each dot (bar) represents the point estimate (95th confidence interval) of the coefficient on $CCYears_i \times YearToBirth_{i,n}$ in Equation 2. *High%Mom96* is a dummy equal to one if the individual's current employer had above-industry-median percentage of moms of pre-kindergarten children among all employees in 1996, the year before the reform.

Table 1: Treatment Intensity by Cohort

Child age		Calendar year											*CCYears*
		1993	1994	1995	1996	1997	1998	1999	2000	2001	2002	2003	
Childbirth year	1993	0	1	2	3	4							1
	1994		0	1	2	3	4						2
	1995			0	1	2	3	4					3
	1996				0	1	2	3	4				4
	1997					0	1	2	3	4			5
	1998						0	1	2	3	4		5
	1999							0	1	2	3	4	5

This table shows the intensity of treatment received by each cohort of parents. The rows indicate cohorts by childbirth year and the columns indicate calendar years. The numbers in the shaded cells indicates the age of the child for a cohort of parents in that calendar year. Grey cells indicate the years post childbirth before the reform. Light blue cells indicate the anticipatory years when parents knew about the program but before their kids were age-eligible for the subsidized rate. In those years parents could enroll their child into childcare early to claim a spot, albeit at the unsubsidized rate; they also benefit from the increased supply of childcare spaces. Darker blue cells indicate the eligible years when the child was actually eligible for subsidized rate. The total number of blue cells for each cohort corresponds to the value of $CCYears$, i.e., the number of years each cohort of parents had access to government childcare.

Table 2: Summary Statistics (*Pending Dislcosure*)

Variable	N	Mean	P5	P50	P95	Std. Dev.
				Individual-level:		
Employed (T3, C1)						
Pre-birth employed (T3, C1)						
Quits (T4, C1)						
Switch employer (T4, C4)						
Raw earnings (T5, C1)						
Earnings (scaled) (T5, C1)						
Promotion (T5, C3)						
Demotion (T5, C6)						
Married (T3, C1)						
Age at childbirth (T3, C1)						
Raw pre-birth earnings (T3, C1)						
Sick leave (T8, Panel A, C1)						
School leave (T8, Panel A, C3)						
Num_kids_5yrs (T8, Panel B, C1)						
Num_kids_10yrs (T8, Panel B, C2)						
High%Mom96 (T9, Panel A, C1)						
High%Dad96 (T9, Panel A, C3)						
%Mom96 (T9, Panel A, C1)						
%Dad96 (T9, Panel A, C3)						
Switch2High%Mom96 (T9, Panel B, C2)						
Switch2Low%Mom96 (T9, Panel B, C3)						
				Firm-level:		
Low%Mom96 (T10, C1)						
Ln(sales growth) (T10, C1)						
ROA (T10, C2)						
Ln(sales/emp) (T10, C3)						

This table presents the summary statistics for our individual-level and firm-level samples. All variables are defined in the main text.

Table 3: Employment Effect

	(1)	(2) Employed	(3)
CCYears × PostBirth	0.0035***	0.0046***	0.0011
	[0.0006]	[0.0013]	[0.0007]
Pre-birth status	All	Unemployed	Employed
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Observations	2,731,040	723,150	2,007,890
Ad. R-squared	0.527	0.304	0.320

This table examines the effect of childcare access on female's employment status. The specification is based on Equation 1. *Employment* is an indicator equal to one if the individual is employed in that year. Column 1 examines all female individuals in our sample, and columns 2 and 3 split by individuals' employment status in the year prior to childbirth. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 4: Turnover Effect

	(1)	(2)	(3)	(4)	(5)	(6)
		Quit			Switch Employer	
CCYears × PostBirth	0.0023***	0.0022***	0.0021***	0.0021**	0.0023***	0.0016*
	[0.0005]	[0.0004]	[0.0005]	[0.0009]	[0.0009]	[0.0008]
Individual FE	X	X	X	X	X	X
Year FE	X	X	X	X	X	X
Event year FE	X	X	X	X	X	X
Industry FE		X			X	
Firm FE			X			X
Observations	1,692,000	1,692,000	1,654,540	1,692,000	1,692,000	1,654,540
Ad. R-squared	0.062	0.073	0.136	0.142	0.145	0.192

This table examines the effect of childcare access on female's likelihood of job turnover. The specification is based on Equation 1. *Quits* (columns 1-3) is an indicator equal to one if the individual voluntarily leaves the employer from the previous year as identified from record of employment (ROE). *Switch Employer* (columns 4-6) is an indicator equal to one if the individual is with a different employer this year compared with last year as identified from T4 filing. All columns condition on employed individual-years. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 5: Effect on Earnings

	(1) Earnings	(2) Earnings	(3) Promotion	(4) Promotion	(5) Promotion_moved	(6) Demotion	(7) Demotion	(8) Demotion_moved
CCYears × PostBirth	0.0432***	0.0150***	0.0163***	0.0138***	0.0157***	-0.0106***	-0.0075***	-0.0028***
	[0.0054]	[0.0039]	[0.0008]	[0.0009]	[0.0006]	[0.0009]	[0.0010]	[0.0007]
Individual FE	X		X		X	X		X
Year FE	X	X	X	X	X	X	X	X
Event year FE	X	X	X	X	X	X	X	X
Individual-Firm FE		X		X			X	
Observations	1,686,920	1,433,320	1,692,000	1,437,530	1,692,000	1,692,000	1,437,530	1,692,000
Ad. R-squared	0.603	0.781	0.454	0.430	0.410	0.425	0.351	0.234

This table examines the effect of childcare access on female's earnings relative to their pre-childbirth earnings. The specification is based on Equation 1. *Earnings* is total T4 earnings divided by the individual's earnings in the year before childbirth. *Promotion* is an indicator equal to one if the individual's current earnings is more than 110% of her pre-childbirth earnings. *Promotion_moved* is an indicator equal to one if the individual is promoted and has moved to a different employer relative to the pre-childbirth employer. *Demotion* is an indicator equal to one if the individual current earnings is less than 90% of her pre-childbirth earnings. *Demotion_moved* is an indicator equal to one if the individual is demoted and has moved to a different employer relative to the pre-childbirth employer. All columns condition on individuals employed in the year before childbirth. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 6: Triple-Difference: Female vs Male

	(1)	(2)	(3)	(4)	(5)	(6)
	Employed		Quit		Earnings	
CCYears × PostBirth	0.0003		-0.0004		0.0108*	
	[0.0006]		[0.0005]		[0.0057]	
CCYears × Female × PostBirth	0.0032***	0.0000	0.0027***	0.0042***	0.0325***	0.0273***
	[0.0008]	[0.0004]	[0.0006]	[0.0009]	[0.0079]	[0.0082]
Individual FE	X	X	X	X	X	X
Year x Sex FE	X	X	X	X	X	X
Event year-Sex FE	X	X	X	X	X	X
Cohort-event year FE		X		X		X
Family-year FE		X		X		X
Observations	5,459,330	1,809,780	3,564,880	1,537,320	3,554,900	1,533,480
Ad. R-squared	0.552	0.819	0.059	0.143	0.624	0.647

This table reports the triple-difference results comparing male and female for our three main outcomes. The specification is based on Equations 3 in columns 1, 3, 5, and is based on Equation 4 in columns 2, 4, 6. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 7: Heterogeneity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Employed			Quits			Earnings	
CCYears × PostBirth	0.0170***	0.0136***	0.0126***	0.0036***	0.0040***	0.0042***	0.1628***	0.1047***	0.1051***
	[0.0007]	[0.0008]	[0.0008]	[0.0006]	[0.0006]	[0.0006]	[0.0088]	[0.0084]	[0.0080]
Married × PostBirth	-0.1578***			-0.0035***			-1.0655***		
	[0.0018]			[0.0012]			[0.0251]		
CCYears × Married × PostBirth	-0.0237***			-0.0023***			-0.1411***		
	[0.0008]			[0.0005]			[0.0094]		
Older × PostBirth		-0.0844***			-0.0014			-1.2789***	
		[0.0018]			[0.0010]			[0.0211]	
CCYears × Older × PostBirth		-0.0196***			-0.0030***			-0.1162***	
		[0.0008]			[0.0005]			[0.0088]	
HighEarn × PostBirth			0.0076***			0.0050***			-1.6078***
			[0.0019]			[0.0010]			[0.0185]
CCYears × HighEarn × PostBirth			-0.0260***			-0.0035***			-0.1611***
			[0.0009]			[0.0005]			[0.0079]
Individual FE	X	X	X	X	X	X	X	X	X
Year FE	X	X	X	X	X	X	X	X	X
Event year FE	X	X	X	X	X	X	X	X	X
Observations	2,731,040	2,731,040	2,007,890	1,692,000	1,692,000	1,692,000	1,686,920	1,686,920	1,686,920
Ad. R-squared	0.532	0.529	0.322	0.062	0.062	0.062	0.599	0.601	0.604

This table examines the cross-sectional heterogeneity in our baseline results for employment, turnover, and earnings. $Married$ indicates that the individual was married in the year before childbirth. *Older* indicates that the individual had an above-median age in the year before child birth. $HighEarn$ indicates that the individual had above-median earnings in the year before childbirth. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 8: Alternative Outcomes

Panel A: Absenteeism and further education

	(1)	(2)	(3)	(4)
	Sick leave		School leave	
CCYears × PostBirth	-0.0010***	-0.0011***	0.0003	0.0002
	[0.0002]	[0.0002]	[0.0002]	[0.0002]
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Industry FE				X
Firm FE		X		
Observations	1,692,000	1,692,000	1,686,920	1,686,920
Ad. R-squared	0.025	0.025	0.079	0.090

Panel B: Fertility

	(1)	(2)	(3)	(4)
	Num_kids_5yrs	Num_kids_10yrs	Num_kids_5yrs	Num_kids_10yrs
CCYears × PostBirth	-0.0098***	-0.0035***	-0.0068***	0.0004
	[0.0012]	[0.0011]	[0.0012]	[0.0011]
Age bin FEs			X	X
Observations	283,850	283,850	283,850	283,850

This table examines the effect childcare access on other individual-level outcomes. In Panel A, the specification follows Equation 1. *Sick leave* is an indicator equal to one if the individual took a temporary sick leave in a year. *School leave* is an indicator equal to one if the individual took a leave to pursue schooling or further education. Panel B estimates a Poisson pseudo maximum likelihood (PPML) regression on a cross-section of mothers of first child. The outcome *Num_kids_5yrs* (*Num_kids_10yrs*) is the total number of subsequent children a mother had 5 years (10 years) after the birth of the first child. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table 9: Sorting into Firms

Panel A: Firm type in 1996

	(1)	(2)	(3)	(4)
	High%Mom96		High%Dad96	
CCYears × PostBirth	-0.0016**	-0.0019***	-0.0008	0.0001
	[0.0007]	[0.0007]	[0.0008]	[0.0007]
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Industry-Year FE		X		X
Observations	1,388,510	1,388,410	1,388,510	1,388,410
Ad. R-squared	0.624	0.652	0.644	0.691

Panel B: Decomposing turnover

	(1)	(2)	(3)
	Switch employer	Switch2High%Mom96	Switch2Low%Mom96
CCYears × PostBirth	-0.0015	-0.0002	-0.0003
	[0.0014]	[0.0011]	[0.0009]
High%MomPB × PostBirth	-0.0089*	-0.1088***	0.0917***
	[0.0053]	[0.0037]	[0.0037]
High%MomPB × CCYears × PostBirth	0.0049***	0.0002	0.0065***
	[0.0013]	[0.001]	[0.0008]
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Observations	1,454,430	1,454,430	1,454,430
Ad. R-squared	0.145	0.086	0.067

This table examines how childcare access affects the type of firms women choose to work for. The specification follows Equation 1 and the sample conditions on employed individual-years. In Panel A, the dependent variable *High%Mom96* (*High%Dad96*) is a dummy equal to one if a firm had above-industry-median percentage of moms (dads) of pre-kindergarten children among all employees in 1996, the year before the reform. In Panel B, we decompose the dependent variable *Switch employer* into switching to an employer that was *High%Mom96* (*High%Dad96*) in 1996. On the right hand side, we interact with *High%MomPB*, an indicator for whether the pre-birth employer had above- or below-industry-median percentage of moms of pre-kindergarten children among all employees in the pre-birth year. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

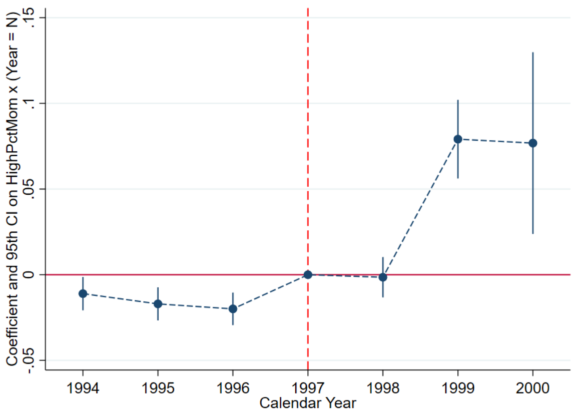
Table 10: Impact on Firm Performance

	(1) Ln(sales growth)	(2) ROA	(3) Ln(sales/emp)
Low%Mom96 × Post97	0.0381***	0.0330	0.0230**
	[0.0053]	[0.0266]	[0.0099]
Firm FE	X	X	X
Industry-Year FE	X	X	X
Observations	314,420	257,720	325,880
Ad. R-squared	0.146	0.142	0.719

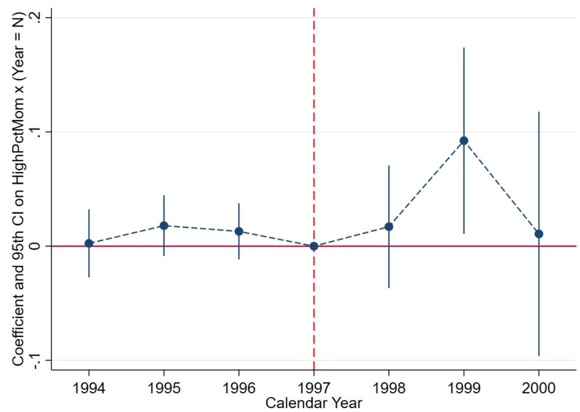
This table examines the impact of the reform on Quebec firms' outcomes, in particular employment outcomes (Panel A) and financial performance (Panel B). The sample period is 1994 to 2000. The specification follows Equation 5. All columns include firm fixed effect and industry-year fixed effects. *Low%Mom96* is an indicator equal to one if a firm had above-industry-median percentage of moms of pre-kindergarten children (age<5) among all employees in 1996. *%Female* is the percent of female employees. *%YoungFemale* is the percent of female employees below age 35 among all employees. *Ln(emp gr)* is log employment growth. *Ln(female emp gr)* (*Ln(male emp gr)*) is log growth of the number of female (male) employees. *Ln(sales growth)* is log sales growth. *ROA* is pre-tax income divided by total assets. *Ln(sales/emp)* is log labor productivity, i.e., log of sales divided by employment. Standard errors are reported in brackets and are clustered at the firm level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Appendix

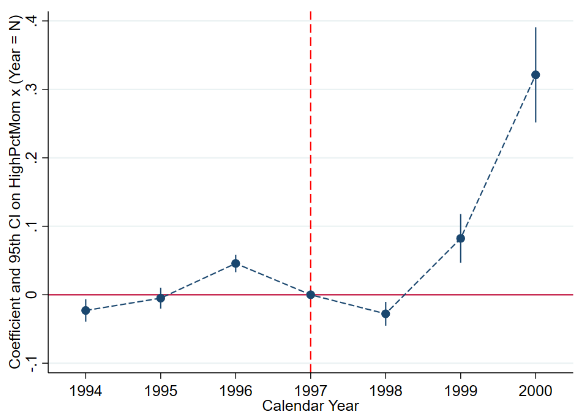
Figure A.1: Firm-Level Impact: Dynamics



(a) Ln(sales growth)



(b) ROA



(c) Ln(sales/emp)

These figures show the firm-level dynamic DID effects estimated from the equation below, where 1997 is the omitted base year:

$$Y_{j,t} = \alpha_j + \beta_{k,t} + \sum_{t=1993}^{2001} \theta_t \times Low\%Mom96_j \times Year_t + \epsilon_{j,t},$$

Each dot (bar) represents the point estimate (95th confidence interval) for the coefficient on $Low\%Mom96_j \times Year_t$. The three panels correspond to the dynamics for sales growth, ROA, and labor productivity, respectively. Standard errors are clustered by firm.

Table A.1: Effect of the Reform on Childcare Take-up

	In day care	
Post97 years	0.0491***	
	[0.0089]	
Anticipated years		0.0309***
		[0.0096]
Eligible years		0.0946***
		[0.0198]
Age FE	X	X
Observations	5,520	5,520
Ad. R-squared	0.033	0.035

This table shows the effect the reform in childcare take-up using the public version of the National Longitudinal Survey of Children and Youth (NLSCY). The sample consists of children of age 0-4 in the 94-95, the 96-97, and the 98-99 survey cycles. Children are de-identified and are not linked across cycles. The dependent variable is a dummy equal to one if the child is in daycare at the time of the survey. *Post97 years* indicate years after 97. *Anticipatory years* indicates years post reform but before the child was age-eligible for the subsidy (i.e., the light blue cells in Table 1). *Eligible years* indicates years the child was age-eligible for the subsidy (i.e., the dark blue cells in Table 1). All columns include age fixed effects. Robust standard errors are reported in brackets. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.2: Individual-level Robustness: Alternative Definitions

Panel A: Alternative definitions of turnover and earnings

	(1)	(2)	(3)	(4)
	Leave pre-birth employer		Earnings growth	
CCYears × PostBirth	0.0140***	0.0141***	0.0610***	0.0632***
	[0.0008]	[0.0007]	[0.0047]	[0.0049]
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Firm FE		X		X
Observations	1,692,000	1,654,540	1,544,960	1,510,220
Ad. R-squared	0.577	0.702	0.033	0.068

Panel B: Alternative cutoffs of promotions and demotions

	(1)	(2)	(3)	(4)
	Promotion_20%	Promotion_30%	Demotion_20%	Demotion_30%
CCYears × PostBirth	0.0111***	0.0074***	-0.0032**	-0.0039**
	(0.0012)	(0.0011)	(0.0015)	(0.0015)
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Observations	956050	956050	956050	956050
Ad. R-squared	0.458	0.487	0.313	0.291

Panel A shows the robustness of our baseline individual-level results to alternative definitions of turnover and earnings. *Leave pre-birth employer* is an indicator equal to one if the individual's current employer is different from her employer in the year before childbirth. *Earnings growth* is the year-to-year growth rate of an individual's earnings. Panel B restricts to cohorts 1993-1997 to rule out concerns of pregnancy or birth timing in response to the reform. Panel B shows the robustness of our promotion and demotion results to alternative cutoffs for large earnings changes. *Promotion_20%* and *Demotion_20%* are based on > 20% earnings change and *Promotion_30%* and *Demotion_30%* are based on > 30% earnings change.

Table A.3: Individual-level Robustness: Additional Fixed Effects

Panel A: Age bin fixed effects

	(1) Employed	(2) Quit	(3) Earnings	(4) High%Mom96
CCYears × PostBirth	0.0031***	0.0023***	0.0381***	-0.0016**
	[0.0006]	[0.0005]	[0.0053]	[0.0007]
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Age bin FE	X	X	X	X
Observations	2,731,040	1,692,000	1,686,920	1,384,300
Ad. R-squared	0.532	0.062	0.603	0.624

Panel B: Pre-birth characteristics interacted with event year fixed effects

	(1) Employed	(2) Quit	(3) Earnings	(4) High%Mom96
CCYears × PostBirth	0.0030***	0.0023***	0.0315***	-0.0017**
	[0.0006]	[0.0005]	[0.0049]	[0.0007]
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Pre-birth char. x event year FE	X	X	X	X
Observations	2,731,040	1,692,000	1,686,920	1,384,300
Ad. R-squared	0.542	0.063	0.683	0.624

This table shows the robustness of our baseline individual-level results to additional fixed effects. Panel A includes fixed effects for parents' age bins in units of 5. Panel B includes the interactions of individuals' pre-birth characteristics with event year fixed effects. Pre-birth characteristics include a dummy for being married, a dummy for age>30, and the log of earnings, all measured in the year before childbirth. For those unemployed before childbirth, earnings is set to zero.

Table A.4: Individual-level Robustness: 1993-1997 Cohorts Only

	(1) Employed	(2) Quit	(3) Earnings
CCYears × PostBirth	0.0030***	0.0025***	0.0211***
	[0.0008]	[0.0007]	[0.0078]
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Observations	1,666,010	1,031,370	1,028,820
Ad. R-squared	0.533	0.063	0.594

This table shows the robustness of our individual-level results to restricting to 1993-1997 cohorts to rule out concerns of pregnancy or birth timing in response to the reform. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.5: Individual-level Robustness: Dropping Part-Time or Public Sector Employees

Panel A: Drop part-time employees

	(1) Employed	(2) Quit	(3) Earnings	(4) High%Mom96
CCYears × PostBirth	0.0022***	0.0020***	0.0129***	-0.0016**
	(0.0007)	(0.0005)	(0.0020)	(0.0007)
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Observations	1,793,820	1,562,140	1,562,140	1,296,700
Ad. R-squared	0.3	0.055	0.418	0.644

Panel B: Drop public sector employees

	(1) Employed	(2) Quit	(3) Earnings	(4) High%Mom96
CCYears × PostBirth	0.0040***	0.0022***	0.0428***	-0.0017**
	(0.0006)	(0.0005)	(0.0057)	(0.0008)
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Observations	2,727,020	1,593,610	1,590,630	1,296,230
Ad. R-squared	0.562	0.061	0.597	0.618

This table shows the robustness of our individual-level results to dropping part-time employees (Panel A) or dropping public sectors employees (Panel B). * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.6: Correlations between *Low%Mom96* and Firm Characteristics

	(1) Low%Mom96
Ln(assets)	-0.2087*
ROA	0.0063
Ln(sales/emp)	0.1442*
Ln(capital/emp)	0.0665*

This table examines firm-level correlations between the $Low\%Mom96$ measure used in Table 9 and various firm characteristics in 1996. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.7: Sorting into Firms – Alternative Firm Type Measures

	(1)	(2)	(3)
	HighPayConvexity96	HighSales96	
CCYears × PostBirth	0.0014**	-0.0018**	-0.0019**
	[0.0007]	[0.0009]	[0.0009]
Individual FE	X	X	X
Year FE	X	X	X
Event year FE	X	X	X
Industry-Year FE			X
Observations	1,498,990	1,097,980	1,097,980
Ad. R-squared	0.659	0.640	0.671

This table examines how childcare access affects new mothers' sorting into alternative measures of firm types. The specification follows Equation 1 and the sample conditions on employed individual-years. *HighPayConvexity96* is a dummy equal to one if a firm was in a 2-digit NAICS industry with above-median pay convexity, i.e., the elasticity of annual earnings to weekly hours (Goldin (2016)) in 1996. The measure is aggregated from occupation-level estimates from Goldin (2016) using occupation-industry crosswalk, based on occupation weights within each industry. *HighSales96* is a dummy equal to one if a firm had above-industry-median sales in 1996. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.8: Sorting into Firms – Decomposing Employer Switches

	(1) High2Low%Mom	(2) Low2High%Mom	(3) High2High%Mom	(4) Low2Low%Mom
CCYrs x PostBirth	0.0009**	-0.0009	0.0001	0.0026***
	[0.0004]	[0.0006]	[0.0005]	[0.0004]
Individual FE	X	X	X	X
Year FE	X	X	X	X
Event year FE	X	X	X	X
Observations	1,566,060	1,566,060	1,566,060	1,566,060
Adjusted R-squared	0.001	0.019	0.080	0.074

This table examines how childcare access affects new mothers reallocation across firm types. The specification follows Equation 1 and the sample conditions on employed individual-years. *High2Low%Mom* is a dummy equal to one if a mother was working at a high%mom-firm in the previous year and switches to a low%mom-firm this year, where high%mom means the firm had above(below)-industry-median percentage of moms of pre-kindergarten children among all employees in that year. *Low2High%Mom*, *High2High%Mom*, and *Low2Low%Mom* are defined analogously. Standard errors are reported in brackets and are clustered at the individual level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.9: Impact on Firm Employment Outcomes

Panel A: Employment

	(1) %Female	(2) %YoungFemale	(3) %YoungFemaleLeavers	(4) %YoungFemaleJoiners	(5) Ln(emp gr)	(6) Ln(female emp gr)	(7) Ln(male emp gr)
Low%Mom96	0.0130***	0.0180***	-0.0027	0.0297***	0.0365***	0.0475***	0.0071*
× Post97	(0.0012)	(0.0012)	(0.0025)	(0.0024)	(0.0055)	(0.0040)	(0.0041)
Firm FE	X	X	X	X	X	X	X
Industry-Year FE	X	X	X	X	X	X	X
Observations	334490	334490	263650	277470	315300	270350	295840
Ad. R-squared	0.887	0.820	0.500	0.511	0.198	0.050	0.024

Panel B: Wages

	(1) Ln(avg wage)	(2) Ln(avg wage_female)	(3) Ln(avg wage_male)	(4) GenderPayGap
Low%Mom96	-0.0023	-0.0036	0.0070*	0.0174***
× Post97	(0.0030)	(0.0038)	(0.0039)	(0.0041)
Firm FE	X	X	X	X
Industry x Year FE	X	X	X	X
Observations	401300	340310	372260	311250
Adjusted R-squared	0.789	0.721	0.761	0.576

This table examines the impact of the reform on Quebec firms' employment (Panel A) and wages (Panel B). The sample period is 1994 to 2000. The specification follows Equation 5. *Low%Mom96* is an indicator equal to one if a firm had above-industry-median percentage of moms of pre-kindergarten children (age<5) among all employees in 1996. *%Female* is the percent of female employees. *%YoungFemale* is the percent of female employees below age 35 among all employees. *%YoungFemaleLeavers* (*%YoungFemaleJoiners*) is the percent of female employees below age 35 leaving (joining) the firm in a year relative to all employees in the previous year. *Ln(emp gr)* is log employment growth. *Ln(female emp gr)* (*Ln(male emp gr)*) is log growth of the number of female (male) employees. *Ln(avg wage)* (*Ln(avg wage_female)*) (*Ln(avg wage_male)*) is the logarithm of the average earnings across all (female) (male) employees. *GenderPayGap* is (*Avg wage_male* − *Avg wage_female*)/*Avg wage*. Standard errors are reported in brackets and are clustered at the firm level. * indicates statistical significance at the 10% level, ** at the 5% level, and *** at the 1% level.

Table A.10: Fiscal Analysis of the Quebec Universal Childcare Program

	Quebec	Quebec + Federal
Effective tax rate	14.4%	32.3%
Marginal tax rate	15.4%	35.4%
Δ employment	5,925	5,925
Base earnings ($)	25,749	25,749
Base employment	338,595	338,595
Δ earnings ($)	5,562	5,562
Δ personal income tax revenue ($m)	312	716
Δ corporate income ($)	3600	3600
Marginal tax rate	9%	30%
No. of QC firms	128,197	128,197
Δ corporate income tax revenue ($m)	41.5	138.5
Δ total tax revenue ($m)	353.5	854.4
Program cost/year ($m)	593	593
Net benefits/year ($m)	-239.5	261.4

This table provides a back-of-the-envelope calculation of the net fiscal payoff of the program. All numbers are annual average over the period of 1997 to 2002. The fiscal costs come from realized program spending from Table 1 of Lefebvre and Merrigan (2008). The fiscal benefits come from the additional tax revenue from higher personal income and higher corporate income. Column 1 shows the fiscal benefits from additional provincial tax revenue. Column 2 shows the fiscal benefits from additional provincial plus federal tax revenue. We estimate Δ *personal income tax* = Δ *no. of women employed* × *base earnings* × *effective tax rate* + *base no. of women employed* × Δ *earnings* × *marginal tax rate*. We estimate Δ *corporate income tax* = *marginal tax rate* ×Δ *pre-tax income* × *no. of Quebec firms*, where Δ *pre-tax income* is estimated from a difference-in-differences analysis comparing Quebec and non-Quebec firms before and after the reform.